Does Reciprocity Affect Information Production?

Evidence from Syndication Relationships in Securities Underwriting

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Abstract

Reciprocity is a subtle but influential element in maintaining relationships in business dealings. However, empirical research on the effects of reciprocity is scarce due to the challenge of data availability. In this paper, we fill the gap by examining the stock recommendations issued by "dependent" analysts affiliated with securities underwriters who rely on other major underwriters' invitations to be in syndicates. We find that dependent analysts delay releasing negative information of other major underwriters' clients, despite the fact that these dependent analysts' affiliated underwriters do not received fees from those clients. We also document temporary retaliation by major underwriters when dependent analysts deviate from such expected reciprocity.

1.   Introduction

A concern of investors when interpreting analyst reports is that analyst recommendations about firms depend on the relationships between the analysts' banks and the firms being analyzed. This widely-reported conflict of interest contradicts the notion that analysts are unbiased and that their reports can be trusted by market participants who do not fully understand the relationships between analysts and the firms they cover.[1] To make matters worse for investors, the financial services industry contains networks of relationships between institutions that could influence participants' behaviors. While there are good reasons for these relationships, their existence potentially leads institutions to behave in a reciprocal manner to maintain the relationships.[2] The desire to be reciprocal could lead to collusive behavior that undermines the functions of financial services.[3] In fact, the possibility of 'reciprocity' in investment banking has raised concerns among regulators, the Financial Conduct Authority in U.K. has launched an investigation that scrutinizes the practices of reciprocity among European banks. In this paper, we analyze the extent to which reciprocity affects analyst recommendations and compromises the usefulness of the information they produce.

Recent research in economics has suggested that individuals tend to display "reciprocity," which "assumes that a player cares about the intentions of her opponent. If she feels treated kindly, she wants to return the favor and be nice to her opponent. If she feels treated badly, she

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[1] Lin and McNichols (1998) was the original study documenting the bias of analysts' recommendations depending on the investment banking relationship between the analyst and the firm. Besides the conflict of interest due to underwriting fees, subsequent work, such as Cowen, Groysberg, and Healy (2006), also show that analysts can be pressured to generate more trading commissions by issuing optimistic reports.

[2] Such relationships could enhance financial services ranging from securities underwriting, syndicate lending, to venture capital investing. See, for example, Hochberg, Ljungqvist, and Lu (2007).

[3] See "European Banks' Reciprocity Draws Scrutiny: Banks give work to competitors partly based on how much business they receive in return," by David Enrich and Shayndi Raice, Wall Street Journal, March 1, 2015.

wants to hurt her opponent."[4] The tendency of individuals toward reciprocity can potentially lead toward implicit collusion, especially in settings in which people have to interact in multiple dimensions. Such a setting is the financial services industry, since there are multiple networks in which banks interact, so have incentives to develop a reciprocal relationship with one another. One area where we can observe the empirical implications of this behavior is in the analyst recommendations.[5]

In the securities underwriting markets, deals are often organized by prominent underwriters who invite other underwriters to participate in the syndicates based on either issuing firms' requests or past syndication relationships.[6] However, not all underwriters have the capacity to originate their own deals and reciprocate correspondingly by deal opportunities, instead they have to rely on other major underwriters' invitations to be in syndicates. This reliance can give rise to expected reciprocity in other dimension of business, i.e., it is possible that major underwriters anticipate analysts of these syndicate members to curry favor their reports on the major underwriters' clients even when syndicate members do not receive underwriting fees from the covered firms.

To test this proposition, we consider a sample of firms that have been sued for alleged financial misreporting, and examine the timeliness of downgrades issued by analysts bearing different degrees of reciprocal pressure. We identify banks that have syndication relationships with major banks but do not have capacity to arrange deals and do not serve major banks' clients

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[4] See Fehr and Schmidt (2003) for this quote.

[5] Although extant literature documented the conflict of interest of affiliated analysts, other studies also provide evidence that sell-side analysts channel information from firms to investors. Gurun, Johnston, and Markov (2016) focus on debt analysts and find that timely debt reports enhance market efficiency by inducing quicker trading and greater return responses. Using scenario-based equity valuations, Joos, Piotroski, and Srinivasan (2016) show that analysts' assessments of state-contingent valuation risk appear to be unbiased.

[6] See, for example, Corwin and Schultz (2005).

in securities underwriting.[7] We evaluate whether analysts working for a bank with such a syndication relationship affects their recommendations. While the existing literature focuses on potential biases from "affiliated" analysts, who work for banks that underwrite the covered firm's securities, we consider the possibility that analysts adjust their recommendations because of reciprocity between banks. Our reciprocal pressure hypothesis predicts the following: analysts working for banks that are susceptible to reciprocal pressure behave more like affiliated analysts in delaying downgrades of sued firms. Therefore, we refer to these reciprocity pressured analysts as "dependent" analysts. In contrast, independent analysts should provide downgrades of sued firms more promptly than affiliated analysts.

