7-2011

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Citation

LU, Hai; Hai LU; and GUAN, Yuyan. Conflict of interest reforms and analysts' research biases. (2011). *Journal of Accounting, Auditing and Finance*. 27, (4), 443-470.
Available at: https://ink.library.smu.edu.sg/soa_research/1598

Published in Journal of Accounting, Auditing and Finance, 2011 July, Volume 27, Issue 4, Pages 443-470
http://doi.org/10.1177/0148558X11409159

Conflict-of-Interest Reforms and Investment Bank Analysts' Research Biases

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Abstract

This study examines the consequences of the series of reforms targeting investment banking–related conflicts of interest. The authors compare and contrast optimism biases in analysts' stock recommendations and earnings forecasts across different types of analyst firms in the postreform period of 2004 to 2007 versus the prereform period of 1998 to 2001. The authors document a significant reduction in the relative optimism of sanctioned investment bank analysts' stock recommendations but not in their earnings forecasts. Moreover, the authors find little change in the profitability of their stock recommendations but detect a drop in the accuracy of earnings forecasts made by investment bank analysts. In sum, the reforms achieve the objective of mitigating the apparent optimism in investment bank stock recommendations, but they do not provide benefit to investors in terms of more profitable recommendations or more accurate earnings forecasts.

Keywords

equity analyst, conflicts of interest, securities regulations, investment banks

Biased analyst research is believed to have contributed to large investor losses during the stock market downturn in 2000 and 2001. Investors, the business press, and regulators have long suspected that investment bank analysts bias their research in return for investment banking business from the companies they follow (see, for example, Becker, 2001; Morgenson, 2001). 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 (SEC) imposed enforcement actions against 10 of the largest U.S. investment banks (the so-called ''Global 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 operation of their research and investment banking departments.

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Concerns have been voiced regarding the effectiveness of these reforms. The incentive related to investment-banking businesses is only one of the several types of incentives that could potentially cause analyst research biases. Specifically, the incentive to generate trading commissions, which is not addressed by the reforms, will continue to drive analyst research optimism (e.g., Cowen, Groysberg, & Healy, 2006; Irvine, 2004; Jackson, 2005). This is of particular concern for investment bank research, as its research funding source is shifted from underwriting to trading because of the reforms. Hence, it is an empirical question whether the conflict-of-interest reforms have achieved the goal of improving the objectivity of investment research.

In this article, we examine the impact of the reforms on investment bank analysts' research biases. If the reforms resolve investment banking–related conflicts of interest, we expect to find a reduction in investment bank analysts' research biases in the postreform period, ceteris paribus. We conduct our tests on analysts from different type of securities firms (research firms, brokerage firms, syndicate banks, and investment banks), and we also separate investment banks into nonsanctioned and sanctioned banks to examine the incremental effect of the Global Settlement.[1] We examine the change in analysts' research biases between the prereform period (January 1998-December 2001) and postreform period (January 2004-December 2007).

We document two key results. First, we find a significant reduction in the relative optimism of sanctioned bank stock recommendations but no change in the relative optimism of their earnings forecasts.[2] Second, we show that sanctioned bank analysts become significantly less optimistic than research firm analysts in the postreform period. These findings are consistent with the reforms reducing the optimism of stock recommendations issued by analysts from sanctioned investment banks. This eases the concern that the reforms might induce other biases to investment bank research as their incentive has shifted from gaining investment banking businesses to generating trading commissions.

However, there are reasons to believe that the reforms have unintended consequences on the quality of research. First, Mehran and Stulz (2007) argue that if investment bank analysts provide better research as a consequence of the conflicts, the consumers of this research will benefit. Moreover, these conflicts impose deadweight costs on investment banks because their customers take the conflicts into account, and hence, investment banks already have incentives to reduce these conflicts and the associated costs. Any regulation might simply replace these deadweight costs with regulatory costs. Second, without funding provided by investment banking businesses, research departments might have to reduce their coverage or the quality of their research (e.g., Boni, 2006; Boni & Womack, 2002; O'Leary, 2007), and elite analysts might leave sell-side research to pursue other lucrative opportunities (e.g., Groysberg, Healy, & Chapman, 2008; Mattlin, 2007; Pizzani, 2009; Guan, Lu, & Wong, 2009). Third, participation of equity analysts in investment banking deals helps analysts become more familiar with the companies and their industries (e.g., Mattlin, 2007; Jacob, Rock, & Weber, 2008; Mehran & Stulz, 2007; Pizzani, 2009). Hence, separating research from investment banking activities may reduce the quality of investment research.

Consistent with these arguments, we document that sanctioned bank buy recommendations become less profitable, whereas the profitability of sell/hold recommendations improves insignificantly. Moreover, the accuracy of investment bank forecasts drops. These results are consistent with the reforms providing little incremental benefit to investors in terms of more profitable recommendations or more accurate forecasts (Kim, 2009; Mehran & Stulz, 2007).[3] These findings also call into question the efficacy of a Global Settlement

requirement that sanctioned banks furnish third-party independent research to their retail clients. This is because research firms are more optimistic in their earnings forecasts and recommendations in the postreform period. Moreover, the accuracy of their forecasts and the profitability of their recommendations are not significantly different from those of investment banks after the reforms.[4]

This study adds to the strand of literature examining investment banking–related conflicts of interest as the cause of the research biases in various prereform periods (see, for example, Agrawal & Chen, 2008; Cowen et al., 2006; Jacob, Rock, & Weber, 2008; Ljungqvist, Marston, Starks, Wei, & Yan, 2007). In general, these studies find mixed evidence that investment banks issued more optimistic forecasts or recommendations than noninvestment banks (see Mehran & Stulz, 2007, for a summary). We use the reforms as a unique setting to shed further light on this issue. In particular, if investment bank analysts were biased because of conflicts of interest, their banks would take actions to alleviate the biases in response to the reforms. The larger the incentive problem in the prereform period, the bigger will be the reduction in analysts' optimistic biases as a result of the mitigating actions taken by the banks. We document that the reduction in the relative recommendation optimism of sanctioned bank analysts is larger than that of their research firm counterparts, which is consistent with sanctioned investment bank analysts being optimistically biased in the prereform period and reacting to the reforms swiftly as a result.

This study also adds to Barber, Lehavy, McNichols, and Trueman (2006) and Kadan, Madureira, Wang, and Zach (2009), which investigate the effect of the reforms on the properties of stock recommendations using a postreform period ending in June 2003 and December 2004, respectively. Our tests supplement these two studies by examining the long-term effect of the reforms using a longer postreform period: from January 2004 through December 2007. Moreover, as discussed below in the section titled ''Recommendation Optimism,'' our research design is different from the research designs of these two studies and, hence, provides triangulating evidence on the economic consequences of the reforms.

The rest of the article is organized as follows. Section titled ''Reforms on Analyst Conflicts of Interest'' summarizes the series of reforms. Section titled ''Sample and Data'' describes the sample and data, whereas section titled ''Model Specification, Variable Definitions, and Descriptive Statistics'' explains our research design. Sections titled ''Empirical Findings on Stock Recommendations'' and ''Empirical Findings on Earnings Forecasts'' present the empirical results on stock recommendations and earnings forecasts, respectively, and the final section ''Concluding Remarks'' provides concluding remarks.

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. As a result, the financial industry, self-regulatory organizations (SROs), and regulators introduced proposals or 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 the CFA Institute) formed a task force on analyst independence and released a white paper titled ''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, the 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 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)[5] filed the first round of proposed SRO rules: amendments to NYSE Rule 351 (reporting requirements) and Rule 472 (communications with the public) and the new NASD Rule 2711 (Research Analysts and Research Reports). The U.S. 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 determining 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 their rating systems 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 10 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 industry and bolster confidence in the integrity of equity research. (SEC December 20, 2002 press release, 2002-179)

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 firms set the budgets of the research departments 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 is prohibited. Investment bankers do not take part in evaluating analysts' job performance or determining their compensation. Research management makes 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 5-year period. Last but not least, these banks must publicly disclose their research

analysts' historical ratings and price-target forecasts to assist investors in evaluating the performance of analysts.

Sample and Data

Our sample of analysts comes from Thomson Financial's Institutional Brokers' Estimate System (I/B/E/S) database and covers the period from January 1998 to December 2007. We divide the sample into three subperiods: the prereform period (January 1998-December 2001), the transition period (January 2002-December 2003), and the postreform period (January 2004-December 2007). We examine the change in analysts' research biases between the pre- and postreform periods. We exclude the transition period from the analysis because it is the period when the reforms were proposed, deliberated, and implemented. As the regulatory environment underwent continual changes during the transition period, including this period, our analysis could potentially have induced ''background noise'' in estimating the permanent effect of the reforms on analysts' research biases.