Our evidence suggests that such reciprocity exists in the financial services industry. Specifically, our analysis shows that dependent analysts did not provide more prompt downgrades on sued firms than affiliated analysts. Independent analysts issued downgrades significantly earlier by about 26% of the duration of the class period (about 77 days) than affiliated analysts.[8] These findings are consistent with our conjecture that dependent analysts are more susceptible to reciprocal pressure than independent analysts.

In addition, major underwriters appear to retaliate against uncooperative syndicate members. In particular, when dependent analysts deviate from expected reciprocity by downgrading sued firms earlier than the analysts of their underwriters, these major underwriters retaliate by reducing syndicate invitations. We measure the intensity of syndicate invitation by

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[7] We label these banks as co-manager syndicate banks in our analysis. The definitions of analyst/bank types can be found in Panel A of Appendix A1. In Figure 1, we illustrate the relationships of these bank types.
[8] Each lawsuit filing provides two critical dates: the class period starting date (a proxy for the starting date of the covered firm's wrongdoings) and the class period ending date (a proxy for the date that wrongdoings are usually uncovered). We use this class period—lasting on average about one year—to analyze information production by analysts prior to public knowledge about the wrongdoings. Although the class period of a lawsuit is a reasonable proxy for private corporate wrongdoing period, it may suffer from the problem of statute of limitations. We address this issue in Section 4.3.3.

aggregating all equity deal amounts that an invitee bank participates in a major underwriter's syndicates during an event window. This aggregated number for the invitee is then scaled as a percentage of the major underwriter's total deal amounts during the same period. We conduct difference-in-differences (DID) test relative to a matched sample for the one-year and three-year windows surrounding the uncooperative downgrades. We find that, on average, major underwriters significantly reduce syndicate invitations by 4.9% for the one-year window. This finding is also economically significant because the average syndication relationships is about 14%. However, the result for the three-year window is not significant. It appears that the retaliation is only temporary. In contrast, such retaliation is not observed toward banks affiliated with independent analysts who appear to be more independent in the first place.

Furthermore, we find significant changes in the timeliness of downgrades following the Global Settlement and the adoption of NASD Rule 2711, which aim to improve analyst independence. We document that affiliated analysts improved their promptness in disseminating negative information by more than 70% of the class period duration. The dependent analysts also became more prompt in downgrading sued firms and were no longer later than the independent analysts. It appears that Rule 2711 not only reduces the direct conflict of interest of client pressure between underwriters and securities issuing firms but also the more subtle reciprocal pressure between major underwriters and syndicate members. These results suggest that reciprocity is an important element of banking relationships and affects banking actions to a much greater degree than the literature has previously documented.

Our paper fits into the literature addressing the following general research question: How do institutions work with and affect each other? This is an important question but empirical evidence is scarce. However, studies of this sort are emerging in the literature. For example,

Baum, Bowers, and Mohanram (2016) examine the effect of mutual forbearance—a reduction in competition due to familiarity and deterrence—among securities analysts covering multiple stocks in common with their rivals. In contrast, we provide evidence to show that reciprocity has significant effects on the behaviors of financial institutions and such incentives can affect information production of their analysts. Another example is Hochberg, Ljungqvist, and Lu (2010). They show that strong networks among incumbent venture capitalists restrict entry, thus, increase their bargaining power over entrepreneurs. They also document a retaliation behavior that incumbents freeze out other incumbents who facilitate entry into their market.

Furthermore, our analysis adds to growing literature that looks beyond traditional analyst studies. For example, Brochet, Miller, and Srinivasan (2014) study the relationships between analysts and CEOs/CFOs of covered firms. Our paper focuses on the relationships among syndicate members of securities underwriting and the potential impacts of such relationships on analyst behavior.

By using the securities lawsuits, we provide the first analysis of the timeliness of analysts' recommendations during negative events prior to public awareness of the events. Most of the prior studies on analysts' conflicts of interest have been focused on earnings forecasts accuracy and biases in stock recommendations.[9] There is little extant research on how analysts' conflicts of interest affect the timeliness of information that they provide. Exceptions are O'Brien, McNichols, and Lin (2005), who document that affiliated analysts downgrade significantly more slowly than unaffiliated analysts after IPOs and SEOs. However, there are no particular private information events following equity issuance in their study. In contrast, the class periods of

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[9] We find that, in general, independent analysts provide the least optimistic recommendations, followed by dependent analysts whose recommendations tend to be closer to those of affiliated analysts, i.e., the most optimistic group. This pattern is consistent with our reciprocal pressure hypothesis. However, it exists in both sued and matched samples. Therefore, we do not pursue the analysis along this dimension, instead we focus on the changes of recommendations and the timeliness of downgrades.

securities lawsuits provide unique windows of corporate wrongdoings when the public is not aware of such actions. Without the class periods, it is very challenging to examine timeliness of recommendations in a meaningful way. We also believe that it is essential to examine how timely analysts disseminate *negative private* information, since this channel will greatly contribute to a fair and efficient market.

Finally, the analysis surrounding the adoption of NASD Rule 2711 enhances the understanding on the effects of this rule. For example, Chen and Chen (2009) focus on the improvement of stock recommendations reflecting firms' fundamental values. Kadan et al. (2009) document the declining optimism of affiliated analysts and a massive shift of rating mechanisms from a five tier to a three tier system surrounding the summer of 2002, which also reduces the quality of information. Therefore, the lower optimism documented by prior literature can also be driven by this loss of information. Our analysis examining the promptness of analysts in disseminating unfavorable information is less susceptible to the shift in rating mechanisms.

The remainder of the paper is organized as follows. In Section 2, we elaborate on our testable hypotheses. Section 3 describes the data and variables. Section 4 presents the empirical results, and section 5 concludes the paper.

2.   Hypothesis Development and Refining the Classification of Analysts

Different from the prior literature that has been focused on how receiving underwriting fees from client firms imposes a conflict of interest problem for investment banks, we investigate an under-researched factor—reciprocity among financial institutions—and its economic consequences. Studies of reciprocity in economic literature are fairly recent compared to the standard economic theory focuses on the self-interest of agents. This line of research was originated by Rabin (1993), who observes that people tend to behave reciprocally, i.e., we return

kindness to kindness and retaliate when we were hurt even if it is costly to do so. Rabin's work was the first that made the notion of reciprocity precise, which he referred to as "fairness." Fehr and Schmidt (2003) provide an excellent summary in this area, comparing the differences of theoretical approaches in great detail and summarizing experimental work.

We are interested in whether the behavior of reciprocity exists in the securities underwriting business and to what extent this force affects information production in this market. The securities underwriting business is characterized by different types of syndication relationships, which can potentially suffer from different degrees of reciprocal pressure on analyst recommendations. We identify two types of banks that have worked with the underwriters (main bank in Figure 1) of a covered firm (firm A in Figure 1) but do not serve covered firm in securities underwriting.

The first type is "co-lead syndicate banks," which have arranged deals with main banks as joint book managers, i.e., co-leading the deals. These banks are also prominent underwriters and have the capacity to organize their own syndicates. The second type is "co-manager syndicate banks," which only play a junior role and have to rely on main banks to be included in syndicates. This difference is crucial for distinguishing the levels of reciprocal pressure on analyst reports because if there is a pressure of being reciprocal, co-lead syndicate banks can reciprocate by deal opportunities but co-manager syndicate banks cannot, which gives rise to the possibility that co-manager syndicate banks have to reciprocate in other dimensions of business. We propose that stock reports can be the dimension that co-manager syndicate banks give in. Therefore, we label the analysts affiliated with co-manager syndicate banks as "dependent" analysts. Those affiliated with co-lead syndicate banks remain as independent analysts.

We also separate main banks, the underwriters of covered firms, into lead manager and co-manager to serve as a confirmation that banks with similar levels of conflict of interest should behave similarly due to fees collected through direct underwriting relationships with covered firms. Therefore, their analysts are defined as affiliated analysts as in the prior literature (see, for example. Lin and McNichols (1998)).

Finally, independent banks/research firms (labelled as independent group) are those have neither syndication relationships with main banks nor direct underwriting relationships with covered firms. These independent analysts have the least potential for conflict of interest, so we expect them to give the most accurate recommendations. We note that in the prior literature, analysts of syndicate banks and independent banks/research firms were generally combined in one category. In sum, the three types of analysts are: (1) affiliated analysts (those working for either lead manager or co-manager main banks); (2) dependent analysts (those working for co-manager syndicate banks); (3) independent analysts (those working for co-lead syndicate banks or independent group).[10]

More specifically, we focus on co-manager syndicate banks and their potential reciprocal correspondences with main banks in stock recommendations. Ljungqvist, Marston, and Wilhelm (2009, hereafter LMW) suggest that aggressively optimistic research attracts co-management appointments, which in turn significantly increases a bank's chances of winning lead-management mandates in the future. Syndicate banks that desire to gain or maintain access to participate in a syndicate as co-managers, could be coerced to behave similarly as their network partner (e.g., a main bank), withholding negative information from the public longer than

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[10] If there are both co-lead and co-manager relationships between a pair of banks, we allow the co-lead relationship to dominate the co-manager relationship. By the same token, we allow the main bank relationship to dominate the syndicate bank relationship.

independent group that are not subject to reciprocal pressure. We evaluate the extent to which analysts from syndicate banks behave in this fashion, which we refer to as the "reciprocal pressure" hypothesis.