We retrieve all analyst earnings forecasts and stock recommendations from the I/B/E/S database in 2008. Ljungqvist, Malloy, and Marston (2009) indicate that all post-2006 I/B/E/S stock recommendation data are free from the errors they identified in their study. We use the 2006 I/B/E/S translation file to identify the affiliation and name of each equity analyst, which allows us to have a sample spanning the period from January 1998 through December 2007.[6] Stock price and return data are from Center for Research in Security Prices and financial statement data are from Compustat.

An analyst is considered to be subject to investment banking conflict of interest if he or she is working for an investment bank. Following Clarke et al. (2004) and Cowen et al. (2006), we classify securities firms into four types based on information from Nelson's Directory of Investment Research (2000-2007) and Securities Data Company (SDC) database.[7] First, investment banks are those listed as investment banks by Nelson's and identified as lead or colead underwriters by SDC. We further divide the investment banks into sanctioned and nonsanctioned 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. Second, syndicate banks are those firms listed by Nelson's as either investment banks or brokers and identified by SDC as managers or comanagers but not as lead or colead underwriters.[8] Third, research firms are those listed as such by Nelson's and not found in the SDC database. Fourth, brokerage firms are those firms classified by Nelson's as major institutional brokers, major or small regional brokers, or investment banks/brokers that are not identified as lead/colead underwriters or managers/comanagers by SDC.[9]

The final sample consists of those analysts who make both stock recommendations and earnings forecasts. The sample is further subject to two additional restrictions to facilitate the calculation of the analyst-specific relative research bias measures. First, we compute these measures using only company-year observations that are followed by at least three analysts. Second, we calculate these measures using companies that are covered by at least one research firm analyst and one investment bank analyst in a particular forecasting period. The latter restriction is done to ensure a fair comparison of the research biases of research firm analysts with those of their investment bank counterparts. In particular, it rules out the possibility that difference in coverage is driving the difference in the research biases of these two types of analysts (we further control for the difference in the portfolio

of companies covered by analysts in subsequent regression analysis). Although this restriction reduces the number of company-years used in the computation of these measures, the results (not tabulated) remain qualitatively unchanged if we do not impose this restriction.[10]

Table 1 reports the number of analysts in the sample, the number of securities firms represented by these analysts, and the number of companies included in the computation of analyst-specific relative bias measures. Consistent with prior studies, Panel A shows that the majority of the analysts in our sample come from investment banks (both sanctioned and nonsanctioned banks). The number of analysts increases from the prereform period (1998-2001) to the postreform period (2004-2007) across all five types of securities firms. This might be attributed to the fact that more analysts are making stock recommendations and earnings forecasts in the later part of the sample period. Panel B reports the number of securities firms represented by our sample of analysts. The number of securities firms also goes up in the postreform period, especially the number of research firms, which could be due to the funding for independent research provided by the Global Settlement. Panel C reports the number of companies used in the computation of analyst-specific relative bias measures. To be included in the sample, a company must be followed by at least three analysts, including one research firm analyst and one investment bank analyst. The number of companies increases sharply from the prereform period to the postreform period, which is likely due to the increase in the number of research firm analysts in the postreform period; hence, more companies meet the sample inclusion restrictions.

Model Specification, Variable Definitions, and Descriptive Statistics

We use the difference-in-differences (DD) method to investigate the impact of the reforms on the biases of analysts' stock recommendations and earnings forecasts. In the DD analysis, we compare the change in the biases of research firm analysts to those of analysts from different types of securities firms (brokerage firms, syndicate banks, nonsanctioned, and sanctioned investment banks). The DD method explicitly controls for time-specific variations that are common across the groups but not attributed to the reforms per se (i.e., confounding effects). We also control for other sources of variations in research biases across analysts and sample period in the DD regression model, which is specified as follows:

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

where *DEP* is a measure of analyst *i*'s research biases (to be defined in sections titled Empirical Findings on Stock Recommendations'' and ''Empirical Findings on Earnings Forecasts''). *D* is an indicator variable that equals one in the postreform period and zero in the prereform period. *BROKERAGE*, *SYNDICATE*, *NONSANC*, and *SANCTIONED* are indicator variables that equal one, respectively, if analyst *i* is employed by a brokerage firm, syndicate firm, nonsanctioned investment bank, and a sanctioned investment bank, and zero otherwise.

The estimated coefficients α_2 to α_5 represent the prereform research biases of brokerage, syndicate, nonsanctioned investment, and sanctioned investment firm analysts, respectively,

Table 1. Statistics on the Numbers of Equity Analysts, Securities Firms, and Companies Being Followed, by Firm Type and Year

	Research firms	Brokerage firms	Syndicate firms	Nonsanctioned investment banks	Sanctioned investment banks
Panel A: Number of analysts					
1998	35	26	59	366	162
1999	40	62	136	768	401
2000	51	33	99	571	261
2001	28	21	69	398	247
2002	10	7	29	179	118
2003	39	50	148	795	490
2004	186	35	232	1,104	609
2005	201	62	207	1,076	563
2006	195	98	188	1,084	615
2007	107	95	150	911	601
Panel B: Number of securities firms					
1998	9	9	25	70	10
1999	11	15	35	80	10
2000	12	14	35	75	10
2001	9	9	23	69	10
2002	8	4	14	47	10
2003	13	13	36	80	10
2004	40	21	40	88	10
2005	46	21	39	88	10
2006	47	30	40	84	10
2007	27	38	38	82	10
Panel C: Number of companies followed					
1998	208	30	80	177	148
1999	351	131	183	321	274
2000	241	45	98	223	153
2001	139	31	68	117	113
2002	70	11	24	62	49
2003	435	115	240	414	378
2004	1,094	111	518	1,023	828
2005	993	162	412	947	683
2006	1,097	267	384	1,023	762
2007	803	191	261	746	683

Note: The sample covers the period from January 1998 to December 2007. The pre- and postreform periods cover January 1998 to December 2001 and January 2004 to December 2007, respectively. Stock recommendations and analyst earnings forecasts are from Thomson Financial's Institutional Brokers' Estimate System database. To be included in the sample and used in the calculation of analyst-specific relative bias measures, a company must be followed by at least three analysts and by one research firm analyst and one investment bank analyst. Investment banks are those listed as investment banks by Nelson's Directory of Investment Research and identified as lead or colead underwriters by SDC database. Investment banks are further divided into sanctioned and nonsanctioned 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 Nelson's as either investment banks or brokers and identified by SDC as managers or comanagers but not lead or colead underwriters. Research firms are those listed as such by Nelson's and not found in the SDC database. The rest of the firms are classified as brokerage firms, if they are not identified as lead/colead underwriter or manager/comanager by SDC.

relative to that of research firm analysts. The estimated coefficients α_6 to α_9 are the DD estimates, indicating the changes in the biases of analysts from brokerage firms, syndicate firms, and nonsanctioned and sanctioned investment banks, respectively, relative to the change in the bias of research firm analysts.

We estimate the DD regression model using the ordinary least squares method on a panel of analysts. Hence, we include year-dummy variables to control for unobserved time effects, and we cluster by analyst to absorb unobserved analyst effects. Petersen (2009) shows that if the time effect is fixed, this approach will produce unbiased standard errors. Given the short time series, we are not able to cluster on both year and analyst or to formally model the time dependence.

As the unit of analysis is analyst-year, we further control for variations across analysts and over time in the DD regression. In particular, we control for the characteristics of the analysts, the brokerage firms in which they work, and the portfolio of companies they covered. We rely on prior studies (e.g., Bradshaw, Richardson, & Sloan, 2006; Hong & Kubik, 2003; Jacob, Lys, & Neale, 1999; Mikhail, Walther, & Willis, 1999) to identify the set of variables that have shown to be associated with analyst optimism and accuracy. We discuss these variables next.

Analyst characteristics are captured by analyst experience, number of companies followed, analyst industry specialization, analyst turnover indicator, and percentage of new followings. Analyst experience is the average number of years the analyst has issued earnings forecasts or recommendations for the companies they follow. Number of companies followed is the number of companies for which the analyst provides earnings forecasts in a corresponding calendar year. Analyst specialization is the average percentage of companies followed by the analyst with the same two-digit Standard Industrial Classification code as each company being followed. The denominator is the total number of firms followed by the analyst in the sample period 1998 to 2007. Analyst turnover is an indicator variable that equals one in the year when the analyst left the brokerage house where she worked last year; otherwise, it equals to zero. Percentage of new following is the percentage of companies that the analyst covers in the current year that are not being covered in the previous year.

Brokerage firm characteristics are captured by brokerage firms' size rank and specialization. Brokerage firm size rank is the percentile ranking of the total number of analysts employed by the brokerage house to which the analyst belongs, relative to other brokerage houses. Brokerage specialization is the percentage of the analyst's brokerage house analysts who follow company *j*'s industry.

Portfolio characteristics are captured by the average company size, leverage, gross margin, sales growth, book-to-market ratio, and amount of external financing of the portfolio of companies being followed by the analyst. Company size is the logarithm of the market value of equity. Leverage is the debt-to-equity ratio. Gross margin is equal to one minus the cost of goods sold scaled by total sales. Sales growth is the growth in total net sales. Book-to-market is the book-to-market ratio. The amount of external financing is the net amount of cash flow received from external financing activities scaled by average total assets.

Finally, we also control for forecast horizon and lagged relative earnings forecast accuracy in the regressions. Forecast horizon is the average number of days between the forecast date and the forecast period end date for the portfolio of companies followed by an analyst. Following Hong and Kubik (2003), among others, we calculate relative forecast

accuracy, $Accuracy_{it}$, by averaging $Accuracy_{ijt}$ across all companies followed by analyst i in calendar year t. In particular,

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

where $Rank_{ijt}$ is analyst i's forecast accuracy rank for company j in fiscal year t, and $NumberFollowing_{jt}$ is the number of analysts following company j in fiscal year t. We use the last forecast made by each analyst for the same company and forecast period (FY1—the current fiscal year). By construction, this measure controls for difference in the composition of companies followed by the analysts.

Table 2 presents statistics on the control variables that we include in the DD regressions. The statistics indicate that research firm analysts have less experience than their counterparts at investment banks and, on average, follow fewer companies and have more new following than investment bank analysts. However, investment bank analysts have higher industry specialization and lower job turnover than other analysts. They also tend to follow companies that are larger, more leveraged, and more profitable than those followed by research firm analysts. Besides the cross-sectional variations, these characteristics also vary across the two subperiods. The variations across analysts and over time could potentially affect the relative change in analysts' research bias over the pre- and postreform periods, and, hence, we control for these sources of variations in the DD regressions.

Empirical Findings on Stock Recommendations

Recommendation Optimism

We first create two measures to capture the relative ranking of analysts' stock recommendations. For each company j followed by analyst i in fiscal year 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 ($LessPOS_{ijt}$) and that are less favorable ($LessNEG_{ijt}$). A high (low) $LessPOS_{ijt}$ indicates that analyst i's recommendation of company j is relatively less (more) positive and a high (low) $LessNEG_{ijt}$ means that analyst i's recommendation is relatively less (more) negative, relative to the recommendations of other analysts for the same company. As a stock recommendation could be favorable, unfavorable, or identical when compared with other recommendations, we need both $LessPOS_{ijt}$ and $LessNEG_{ijt}$ to capture the relative optimism of a recommendation.[11] These two rankings are computed for all companies that are followed by at least three analysts. We average $LessPOS_{ijt}$ and $LessNEG_{ijt}$ across all companies followed by analyst i in calendar year t to obtain the average relative rankings, $LessPOS_{it}$ and $LessNEG_{it}$, of analyst i in calendar year t.

We then define a relative recommendation optimism measure, $RROPT_{it}$, as the difference between $LessNEG_{it}$ and $LessPOS_{it}$. $RROPT_{it}$ is a parsimonious way to combine the information in $LessNEG_{it}$ and $LessPOS_{it}$. A positive (negative) $RROPT_{it}$ indicates that analyst i is more optimistic (pessimistic) in her stock recommendations than other analysts who follow the same companies as analyst i. The construction of $RROPT_{it}$ follows the same logic of the relative forecast optimism metric of Clement (1999) and others in that it accounts for difference in the portfolio of companies followed by different analysts and for time effect.

Table 2. Analyst, Firm, and Company Characteristics by Securities Firm Type

	Research firms	Brokerage firms	Syndicate firms	Nonsanctioned banks	Sanctioned banks
Panel A: Prereform period					
Number of analyst-year observations	95	116	285	1,922	1,011
Analyst experience (years)	5.07	7.03	5.86	6.85	7.19
Number of companies following	12.19	11.94	14.12	14.84	15.72
Analyst industry specialization	0.40	0.53	0.48	0.54	0.58
Analyst turnover	0.31	0.21	0.24	0.19	0.16
Percentage of new following	0.52	0.38	0.44	0.38	0.34
Brokerage firm size rank	46.38	67.05	61.78	85.54	95.96
Brokerage firm specialization	0.49	0.28	0.38	0.25	0.19
Average company size (log)	6.11	7.99	7.42	7.30	7.76
Average leverage	0.47	0.57	0.51	0.51	0.55
Average gross margin	0.17	0.24	0.40	0.28	0.28
Average sales growth	1.43	1.21	1.46	1.51	1.48
Average book-to-market	0.48	0.43	0.44	0.47	0.47
External Financing	0.06	0.03	0.07	0.07	0.05
Forecast horizon (days)	256	271	262	272	278
Panel B: Postreform period					
Number of analyst-year observations	547	245	706	3,924	2,170
Analyst experience (years)	5.44	6.68	6.77	7.11	6.90
Number of companies following	11.44	11.69	12.25	14.91	14.77
Analyst industry specialization	0.52	0.54	0.57	0.58	0.59
Analyst turnover	0.22	0.25	0.22	0.17	0.12
Percentage of new following	0.41	0.35	0.35	0.32	0.32
Brokerage firm size rank	61.18	60.04	72.13	86.64	97.08
Brokerage firm specialization	0.37	0.42	0.35	0.20	0.11
Average company size (log)	7.23	7.79	7.54	7.51	8.22
Average leverage	0.49	0.51	0.50	0.51	0.55
Average gross margin	0.24	0.33	0.20	0.17	0.24
Average sales growth	1.25	1.20	1.26	1.70	2.00
Average book-to-market	0.41	0.43	0.41	0.42	0.42
External financing	0.02	−0.01	0.02	0.02	0.00
Forecast horizon (days)	273	280	284	288	288

Note: The table presents the average value of analyst, firm, and company characteristics in the pre- and postreform periods, covering January 1998 to December 2001 and January 2004 to December 2007, respectively. See Table 1 for the classification of securities firm type. All variables are defined in the section titled "Model Specification, Variable Definitions, and Descriptive Statistics."

Table 3, Panel A presents the levels and changes in relative stock recommendation optimism, *RROPT*, by subperiod and analyst affiliation. In both the pre- and postreform periods, investment bank analysts are relatively less optimistic than research firms. For example, the average *RROPT* for sanctioned bank analysts is −0.071 in the postreform period, compared with 0.059 for research firm analysts. However, columns 3 and 4 show that analysts from syndicate firms and nonsanctioned banks exhibit a significant increase in *RROPT* after the reforms. However, the increases are not significantly different from that

Table 3. Descriptive Statistics on Relative Stock Recommendation Optimism by Firm Type and Sample Period (N = 11,021)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample period	Research firms	Brokerage firms	Syndicate firms	Nonsanctioned banks	Sanctioned banks	(2) − (1)	(3) − (1)	(4) − (1)	(5) − (1)
Panel A: *RROPT*									
Preperiod	0.030	−0.022	−0.023	0.002	−0.015	−0.052	−0.052	−0.027	−0.044
Postperiod	0.059	0.042	0.047	0.039	−0.071	−0.017	−0.012	−0.020	−0.130***
Change	0.029	0.064	0.070**	0.037***	−0.056***	0.035	0.041	0.007	−0.085**
Panel B: *LessPOS*									
Preperiod	0.216	0.256	0.264	0.248	0.253	0.040	0.048**	0.032*	0.037**
Postperiod	0.215	0.230	0.231	0.224	0.280	0.014	0.016	0.009	0.065***
Change	−0.001	−0.026	−0.033**	−0.024***	0.027***	−0.026	−0.034	−0.023	0.028
Panel C: *LessNEG*									
Preperiod	0.245	0.234	0.242	0.251	0.238	−0.011	−0.003	0.005	−0.007
Postperiod	0.274	0.272	0.278	0.264	0.210	−0.002	0.004	−0.010	−0.064***
Change	0.029	0.038	0.036**	0.013**	−0.028***	0.009	0.007	−0.016	−0.057**

Note: The table reports results on relative stock recommendation optimism of the analysts from different securities firm type. The pre- and postreform periods cover January 1998 to December 2001 and January 2004 to December 2007, respectively. *RROPT* is relative recommendation optimism for each analyst, computed as *LessNEG* minus *LessPOS*. *LessPOS* (*LessNEG*), stands for less positive (less negative), is the percentage of other analysts' recommendations for the same company in the same period that are more (less) favorable than the analyst's recommendation. Stock recommendation data are from Thomson Financial's Institutional Brokers' Estimate System database. See Table 1 or the text for the classification of securities firm type.

*, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t test.

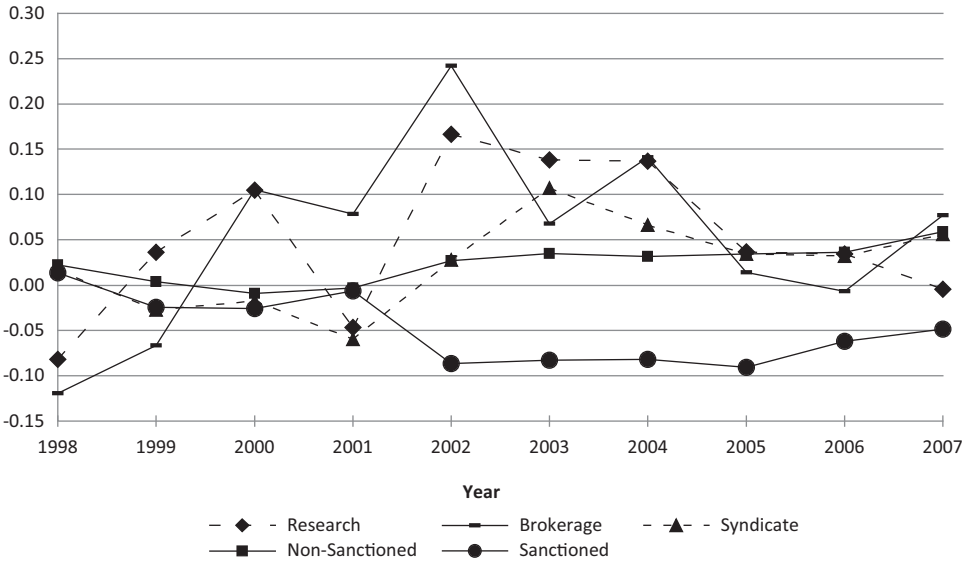

Figure 1. Relative stock recommendation optimism for equity analysts from five types of securities firms, 1998-2007

Note: Relative recommendation optimism, *RROPT*, is computed as *LessNEG* minus *LessPOS* for each analyst. *LessPOS* (*LessNEG*), stands for less positive (less negative), is the percentage of other analysts' recommendations for the same company in the same period that are more (less) favorable than the analyst's recommendation. The graph plots the annual averages of *RROPT* for analysts from five different types of securities firms: research firms, brokerage firms, syndicate banks, nonsanctioned investment banks, and sanctioned investment banks.

of research firm analysts, as shown in columns 7 and 8. In contrast, sanctioned bank analysts become less optimistic after the reform (*RROPT* decreases by 0.056) and column 9 indicates that the drop is significantly different from that of research firm analysts.

To better understand the drop in the relative recommendation optimism of sanctioned banks, we examine its two components, *LessPOS* and *LessNEG*. Column 5 in Table 3 shows that sanctioned banks experienced a significant increase in *LessPOS* (Panel B) and a significant decrease in *LessNEG* (Panel C) after the reforms. In other words, the percentage of other analysts who are more favorable than the sanctioned bank analysts increases, whereas the percentage of other analysts who are less favorable decreases. Taken together, this leads to a decrease in the relative recommendation optimism of sanctioned bank analysts. Finally, column 9 indicates that sanctioned bank analysts exhibit a change in *LessNEG* that is significantly more negative than that of research firm analysts.

To shed further light on the trend of the change in relative recommendation optimism, we plot the annual *RROPT* for all five firm types in Figure 1. The figure shows that the *RROPT* of sanctioned bank analysts starts to decrease in 2002 and stays below the level exhibited in the prereform period. The *RROPT* of analysts from other firm types either increases (nonsanctioned bank) or fluctuates (research, brokerage, and syndicate firms) during the sample period, and it is always higher than that of sanctioned bank analysts since 2002. Figure 1 suggests that the reforms have a permanent mitigating effect on the relative recommendation optimism of sanctioned bank analysts.

Table 4. Difference-in-Differences Regressions of Relative Recommendation Optimism and its Components (N = 11,021)

	RROPT		*LessPOS*		*LessNEG*	
	Coefficient	*t* statistics	Coefficient	*t* statistics	Coefficient	*t* statistics
Intercept	22.61	3.70***	13.39	4.10***	36.00	10.38***
D	2.83	0.70	0.04	0.02	2.87	1.22
BROKERAGE	−4.21	−0.79	3.11	1.07	−1.09	−0.36
SYNDICATE	−3.97	−0.92	3.87	1.72*	−0.11	−0.04
NONSANC	−0.40	−0.11	3.32	1.19	1.92	0.85
SANCTIONED	−1.16	−0.29	2.53	1.23	1.37	0.58
D × *BROKERAGE*	2.91	0.49	−2.08	−0.64	0.83	0.25
D × *SYNDICATE*	3.77	0.80	−2.79	−1.14	0.98	0.36
D × *NONSANC*	−0.33	−0.08	−1.53	−0.76	−1.86	−0.81
D × *SANCTIONED*	−9.77	−2.44**	3.49	1.68*	−6.28	−2.68***
Relative accuracy	0.05	2.11**	−0.04	−2.71***	0.02	1.15
Forecast horizon	−0.03	−2.86***	0.01	2.34**	−0.01	−2.81***
Experience	0.09	1.19	−0.01	−0.32	0.08	1.82*
Number of companies following	−0.08	−1.65*	0.01	0.53	−0.07	−2.46**
Industry specialization	1.34	0.94	−0.33	−0.42	1.02	1.28
Analyst turnover	0.20	0.18	0.37	0.61	0.57	0.89
Percentage of new following	−3.32	−1.95*	1.45	1.53	−1.87	−2.00**
Brokerage firm size rank	−0.11	−3.07***	0.03	1.43	−0.08	−4.11***
Brokerage firm specialization	−9.27	−3.38***	3.73	2.47**	−5.54	−3.60***
Company size	−0.60	−1.70*	0.92	4.79**	0.32	1.62
Leverage	−1.95	−0.69	−4.47	−2.93***	−6.43	−4.00***
Gross margin	−0.001	−0.29	0.001	0.89	0.0002	0.06
Sales growth	−0.01	−0.25	−0.01	−0.54	−0.02	−1.04
Book-to-market	0.03	1.68*	−1.90	−1.91*	1.15	1.16
External Financing	−0.06	−0.02	0.99	0.52	0.93	0.46
R^2	.020		.020		.019	

Note: This table reports the ordinary least squares estimation results of the following regression:

$$DEP_{it} = \alpha_0 + \alpha_1 D + \alpha_2 BROKERAGE + \alpha_3 SYNDICATE + \alpha_4 NONSANC + \alpha_5 SANCTIONED + \alpha_6 D \times BROKERAGE + \alpha_7 D \times SYNDICATE + \alpha_8 D \times NONSANC + \alpha_9 D \times SANCTIONED + CONTROLS + e_{t,}$$

where the dependent variable, *DEP*, is *RROPT*, *LessPOS*, or *LessNEG*. *RROPT* is relative recommendation optimism for each analyst, computed as *LessNEG* minus *LessPOS*. *LessPOS* (*LessNEG*) stands for less positive (less negative) and is the percentage of other analysts' recommendations for the same company in the same period that are more (less) favorable than the analyst's recommendation. The dependent variables are multiplied by 100. *D* is an indicator variable that equals to one in the postreform period (January 2004 to December 2007) and zero in the prereform period (January 1998 to December 2001). *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 nonsanctioned 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. *CONTROLS* represent a set of control variables for analyst, firm, and portfolio characteristics, which are defined in the section titled "Model Specification, Variable Definitions, and Descriptive Statistics." Year-fixed effects are also included. Robust standard errors are clustered by analyst.

*, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided *t* test.

Next, we turn to a multivariate analysis of the effect of the reform on analysts' relative recommendation optimism. We use a DD regression model to control for other sources of variations that could affect the relative recommendation optimism of analysts from different firm types.

Table 4 summarizes the DD of *RROPT* and its two components. In the *RROPT* regression, the estimated coefficients on *BROKERAGE*, *SYNDICATE*, *NONSANC*, and *SANCTIONED* are not distinguishable from zero, indicating that there is no difference in the level of relative recommendation optimism between research firms and other firm types before the reforms. However, the estimated coefficient on $D \times SANCTIONED$ (i.e., the DD estimate) is significantly negative at the 5% level ($t = -2.44$), indicating that the reforms have a significant negative effect on the optimism of stock recommendations made by sanctioned bank analysts. The DD estimates for other analysts are indistinguishable from zero, and, hence, there is no evidence that analyst from other firm types are changing their recommendation optimism in response to the reforms.