In addition, we consider the possibility that co-lead syndicate banks are less prone to reciprocal pressure, since they are lead banks and have the power and capacity to organize syndicates on their own. On the contrary, co-manager syndicate banks are more subject to reciprocal pressure because their access to syndicate participation is mostly dependent on invitations by other lead banks. Appendix A2 presents the average market share of different types of banks as lead underwriters in securities underwriting during our sample period for each type of banks. Co-manager syndicate banks with average equity market share of merely 0.15% is much smaller than co-lead syndicate banks whose average equity market share is above 7%. To ensure that our findings are not driven by the size of banks, potentially because of their market power, we control for bank market shares in the statistical analysis we present below.

The pool of co-managers has increased dramatically over time. LMW (2009) document that, in 1970, the average lead bank in equity deals had only 5.3 unique co-management partners. In contrast, the number of unique co-management partners increases sharply during the 1990s to 46.3 in 2000. Debt syndicates followed a similar pattern. LMW (2009) conclude that the rising number of co-management opportunities appears to represent a fundamental change in exclusivity rather than the persistence of a small number of strong syndication relationships alongside a large number of incidental partnerships. The change in market structure indicates intense competition among the co-managers that cannot organize their own syndicates. Such a competitive environment is likely to erode the independence of these co-managers from main

banks.[11] Therefore, co-manager syndicate banks are likely to be dependent on main banks to be able to participate in syndicates, suggesting that they are subject to reciprocal pressure, thus, a potential type of dependent analyst is one who works for a co-manager syndicate bank.

3.   Data, Variables, and Descriptive Statistics

We keep all banks with underwriting business and independent research firms in our initial sample. We do so because we intend to examine the analyst behavior of a broader range of brokers, not only those that have a direct underwriting relationship but also those that are under reciprocal pressure. This approach is different from, for example, LMW (2009), who focus on the top 50 banks since their events of interest are securities underwriting. The task requires hand-matching firms from different databases, hand-searching mergers and acquisitions among financial institutions, and hand-matching institutions from various databases at the right point in time due to frequent changes of ownerships among institutions. The data used in this paper are drawn from eight data sources:

(1) The I/B/E/S database of stock recommendations, which provides analyst and brokerage firm information.

(2) The website of Stanford Securities Class Action Clearinghouse in cooperation with Cornerstone Research, which posts federal securities fraud class action lawsuits.

(3) The Thomson Financial/SDC Platinum database of U.S. domestic securities offerings, from which we obtain firm securities issuance history, underwriter characteristics and syndication relationship.

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[11] It would be interesting to examine reciprocal pressure during the earlier period and see whether the changing co-manager landscape affects this pressure, however, the data for stock recommendations in IBES database prior to 1993 are not available.

(4) The Thomson Reuters LPC's DealScan database of loans, which we use to construct loan market shares of broker affiliated parent holding companies and lending relationships between affiliated banks and recommended firms.

(5) The Thomson Financial/Spectrum 13f database of institutional holdings, from which we find equity ownership of broker affiliated institutions and overall institutional holdings of recommended firms.

(6) The Thomson Financial/SDC Platinum database of mergers and acquisitions, which we use to identify the mergers and acquisitions among financial institutions that have effects on our data construction. The effective merger date is used to link institutions from the above databases. We consider two institutions as one integrated organization during the year of merger. We disconnect the ties for the institutions spun off from the parent companies during the year of such transactions. We also search company information, such as their websites, annual report, Hoover's Online, Corporate Affiliates, etc., to identify the history of institutions.

(7) Center for Research in Security Prices (CRSP) for shares outstanding and stock price information.

(8) COMPUSTAT for firm characteristics.

3.1.   Sample selection

3.1.1. Sued firms

In 1995, Congress passed the Private Securities Litigation Reform Act (PSLRA) to discourage frivolous lawsuits. In order to keep the sample within the same regulatory regime, our sample spans the years from 1996 to 2006.[12] We end our analysis in 2006 because IBES stopped

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[12] Evidence supporting PSLRA discourages frivolous securities fraud litigation is provided by Johnson, Nelson, and Pritchard (2007). See also Shivdasani and Song (2011) for the merit of using lawsuits as a proxy of client quality.

providing the broker translations file. As a result, we cannot match analyst characteristics obtained from the historical earnings forecasts file to stock recommendations. We find that only 30% of analysts in the old broker translations file obtained in 2006 can be matched to recently downloaded IBES dataset. The mismatching is consistent with the data revision issue raised by Ljungqvist, Malloy, and Marston (2009).

We start the sample construction by using 1600 securities class action lawsuits. Among them, we identify 706 unique firms (associated with 748 lawsuits) that have main banks, i.e., securities issuance activities, within three years prior to the class period starting dates.[13] There are a small number of firms that have more than one lawsuit during our sample period. We focus on the first lawsuits in our analysis. However, the results remain robust to the inclusion of all lawsuits for each firm.

A unique advantage of using securities class action lawsuits to study the timeliness of analyst recommendations is that the lawsuit filings provide several critical dates. Figure 2 provides a time lines of these dates. The first one is the class period starting date, which specifies when the alleged wrongdoing starts. The second one is the class period ending date, which specifies when the wrongdoing ends. It may be the time at which the wrongdoing is uncovered.[14] The average (median) number of days during the class period is 388 (296). Therefore, the class period represents a uniquely defined window to examine an analyst's ability and incentive to detect a firm's fraudulent behavior prior to the bad news becoming public.

The nature of a negative event also facilitates the study of various degrees of conflicts of interest better than a positive event. We also identify the date that the value of a buy and hold

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[13] We exclude security frauds that involve wrong-doings of agents of the firm or investor, rather than that of the firm management.

[14] We understand that the class period might not be a perfect proxy for the event window. Therefore we examine the robustness of the results using various event windows in Section 4.3.3. The results remain qualitatively the same.

investment strategy starting on the class period starting date reaches its highest point during the class period. As shown in Figure 2, the mean (median) number of days from the beginning of the class period to the maximum value date is 140 (77). Finally, on average, the lawsuits are filed 123 day (or 37 days in median) after the class period ending date.

Figure 2 and Table 1 present results on the wealth change of investing in sued firms during the class action period. Based on a sample of the first lawsuits only, every dollar invested in sued firms at the beginning of the class period up to two days prior to the end of the class period drops 13% to 87 cents on average. However, before it drops to 87 cents, the average value climbs to $1.63 because sued firms might actively cook their books or disseminate overly optimistic information about the prospects of the firms. Therefore, the maximum buy and hold value (BH value) marks the point of declining stock values of sued firms after initial run ups. If an investor purchases the stock at the maximum point, by two days prior to the wrongdoing being uncovered, the investor has lost about half of the investment's value. Furthermore, the stock price of sued firms drops another 21% during the three-day event window surrounding the class period ending date. The value continues to drop an additional 12% between one day following the class period ending date and two days prior to the lawsuit filing. Finally, it drops another 5% during the three-day window surrounding the lawsuit filing date. The total average wealth loss is 43% from the beginning of the class period to one day following the lawsuit filing. On the other hand, both concurrent value- and equally-weighted indexes and all matched samples (not reported) show positive gains in value. In summary, investors of sued firms experience tremendous wealth losses during the class action period.

3.1.2. Matched non-sued firms

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In order to investigate the uniqueness of analyst behaviors associated with the negative information event, e.g., the class action lawsuit, we identify benchmark non-sued firms in the same industries as the sued firms classified by their two-digit SIC codes. We employ three procedures to construct matched samples using information during the fiscal year prior to the class period starting date. The first method chooses non-sued firms having the same types of main bank analysts as those of sued firms followed by the closest total assets, then the closest number of analysts. The second procedure chooses non-sued firms with the closest total assets then the closest number of analysts. The third method is similar to the second one but reverses the criterion by matching with the number of analysts and then total assets.

Sued firms appear to be larger in total assets, have more analysts, and more recommendations than those non-sued firms in any matched sample. Because the third matched sample produces the most comparable size, market value of equity, number of analysts, and number of recommendations, we report results based on this matching procedure. However, our results are robust regardless of which matched sample is used. It is normal to have fewer recommendations for matched sample because, by definition, they have fewer information events than sued firms.

3.2. *Syndication relationship*

As illustrated in Figure 1 and Panel A of Appendix A1, we define main banks as those that have underwritten securities (either debt or equity) offerings within three years prior to issuing recommendations with respect to a client. In this paper, we distinguish main banks' roles as lead managers or co-managers in the syndicates of securities issues.[15] Analysts of these main banks are typically called affiliated analysts in the extant literature. Furthermore, among banks

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[15] Corwin and Schultz (2005) show that the allocation of proceeds to a co-manager is about half of that to a lead manager (i.e., book manager or book runner) percentagewise.

with unaffiliated analysts, we identify an indirect connection between a bank and a covered firm via a syndication relationship between this bank and the covered firm's main bank. There are two types of syndication relationships: (1) lead and lead manager, i.e., co-lead syndication and (2) lead manager and co-manager, i.e., co-manager syndication, where the first role is for the main bank and the second role is for a syndicate bank. For example, when JP Morgan is a main bank of Firm A, it can also lead managed many deals with other underwriters participating as lead or co-managers. If those underwriters have not served Firm A in securities issuance within the three calendar years prior to the recommendation date, they are syndicate banks of JP Morgan with respect to Firm A.[16] To discern among syndicate banks' capacity to organize syndicates, we remove the co-manager syndicate banks that have more than or equal to 1% of market shares as book managers per year in either bond or equity underwriting. We also remove among the co-lead syndicate banks that have less than 1% of market shares as book managers per year in both bond and equity underwriting.[17] Such a differentiation between the two types of syndicate banks provide sharper tests to capture the difference in reciprocal pressure facing between co-lead and co-manager syndicate banks.