Table 4 also indicates that several control variables exhibit the expected association with *RROPT*. Specifically, analysts who followed more firms and those who followed more new firms are less optimistic in their recommendations, as are analysts from large securities firms and from firms with specific industry expertise. Analysts who cover large companies and high-growth companies (i.e., low book-to-market ratio) are also less optimistic in their stock recommendations.

As for the two components of *RROPT*, the last two sets of columns in Table 4 show that the estimated coefficient on $D \times SANCTIONED$ is significantly positive in the *LessPOS* regression and significantly negative in the *LessNEG* regression. In other words, sanctioned bank analysts not only issue less favorable recommendations after the reforms but also more unfavorable recommendations than other analysts who follow the same companies. Taken together, these two findings explain why sanctioned bank analysts became relatively less optimistic after the reforms. Finally, similar to what we document in the *RROPT* regression, none of the other DD estimates (i.e., the coefficients on the interaction terms) are statistically different from zero, suggesting that analysts from other securities firms are no less positive or negative in their stock recommendations than those from research firms.

Using a shorter postreform period, two related studies have tested the impact of the reforms on the properties of investment bank recommendations.[12] Barber et al. (2006) find that (a) the percentage of buy recommendations issued by sanctioned banks is, on average, only slightly higher than that issued by nonsanctioned banks before NASD 2711 became effective and (b) sanctioned banks exhibit a much bigger drop in the percentage of buys than nonsanctioned banks in the 10-month period after NASD 2711. Our test differs from theirs in three respects. First, we extend their analysis by documenting the fact that after the reforms, sanctioned bank analysts issue relatively fewer optimistic recommendations than other analysts who follow the same companies. Hence, sanctioned bank analysts were not only less optimistic than their nonsanctioned bank counterparts but also less optimistic than analysts from syndicate, brokerage, and research firms. Second, we report a relative, instead of an absolute, optimism metric.[13] The use of the relative optimism metric rules out the possibility that the percentage of buys issued by sanctioned banks drops much more than that of nonsanctioned banks because they follow different companies, and these companies are affected differently by the market downturn around the implementation of NASD 2711. Third, we examine optimism bias at the analyst level, instead of at the bank level. This allows us to control for both cross-sectional and time-series differences in analyst characteristics in our regression analysis.

Table 5. Daily Abnormal Return of Buy and Hold/Sell Stock Recommendations by Firm Type and Sample Period (N = 11,021)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample period	Research firms	Brokerage firms	Syndicate firms	Nonsanctioned banks	Sanctioned banks	(2) − (1)	(3) − (1)	(4) − (1)	(5) − (1)
Panel A: Buy portfolio									
Preperiod	0.045	0.028	0.051	0.052	0.060	−0.017	0.006	0.007	0.016
Postperiod	0.021	0.015	0.014	0.021	0.017	−0.006	−0.007	−0.001	−0.004
Change	−0.023	−0.013	−0.037	−0.031***	−0.043***	0.010	−0.014	−0.008	−0.020
Panel B: Hold/sell portfolio									
Preperiod	0.087	0.086	0.068	0.075	0.092	−0.001	−0.019	−0.012	0.005
Postperiod	−0.013	−0.016	−0.001	−0.003	0.008	−0.003	0.012	0.009	0.021**
Change	−0.100*	−0.102***	−0.069	−0.078***	−0.084	−0.002	0.031	0.022	0.016

Note: The table reports results on the daily abnormal returns of the buy and hold/sell recommendations made by analysts from different securities firm type. The pre- and post-reform periods cover January 1998 to December 2001 and January 2004 to 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 expressed in percentage and 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 postreform periods. Stock recommendation data are from Thomson Financial's Institutional Brokers' Estimate System database. See Table 1 or the text for the classification of securities firm type.

*, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t test.

Kadan et al. (2009) show that affiliated investment banks are as likely to issue optimistic recommendations (defined as ''strong buys'' or ''buys'') as unaffiliated banks in the postreform period September 2002 to December 2004. However, there is no change in the reluctance of affiliated investment banks to issue pessimistic recommendations (defined as ''underperform'' and ''sell'') than nonaffiliated banks. Unlike the research design of Kadan et al., our research does not use affiliation to capture conflicts of interest, and we use a relative optimism metric. We add to their results by showing that sanctioned bank analysts (not just affiliated investment bank analysts) become less optimistic than other analysts who follow the same companies. In contrast to their results, we also document that sanctioned bank analysts are issuing more pessimistic recommendations relative to other analysts who follow the same companies. With a postreform period extended to 2007, we are able to investigate the long-term impact of the reforms.[14]

In summary, we document evidence consistent with the conflicts-of-interest reforms reducing the relative optimism of stock recommendations made by sanctioned bank analysts. It is unclear whether the drop in the optimism of stock recommendations would 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.

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 postreform periods. We compute the profitability of stock recommendations using the methodology of Barber, Lehavy, and Trueman (2007), except that we form trading portfolios at the analyst level instead of at the 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 when 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 is the alpha from the estimation of the Fama–French three factors plus the Carhart momentum factor regression model (Carhart, 1997; Fama & French, 1993), estimated by analyst and over the pre- and postreform periods.

Table 5 summarizes the findings. Panel A shows that average daily abnormal returns for the buy portfolios of research firm analysts drop from 4.5 basis points before the reforms to 2.1 basis points after the reforms, but the change is indistinguishable from zero. Similarly, we also see a drop in the recommendation profitability for brokerage and syndicate firm analysts, although not significantly so. However, the changes for both nonsanctioned and sanctioned investment bank analysts are significantly negative. However, the drop in the profitability of investment bank recommendations is not significantly different from that of research firms, as shown in columns 8 and 9.

Panel B reports the results for the hold/sell portfolio. It indicates that after the reforms, average abnormal returns of hold/sell recommendations made by all types of analysts decrease (i.e., it become more profitable), with such decreases being statistically significant for analysts from research firms, brokerage firms, and nonsanctioned banks. The statistics in columns 6 to 9 show that the changes are not statistically different from those of research firms.

Table 6. Difference-in-Differences Regressions of the Level of Recommendation Optimism (N = 11,021)

	Average recommendation level		Average adjusted recommendation level	
	Coefficient	*t* statistics	Coefficient	*t* statistics
Intercept	−139.08	−12.52***	29.47	3.34***
D	−23.23	−3.29*	8.47	1.59
BROKERAGE	−4.22	−0.43	−2.64	−0.34
SYNDICATE	−6.89	−0.95	−4.45	−0.79
NONSANC	0.90	0.13	0.89	0.18
SANCTIONED	3.53	0.50	0.33	0.06
D × *BROKERAGE*	3.25	0.29	0.58	0.07
D × *SYNDICATE*	0.85	0.10	2.58	0.40
D × *NONSANC*	−10.79	−1.57	−3.28	−0.64
D × *SANCTIONED*	−36.97	−5.26***	−20.60	−3.90***
Relative accuracy	0.05	1.08	0.12	2.93***
Forecast horizon	−0.04	−2.65**	−0.04	−2.99***
Experience	0.11	0.75	0.013	1.07
Number of companies following	−0.30	−3.17***	−0.15	−1.98**
Industry specialization	−4.31	−1.59	2.38	1.06
Analyst turnover	3.60	1.70*	1.04	0.60
Percentage of new following	−2.65	−0.83	−5.14	−1.94*
Brokerage firm size rank	−0.20	−2.76***	−0.13	−2.33**
Brokerage firm specialization	−26.93	−5.18***	−13.11	−3.14***
Company size	−2.67	−3.93***	−1.20	−2.15**
Leverage	−15.53	−2.99***	−2.20	−0.50
Gross margin	−0.003	−0.41	−0.001	−0.14
Sales growth	−0.03	−0.54	−0.01	−0.19
Book-to-market	−6.43	−1.89*	5.35	2.06**
External financing	25.89	3.91***	−0.33	−0.06
R^2	.112		.023	

Note: This table reports the ordinary least squares estimation results of the following regression:

$$DEP_{it} = \alpha_0 + \alpha_1 D + \alpha_2 BROKERAGE + \alpha_3 SYNDICATE + \alpha_4 NONSANC + \alpha_5 SANCTIONED + \alpha_6 D \times BROKERAGE + \alpha_7 D \times SYNDICATE + \alpha_8 D \times NONSANC + \alpha_9 D \times SANCTIONED + CONTROLS + e_{t,}$$

where the dependent variable, *DEP*, is either average recommendation level or average adjusted recommendation level.