When a bank did not serve Firm A in the underwriting business (either debt or equity) and had no syndication relationship with any of its main banks within the previous three calendar

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[16] The extent of joint lead relationships increased dramatically during 1990-2005. In 1990, there is only one pair of joint lead managers in bond underwriting among the top ten underwriters. In 2005, almost all top ten banks paired up as joint lead managers in underwriting deals. The prevalence of co-lead syndication relationship among top underwriters suggests that most of the co-manager syndicate banks and independent banks are smaller banks.

[17] In Online-appendix B1, we alternatively define co-lead and co-manager syndicate banks without requiring their market shares being greater or equal to 1% and less than 1% of market shares as book managers per year, respectively. As expected, the differences between estimated coefficients on co-lead and co-manager syndicate banks become marginally significant. Nevertheless, co-lead syndicate banks still issue downgrades significantly earlier than main banks but co-manager syndicate banks do not.

years, it is classified as an independent group, including both independent banks and independent research firms without an underwriting business.[18]

The finer categorization of bank types in this paper is a unique departure from other studies in this area. In particular, we further split the *unaffiliated* analysts into two types—*dependent* analysts working for co-manager syndicate banks, and *independent* analysts working for co-lead syndicate banks or independent group, which allow us to examine the reciprocal pressure hypothesis.

3.3. *Stock recommendation and timeliness*

Because of the unique nature of lawsuits, which provides key event dates, we focus on the timeliness of recommendation revisions after the class period starting date (wrongdoing starting date). Analyzing recommendation revisions allows us to restrict the comparison to be within firms and analysts.

In Table 2, we provide a sample distribution of all the analyst recommendation revisions issued by year and within two years prior to the class period (serving as benchmark period) and from the class period to the lawsuit filing date (information production, i.e., sample period). During the benchmark period (two years prior to the class period), sued firms have a comparable number of revisions to matched firms, 6473 (48.6%) versus 6841 (51.4%). In contrast, sued firms have many more recommendation revisions (61.7%) during the class period than matched firms (38.3%), which is consistent with the nature of the class period being information intensive. The percentage of recommendations issued for sued firms further increased to 74% following the class period and prior to the lawsuit filing date.

3.4. *Explanatory Variables*

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[18] Online-appendix B2 shows that there is no significant difference in the effect of timeliness in downgrades between independent banks and independent research firms, so we group them together.

As we examine the timeliness of analyst recommendations in multiple regressions, we control for many analyst-specific, bank-specific, and firm-specific variables. In this section, we discuss how we construct these independent variables. Detailed variable definitions are provided in Panel B of Appendix A1.

3.4.1. *Analyst characteristics*

We construct three variables measuring the reputation-related career concerns of analysts. The first is based on the annual Institutional Investor All-America Research Team ranking. We define an all-star dummy variable that equals one if the analyst was an all-star (i.e., ranked as a top-three or a runner-up analyst in her industry) in the year prior to making the recommendation, and zero otherwise. The second measure is the analyst's seniority taken as the number of years since her first appearance in the IBES earnings forecasts and recommendation databases. Hong, Kubik, and Solomon (2000) show that senior analysts are more likely to provide bold earnings forecasts and herd less. The third variable is analyst forecast accuracy as in Hong and Kubik (2003). Assuming that analyst reputation partly derives from forecasting ability, forecast accuracy should be a good proxy for analyst reputation. However, this variable is not available for many observations. Therefore, we do not use it in our main analysis. Instead we include it in robustness tests in Section 4.3 and report the results in Online-appendix B3.

As shown in Table 3, sued firms have significantly more senior and all-star analysts than matched non-sued firms during the benchmark period but not the sample period. In fact, during the sample period, sued firms are followed by fewer all-star analysts than the matched sample. Further analysis indicates that this result is not because all-star analysts dropped the sued firms or they lost their all-star status. Instead we find that some non-all-star analysts who did not provide revisions during the benchmark period did offer revisions during the class period, which

in turn drives down the percent of all-star analyst presence in the sample period. There are no significant differences in relative forecast accuracy between these two types of firms regardless of period.