Average recommendation level is the average of all stock recommendations made by an analyst in a particular year. *Average adjusted recommendation level* is the average of all stock recommendations made by an analyst minus the average of all the recommendations made by other analysts who follow the same companies. The dependent variables are multiplied by 100. *D* is an indicator variable that equals to one in the postreform period (January 2004 to December 2007) and zero in the prereform period (January 1998 to December 2001). *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 nonsanctioned 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. *CONTROLS* represent a set of control variables for analyst, firm, and portfolio characteristics, which are defined in the section titled "Model Specification, Variable Definitions, and Descriptive Statistics." Year-fixed effects are also included. Robust standard errors are clustered by analyst.

*, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided *t* test.

In the section titled ''Recommendation Optimism,'' we find that sanctioned bank analysts become relatively less positive and more negative in their recommendations after the reforms. Hence, the shift in the distribution of their recommendations implies that their buy recommendations should become more profitable, whereas their sells should become less profitable.[15] However, in Table 5, column 5 shows that the average change in the profitability of buys for sanctioned bank analysts is significantly negative, whereas that of sells is insignificantly different from zero. This finding leads us to conclude that, although the reforms reduce the optimism of sanctioned bank analysts' recommendations, the reduction does not lead to improvement in the profitability of their recommendations.

Our prereform period (1998-2001) results are consistent with the findings in Barber et al. (2007) that there is no significant difference in the stock recommendation performance between analysts from research firms (including brokerage firms) and those from investment banks for the period January 1996-March 2000. Furthermore, Barber et al. find a significant difference in the returns to investment bank and research firm recommendations in the period from March 2000 through June 2003, suggesting that the reforms have economic consequence. We provide evidence from a long postreform period (January 2004-December 2007) that the reforms have different impact on buy and sell recommendations, but overall, these reforms do not lead to improvement in the profitability of stock recommendations made by investment bank analysts.

Robustness Checks

This subsection includes a series of sensitivity analyses using the same DD research design.[16]

First, following prior studies (e.g., Chen & Chen, 2009; Cowen et al., 2006), we use the level of stock recommendations instead of relative recommendation optimism as the dependent variable (1 = *strong sell*, 2 = *sell*, 3 = *hold*, 4 = *buy*, and 5 = *strong buy*).[17] Given that our observations are at the analyst-year level, we calculate the average of all recommendations made by each analyst in a particular year. As the dependent variable is an average of many discrete values, it is close to a continuous variable when the number of stock recommendations made by an analyst increases (Table 2 indicates that the average analyst covers at least 11 companies), and we use the ordinary least squares method to estimate the regression. The results reported in Table 6 are stronger than our original findings in Table 4. In particular, the *t* statistic on the interaction term $D \times SANCTIONED$ in the *average recommendation level* regression is 5.26. Furthermore, we account for company-fixed effects by subtracting from each recommendation the mean of all recommendations for the same company made by other analysts in the same calendar quarter. The result is summarized under the column titled ''Average adjusted recommendation level.'' The regression result is robust to the mean adjustment, with the *t* statistics on the $D \times SANCTIONED$ term being 3.90.

Second, the composition of our sample changes over time: analysts switch employers, new analysts enter the industry, old analysts exit the industry, and research firms, brokers, and banks are added to and dropped from the I/B/E/S database. As a result, the characteristics of the analysts, the firms they work for, and the companies they follow are changing during the sample period. We have already controlled for these sources of variation in the DD regression. As a robustness check, we redo our tests using a sample of analysts who are present in both the pre- and postreforms periods. We lose two thirds of our analysts due to this strict constraint. Untabulated results show that the original result on the interaction

Table 7. Descriptive Statistics on Relative Forecast Optimism and Accuracy by Firm Type and Sample Period (N = 11,021)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample period	Research firms	Brokerage firms	Syndicate firms	Nonsanctioned banks	Sanctioned banks	(2) − (1)	(3) − (1)	(4) − (1)	(5) − (1)
Panel A: Relative forecast optimism									
Preperiod	0.000	0.023	−0.025	−0.020	0.004	0.023	−0.025	−0.020	0.004
Postperiod	0.076	0.078	−0.020	−0.010	−0.022	0.002	−0.096***	−0.086***	−0.099***
Change	0.076	0.055	0.005	0.010	−0.026	−0.021	−0.072	−0.066	−0.102*
Panel B: Relative forecast accuracy									
Preperiod	53.14	58.40	58.57	61.03	62.64	5.27**	5.44***	7.90***	9.50***
Postperiod	56.06	54.10	57.53	58.92	58.71	−1.95	1.47	2.86***	2.66***
Change	2.92	−4.30*	−1.05	−2.11***	−3.93***	−7.22**	−3.97*	−5.03**	−6.85***

Note: This table reports the relative optimism and accuracy of earnings forecasts made by analysts from different securities firm type. The pre and postreform periods cover January 1998 to December 2001 and January 2004 to December 2007, respectively. Panel A reports results on relative forecast optimism, *RFOPT*, which is forecasted earnings minus actual earnings, scaled by the standard deviation of all forecasts for the same company. Panel B reports results on relative forecast accuracy, which is forecast accuracy rank for all companies followed by an analyst. Analyst earnings forecasts are from Thomson Financial's Institutional Brokers' Estimate System database. See Table 1 or the text for the classification of securities firm type.

*, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided t test.

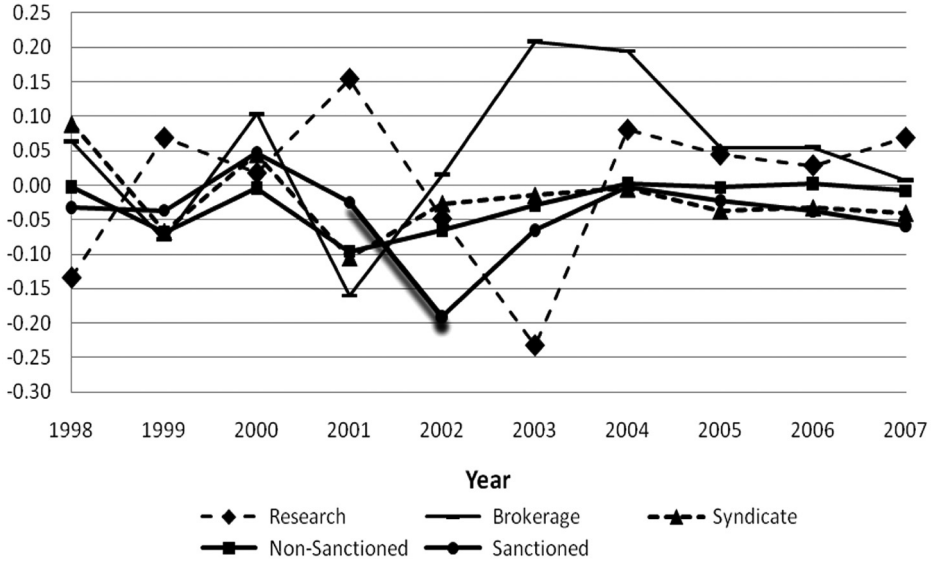

Figure 2. Relative forecast optimism for equity analysts from five types of securities firms, 1998 to 2007

Note: Relative forecast optimism, *RFOPT*, is forecasted earnings minus actual earnings, scaled by the standard deviation of all forecasts for the same company. The graph plots the annual averages of *RFOPT* for analysts from five different types of securities firms: research firms, brokerage firms, syndicate banks, nonsanctioned investment banks, and sanctioned investment banks.

term $D \times SANCTIONED$ in the *RROPT* regression is no longer statistically significant; the qualitative results for the *LessPOS* and *LessNEG* regressions remain unchanged. As the constraint leads to a significant reduction in sample size, survivorship bias could become a problem in this sensitivity analysis. We believe that our original sample is more representative of the general population of analysts.

Third, if we cluster standard errors at the securities firm level, the estimated coefficients on $D \times SANCTIONED$ for the *RROPT* and *LessPOS* regressions in Table 4 (relative recommendation optimism) will become statistically insignificant. The statistical inference of the results reported in Table 6 (recommendation level) is not affected. It is generally true that standard errors are larger (significance levels are lower) when they are clustered at a higher level of aggregation, as the number of clusters decreases. However, our choice of clustering standard errors at the analyst level is in line with prior studies. For example, Barniv, Hope, Myring, and Thomas (2009) and Chen and Chen (2009) use a sample of company-month observations based on analysts' consensus forecasts and recommendations; they compute *t* statistics using robust standard errors adjusted for clustering by company. As we use a sample of analyst-year observations, we cluster the standard errors by analyst.[18] Furthermore, Cowen et al. (2006) use a sample of individual analyst forecasts and recommendations, and they adjust standard errors for clustering by analyst. Kadan et al. (2009) examine a sample of individual analyst recommendations, and they report standard errors clustered at the company level (pp. 16-17). Our *t* statistics are no less conservative than those used in these studies. By aggregating individual forecasts and recommendations

Table 8. Difference-in-Differences Regressions of Relative Forecast Optimism and Accuracy (N = 11,021)

	Relative forecast optimism		Relative forecast accuracy	
	Coefficient	*t* statistic	Coefficient	*t* statistic
Intercept	−32.19	−3.17***	68.51	22.07***
D	9.25	1.51	1.97	0.89
BROKERAGE	1.86	0.24	2.52	0.95
SYNDICATE	−3.21	−0.51	3.35	1.51
NONSANC	−2.60	−0.46	3.55	1.74*
SANCTIONED	−0.25	−0.04	4.05	1.92*
D × *BROKERAGE*	−1.24	−0.14	−3.58	−1.18
D × *SYNDICATE*	−6.90	−0.98	−2.36	−0.97
D × *NONSANC*	−6.04	−1.01	−2.94	−1.39
D × *SANCTIONED*	−9.63	−1.57	−5.16	−2.39**
Relative forecast accuracy	0.28	6.86***		
Forecast horizon	0.03	1.86*	−0.02	−3.98***
Experience	0.07	0.61	−0.13	−3.33***
Number of companies following	−0.04	−0.54	−0.03	−1.28
Industry specialization	0.99	0.45	1.57	2.16**
Analyst turnover	4.26	2.41**	−18.11	−34.68***
Percentage of new following	7.17	2.46**	−4.26	−4.71***
Brokerage firm size rank	−0.02	−0.36	0.04	2.22**
Brokerage firm specialization	−0.21	−0.05	−3.12	−2.13**
Company size	0.92	1.62	−0.19	−1.02
Leverage	−4.08	−0.91	−4.21	−2.76***
Gross margin	0.002	0.19	−0.003	−1.44
Sales growth	0.01	0.30	0.004	0.33
Book-to-market	−2.28	−0.86	−0.15	−0.16
External Financing	2.68	0.44	−8.34	−4.38***
R^2	.013		.145	

Note: This table reports the ordinary least squares estimation results of the following regression:

$$DEP_{it} = \alpha_0 + \alpha_1 D + \alpha_2 BROKERAGE + \alpha_3 SYNDICATE + \alpha_4 NONSANC + \alpha_5 SANCTIONED + \alpha_6 D \times BROKERAGE + \alpha_7 D \times SYNDICATE + \alpha_8 D \times NONSANC + \alpha_9 D \times SANCTIONED + CONTROLS + e_{t,}$$

where the dependent variable, *DEP*, is either relative forecast optimism or relative forecast accuracy. Forecast optimism is forecasted earnings minus actual earnings, scaled by the standard deviation of all forecasts for the same company. Forecast optimism is multiplied by 100. 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 postreform period (January 2004 to December 2007) and zero in the prereform period (January 1998 to December 2001). *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 nonsanctioned 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. *CONTROLS* represent a set of control variables for analyst, firm, and portfolio characteristics, which are defined in the section titled "Model Specification, Variable Definitions, and Descriptive Statistics." Year-fixed effects are also included. Robust standard errors are clustered by analyst.

*, **, and *** denote statistically different from zero, respectively, at the 10%, 5%, and 1% level using a two-sided *t* test.

at the analyst level, we completely remove any correlation among forecasts and recommendations issued by the same analyst.

Empirical Findings on Earnings Forecasts

Relative Forecast Optimism

We estimate the relative forecast optimism measures following Clement (1999) and others. The measure, *RFOPT*, is calculated as follows:

$$RFOPT_{ijt}^{t-k} = \frac{FORECAST_{ijt}^{t-k} - \overline{FORECAST_{jt}^{t-k}}}{STDDEV(FORECAST_{jt}^{t-k})},$$

where $FORECAST_{ijt}^{t-k}$ is analyst i's forecast of company j's earnings for fiscal year t, as of $t - k$. $\overline{FORECAST_{jt}^{t-k}}$ and $STDDEV(FORECAST_{jt}^{t-k})$ are, respectively, the average and standard deviation of all forecasts for company j and fiscal year t, as of $t - k$.[19] Following prior literature (Cowen et al., 2006), we use only the first forecast made by each analyst at the beginning of the fiscal year ($t - k$) for the same company and forecast period (FY1—the current fiscal year). We compute $RFOPT_{ijt}^{t-k}$ only for companies that are followed by at least three analysts. $RFOPT_{ijt}^{t-k}$ is then averaged across all companies followed by analyst i in calendar year t to compute analyst i's average relative forecast optimism at calendar year t, $RFOPT_{it}^{t-k}$. By construction, this measure controls for company- and time-specific factors that would affect forecast optimism across analysts.

Table 7, Panel A reports the levels of and changes in average relative forecast optimism of annual earnings made by analysts from different firm types.[20] The changes in relative forecast optimism are 0.076, 0.055, 0.005, 0.010, and −0.026 for analysts from research, brokerage, syndicate, nonsanctioned, and sanctioned firms, respectively. None of these changes are statistically different from zero. However, column 9 shows that the change in the relative forecast optimism of sanctioned bank analysts is statistically more negative than that of their research firm counterparts.

Figure 2 plots the annual relative forecast optimism by firm type. It shows that the relative forecast optimism of the sanctioned bank analysts is very similar to that of other analysts in both the pre- and postreform periods. The relative forecast optimism of the sanctioned bank analysts does not exhibit a clear trend after the reforms and its movement is close to that of the analysts from other types of securities firms.

Table 8 summarizes the DD regression results. In the relative forecast optimism regression, none of the DD estimates (i.e., those on the interaction terms) are statistically different from zero. This result indicates that the changes in relative forecast optimism around the reforms are not significantly different between research firm analysts and their investment bank counterparts. In other words, we do not observe any significant change in the incentives of analysts making optimistic earnings forecasts across different firm types. As for the control variables, the results indicate that analysts who experience high turnover and cover many new companies are relatively more optimistic.

In sum, we document evidence consistent with the reforms having no statistical effect on the relative forecast optimism of investment bank analysts. This is in contrast to what we find for stock recommendations. The fact that securities regulators focus their attention on stock recommendations rather than earnings forecasts might explain these results. In

other words, sanctioned investment banks reduce their stock recommendation optimism in response to the reforms but leave their earnings forecasts optimism unchanged, partly because earnings forecasts are not the focus of the reform.

Relative Forecast Accuracy

We next examine the consequence of the reforms on the accuracy of analysts' forecasts. The calculation of relative forecast accuracy is given in the section titled ''Model Specification, Variable Definitions, and Descriptive Statistic''.

Panel B in Table 7 shows striking results. First, the initial row of the panel indicates that in the prereform period, the forecasts of brokerage, syndicate, nonsanctioned, and sanctioned firms are more accurate than those of research firms. Second, the accuracy of research firm analysts improves after the reforms. In contrast, sanctioned and nonsanctioned investment bank analysts become significantly less accurate after the reforms, although they are still statistically more accurate than their research bank counterparts in the postreform period (as shown in columns 8 and 9).

The multivariate result for relative forecast accuracy is given in the last set of columns in Table 8. The estimated coefficients on *NONSANC* and *SANCTIONED* are statistically positive, indicating that the earnings forecasts made by analysts from nonsanctioned and sanctioned investment banks are relatively more accurate than those made by analysts from research firms in the prereform period. On the contrary, the DD estimate is significantly negative for sanctioned banks only. This is consistent with the reforms having a differential impact on the accuracy of research firm and sanctioned investment bank analysts.

In summary, the reforms targeting investment bank analysts have negatively affected the forecast accuracy of investment bank analysts and unexpectedly improves the accuracy of research firm analysts. The former might be due to the fact that investment bank research departments lose their funding from investment banking businesses. The latter might be attributed to the fact that the Global Settlement provides US$432.5 million to support independent analyst research. Indeed, the statistics in Table 2 show that research firm analysts cover fewer companies and experience less turnover after the reforms. As a result, the accuracy of sanctioned investment bank analysts is no longer significantly better than their research firm counterparts after the reforms.

Robustness Checks

We conduct three sets of sensitivity analyses. First, Regulation Full Disclosures (Reg FD) might affect our results because its effective date of October 2000 falls within our prereform period (1998-2001). Prior literature examining the impact of Reg FD on analyst forecast accuracy finds mixed results. Although Bailey, Li, and Mao (2003) show that Reg FD had no impact on accuracy, Agrawal, Chadha, and Chen (2006) document that forecasts became less accurate post Reg FD. The sample period used by Bailey et al. (2003) 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.

We repeat our analysis including only year 2001 in the prereform period. Our results (not tabulated) remain qualitatively unchanged, suggesting that the conflicts-of-interest reforms, not Reg FD, are associated with our findings.