3.4.2. *Bank pressure proxies*

We employ several "bank pressure proxies" that measure the amount of pressure an analyst might face to offer an inflated recommendation or to postpone the dissemination of negative firm prospects. The more lucrative the client, the more tempted is the analyst to inflate or to postpone the recommendation, since the benefit of liquidating reputation capital will be greater. We follow LMW (2006) and construct a loyalty index, which, for each bank, measures how often it retains its clients in consecutive equity or debt deals, divided by the number of clients. Because we include many smaller banks that did not underwrite any deals in a particular year, thereby resulting in a missing value in loyalty index, we include a dummy variable for the missing values so as to differentiate them from observations without retained clients. Table 3 shows that sued firms' banks have a significantly higher level of client loyalty than matched firms' banks in the bond market prior to the class period, but a lower level of client loyalty in the equity market during the sample period.

We do not calculate the fee pressure measure proposed by LMW (2006) because our event of interest is lawsuit filing with a broader sample of banks. In addition, fee information is not available for many deals, resulting in too many missing observations. However, we calculate a similar measure—the firm's share of a bank's debt or equity deals during the prior five calendar years. The higher the firm's share, the more important the client is. These measures should be highly positively correlated to fee incomes from this client. Table 3 shows no significant differences in these measures between sued and non-sued firms prior to class periods.

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We also control for the firm's overall debt and equity issuing amounts over the 5-year horizon. Sued firms issued more debt but less equity than matched firms during the benchmark period, but less debt during the sample period.

Since the size of the potential pool of "side payments" bankers used to gain analyst cooperation might change over time, we follow LMW (2006) and control for this effect by computing the percentage difference in market-wide proceeds raised during the current quarter and a 5-year quarterly moving average.

3.4.3. *Equity ownership by investment banks*

We calculate the fraction of a firm's equity directly owned by an investment bank whose analyst(s) provide coverage for the firm. Ownership data are obtained from the Thomson Financial/Spectrum 13f database. Table 3 shows that sued firms have a significantly higher level of institutional ownership than matched non-sued firms during the benchmark period, but it is not significantly different between firm types during the sample period. Sued firms have a greater likelihood of banks holding their equity during both the benchmark period and the sample period.

3.4.4. *Bank reputation in underwriting and lending*

We control for bank reputation by using their market shares in the debt, equity, and loan markets during the prior calendar year.[19] We only consider their roles as lead managers. When the deals are lead managed by multiple banks, we allocated the dollar amounts equally among participant banks. Each bank's deal amounts are aggregated then divided by the total market amounts during the calendar year prior to stock recommendations. We find no significant differences between sued and matched firms during the sample period. However, matched firms

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[19] See Chemmanur and Fulghieri (1994) for a theoretical model on the effect of investment bank reputation on issuer quality screening and securities pricing.

tend to have fewer reputable underwriters than sued firms in both debt and equity markets prior to the sample period.

3.4.5. *Firm characteristics*

The timeliness of analyst revision is likely to be affected by firms' general information environment. In particular, larger firms or firms with lower information opacity will facilitate research analysts to provide timelier information to the market. We include three firm-specific variables (Firm size, Tobin's Q, and information opacity) to capture the cross-sectional difference in the information environment. Firm size is measured by total assets. Tobin's Q is computed as total assets minus book value of equity plus market value of equity, divided by total assets. Following Kim and Verrecchia (2001), we compute a proxy for information opacity as the logarithm of the beta coefficient of trading volume in the regression,

$$Ln\left|\frac{P_t - P_{t+1}}{P_{t+1}}\right| = \beta_0 + \beta_1\left(VOL_t - AVGVOL\right) + \varepsilon, \tag{1}$$

where,

P_t: daily stock closing price,

VOL_t: daily trading volume of the stock in thousands of shares,

$AVGVOL$: the average daily stock trading volume within the last 6-month (we use 182 days) in thousands of shares.

Kim and Verrecchia (2001) posit that, when the firm discloses more information, market makers rely on the disclosure itself, rather than on alternative sources of information about firm value, such as volume. Thus, as the firm commits to report information in a timely fashion, stock returns are less likely to be associated with trading volume. This predicts that firms with poor disclosure (or greater information opacity) should have a larger slope coefficient on trading volume (β_1). As shown in Table 3, sued firms are significantly larger and have a higher Tobin's

Q than non-sued firms during both periods. Information opacity in sued firms is greater than that in non-sued firms.

4. Empirical Results

4.1. Univariate Analysis of Stock Recommendations

Table 4 reports summary statistics on the level, change, and timeliness of analyst recommendations of sued firms and their matched non-sued firms issued by various types of analysts/banks. Based on all analyst revisions (upgrade, downgrade, and no change) from the class period starting date to the lawsuit filing date as shown in the left side of Panel A, we find that the analysts working for syndicate banks and independent group provide less favorable prior and current recommendations than the affiliated analysts working for main banks, with the co-lead syndicate banks being the least optimistic. Since the prior recommendations of syndicate banks and independent group are significantly lower than those of main banks, it rules out the possibility that the subsequent downgrades issued by these unaffiliated banks are driven by adjusting their opinions of sued firms towards consensus.