Second, we examine relative forecast optimism and relative forecast accuracy in our main tests because they control for firm-specific effects (Clement, 1999; Cowen et al., 2006). Nevertheless, it is important to know whether our conclusions are sensitive to the use of the relative measures. When we use absolute forecast optimism and absolute forecast accuracy as the dependent variables in the DD regressions, our original results are robust. Furthermore, most of the control variables exhibit significant explanatory power for analysts' absolute optimism and absolute accuracy.

Third, as in the section titled ''Robustness Checks,'' we repeat our analysis on a sample of analysts who were present in both the pre- and postreforms periods. Untabulated results indicate that our original findings on the effect of the reforms on earnings forecast optimism and accuracy are robust to the imposition of this sample restriction.

Finally, if we cluster standard errors at the securities firm level, the significance level of the estimated coefficients on $D \times SANCTIONED$ remains unchanged.

Concluding Remarks

This article examines the consequences of a series of reforms that aim at resolving analyst conflicts of interest driven by the investment banking business. We conduct our tests on analysts from different types of securities firms: research firms, brokerage firms, syndicate banks, nonsanctioned investment banks, and sanctioned investment banks. We use securities firm type to capture the level of investment banking–related conflicts of interest facing the analysts. We examine the change in analysts' research biases between the prereform period (January 1998-December 2001) and postreform period (January 2004-December 2007).

We find a significant reduction in the relative optimism of stock recommendations but no significant change in the relative optimism of earnings forecasts made by sanctioned investment bank analysts. We also document that the accuracy of investment bank forecasts drops, and the profitability of its stock recommendations remains unchanged after the reforms. Taken together, our evidence from an investigation of the 4-year-long postreform period suggests that although the conflict-of-interest reforms reduce the optimism of stock recommendations issued by sanctioned investment bank analysts, the reforms also have an unintended negative consequence: Specifically, investors do not gain economic benefits from the less pessimistic stock recommendations, although they receive less accurate earnings forecasts.

It should be noted that we capture the level of analysts' conflicts of interest using the type of securities firms, and, hence, we do not test whether affiliated investment bank analysts are more biased than their nonaffiliated counterparts. In a related study, Kadan et al. (2009) show that affiliated investment banks are as likely to issue optimistic recommendations as unaffiliated banks in the postreform period from September 2002 to December 2004. However, they find no change in the reluctance of affiliated investment banks to issue pessimistic recommendations as compared with nonaffiliated banks. We add to their results by showing that sanctioned bank analysts become less optimistic than other analysts (not just affiliated analysts) who follow the same companies. In contrast to their results, we also document that sanctioned bank analysts are issuing more pessimistic recommendations relative to other analysts who follow the same companies.

Acknowledgments

The authors thank Craig Chapman, Gus De Franco, Michael Halling, Artur Hugon, Bin Ke, Francis Kim, Stephannie Larocque, Kent Womack, Yong Yu, an anonymous reviewer, and workshop participants at Chulalongkorn University, the City University of Hong Kong, McGill University, University of Toronto, and 2009 European Finance Association annual conference for their helpful comments. The authors also thank Xiaohua Fang, Yanju Liu, and Yuchao Zeng for their research assistance.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: Guan appreciates research start-up grant from City University of Hong Kong. Lu and Wong acknowledge financial support from the Rotman School of Management at the University of Toronto and the Social Sciences and Humanities Research Council of Canada.

Notes

1. An alternative way to capture investment banking incentives is to divide investment bank analysts into affiliated and nonaffiliated analysts. However, nonaffiliated analysts also have incentive to bias their research to help their banks attract future investment banking business (e.g., Bradley, Jordan, & Ritter, 2006). As the reforms target all investment banks, we believe that our partitioning is the appropriate one for addressing our research questions. Prior studies using the affiliation classification have found mixed results. In particular, 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 favorable recommendations than unaffiliated analysts. Michaely and Womack also find that the stock recommendations of affiliated analysts underperform those of unaffiliated analysts for a sample of initial public offering (IPO) firms. However, Dugar and Nathan, Lin and McNichols, and McNichols, O'Brien, and Pamukcu (2007) find no statistical difference in the profitability of buy recommendations issued by affiliated and unaffiliated analysts.
2. The different outcomes could be due to the fact that the reforms focus on optimistic stock recommendations, and earnings forecasts are seldom mentioned in any of the legislations.
3. Our study is silent on the effect of the reforms on the alignment of earnings-based valuation estimates and stock recommendations, which are addressed in Barniv Hope, Myring, and Thomas (2009) and Chen and Chen (2009). See Bradshaw (2009) for a discussion. Our study is also silent on whether a subset of investors benefit from the reforms. For example, De Franco, Lu, and Vasvari (2007) show that analysts' research biases adversely affect small investors but not institutional investors, and Ljungqvist, Marston, Starks, Wei, and Yan (2007) find that analyst biases are smaller for firms with higher institutional ownership due to the monitoring role of institutional investors.
4. Indeed, only a few of the retail clients of these sanctioned investment banks actually request these independent research reports (Kim, 2009).
5. The two bodies have now consolidated most of these operations into the Financial Industrial Regulatory Authority (2005).
6. Institutional Brokers' Estimate System (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 lose new investment research firms (and their analysts) that were added to I/B/E/S in 2007.

7. The broker identifier in the I/B/E/S translation file is provided at the subsidiary level. We manually check the name of each subsidiary and assign them under its parent securities firm, which is given in the Nelson's Directory of Investment Research.
8. Lead or colead underwriters (or book runners) are chosen by the issuers (IPO or Seasoned Equity Offering companies) to handle all aspects of the equity offerings, including pricing, marketing, and distributing. The managers or comanagers are selected by the lead or colead underwriters to facilitate distribution of the offering. See Cowen Groysberg, and Healy (2006) and Ljungqvist, Marston, and Wilhelm (2009).
9. If an analyst changes jobs from one firm type to another, we assign her to the firm type of her original employer in the switching year. The results (not tabulated) are robust if we exclude these analysts from our sample.
10. These results are reported in a previous draft of the article, which is available on request from the authors.
11. For example, assume analyst *i* follows a company with a hold recommendation. There are nine other analysts following the same company and their recommendations are 3 buys, 4 holds, and 2 sells. *LessPOS* and *LessNEG* will be 30% and 20%, respectively, for analyst *i*. If one of these analysts downgrades from a hold to a sell, analyst *i*'s *LessPOS* and *LessNEG* will change to 30% and 30%, respectively. In other words, analyst *i* becomes relatively less negative (i.e., *LessNEG* increases), because one more analyst has a more unfavorable recommendation than her; however, *LessPOS* remains unchanged. If another analyst upgrades from a sell to a buy, analyst *i*'s *LessPOS* and *LessNEG* will change to 40% and 20%, respectively. In other words, analyst *i* becomes both relatively less positive (i.e., *LessPOS* increases) and more negative (i.e., *LessNEG* decreases).
12. In their review of the extant literature, Mehran and Stulz (2007) point out that ''some of the effects of these [conflicts-of-interest] regulations might not be noticeable with such a short sample period'' (p. 292). Our tests supplement these two studies by examining a longer postreform sample period. Moreover, our research design is different from those of these two studies and, hence, our study provides triangulating evidence on the economic consequences of the reforms.
13. In sensitivity tests, we find that the results are robust when we use an absolute optimism measure.
14. Kadan, Madureira, Wang, and Zach (2009) find that many securities firms moved from a five-tier stock rating system to a three-tier system in 2002. All post-2006 I/B/E/S data tapes include retrospective changes to brokers' alterations of their recommendation scales (Ljungqvist, Malloy, & Marston, 2009). As we use the 2008 I/B/E/S data, our results are not affected by the change in the stock rating system.
15. This follows the logic of Barber, Lehavy, McNichols, and Trueman (2006). They predict and find that the buy (sell) recommendations issued by optimistic securities firms earn lower (higher) abnormal returns than those issued by less optimistic firms.
16. We thank the referee for suggesting these robustness checks.
17. The recommendation level is defined in such a way that the sign on the difference-in-differences estimators is identical to that in Table 4 (relative recommendation optimism).
18. Clustering standard errors at the securities firm level in our tests will be equivalent to clustering standard errors at the industry level in Barniv et al. (2009) and Chen and Chen (2009).
19. We winsorize forecast optimism at the 1st and 99th percentiles because some standard deviations, the deflator, are extremely small. Results are qualitatively similar if we scale this measure by stock price.
20. The sample used in this section is the same as that used in the stock recommendation tests. If we do not restrict the sample here to have stock recommendation data, the sample size will increase from 11,201 to 18,918. The results (not tabulated) based on the larger sample are more significant, but qualitatively similar, to those reported in the table.

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