To investigate how timely different types of analysts update information regarding the firms being sued after wrongdoing occurs, we construct a timeliness variable, "scaled # days" as the number of days between the date of the current revision and the class period starting date divided by the duration of the class period multiplied by 100.[20] Revisions by independent group

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[20] We scale the number of days it takes an analyst to revise her recommendation using the class period duration because firms being sued experienced various class period lengths. Thus "scaled # days" provides a uniform measure of timeliness across sued firms with different lengths of class periods, just like researchers always scale debt by total assets. For example, if an analyst provides a downgrade 100 days after the class period starting date and the length of the firm's class period is 100 days, then this downgrade is not informative because it coincides with the class period ending date when the wrongdoing has been uncovered by the public. On the other hand, if the class period of a firm is 200 days, then this downgrade would have been 100 days earlier than the time the public is aware of the fraud. Therefore, the unscaled number of days itself cannot capture the timeliness that we intend to capture in the study.

come significantly earlier than main banks. While co-manager syndicate banks appear marginally later than lead main banks, they are not different from co-manager main banks.

Next we turn to the right side of Panel A, in which we only focus on downgrade revisions, thereby examining how soon different types of banks disclose negative information about the sued firms. Based on this scaled measure, independent group provide the earliest downgrades for sued firms, followed by co-lead syndicate banks. Co-manager syndicate banks (being classified as unaffiliated in the analyst literature but as dependent in this paper), in contrast, provide downgrades no sooner than main banks. This result is consistent with our reciprocal pressure hypothesis. The difference in timeliness between the two types of syndicated banks reflects different degrees of reciprocal pressure. Co-lead syndicate banks are less sensitive to this pressure than co-manager syndicate banks because they have the capacity to organize and lead their own syndicates.

To address the concern that the timing pattern above is not unique to our event of interest, we conduct the same analysis for matched non-sued firms as shown in Panel B of Table 4. We did not label analyst type like Panel A for brevity. Independent group and co-manager syndicate banks offer revisions and downgrades significantly sooner than main banks. In fact, both types of banks are adjusting their opinions to be more in line with co-lead syndicate banks, which has been least optimistic during all periods. Nevertheless, as we discuss in section 4.2, there are no significant differences in the timing of downgrades among bank types for matched firms in multiple regressions where we control for prior recommendations, firm-, analyst-, and bank-specific variables.

4.2. Multivariate Analysis of Downgrade Timeliness

To examine the incentives to provide timely information during the class period, we test the effect of different bank types on the scaled numbers of days between the class period starting dates and the dates when revisions are provided. In the multivariate framework as shown in Table 5, we regression "Scaled # days" on firm-, analyst-, and bank-specific variables that affect the timeliness of information production. In particular, we control for current stock price using Tobin's Q because the absolute price level does not have a clear meaning. We also control for the level of prior recommendation. Both current stock price and prior recommendation are likely determinants of the extent and timeliness of revisions. Further, we include broker fixed effects. We include four bank dummy variables in the regressions. As a result, the average effect of lead manager main bank is reflected in the intercept. More negative estimates on bank type dummies indicate that the revisions are issued earlier relative to lead manager main banks.

As shown in column (1) of Table 5, we track all downgrades from the class period starting date to the lawsuit filing date. We go beyond the class period ending date (i.e., wrongdoings are known to the public) since some analysts do not provide any downgrades until after the class period ending date. Excluding such analysts may bias our analysis of the timeliness of downgrades because it does not allow many followers to be included in the analysis. We find the same rank order as that of the univariate tests in Table 4. Independent analysts (those work for independent group and co-lead syndicate banks) issue downgrades earlier than affiliated analysts (those work for main banks) by about 26% of the duration of the class period, though the coefficient estimate is statistically significant only for co-lead syndicate banks.[21] Given that the median class period is 296 days, they are equivalent to providing downgrades earlier by 77 days. Because the F-tests of the differences between the estimated

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[21] Online-appendix B1 shows that the estimated coefficient on independent group is less significant partly because of the inclusion of broker fixed effects.

coefficients of co-lead syndicate banks and those of independent group are insignificant regardless of model specifications, for brevity, we do not report them. Although they are defined as unaffiliated analysts in the extant literature, dependent analysts (those work for co-manager syndicate banks) behave no differently from affiliated analysts in the timeliness of their downgrades. The F-test (at the bottom of Table 5) illustrates that the difference between the estimated coefficients of co-manager syndicate and that of co-lead syndicate banks is significant at the 10% level.

One concern with the above analysis is that the timing pattern may be driven by the fact that some types of banks provide multiple downgrades, thereby resulting in a later timing on average than those banks providing only one downgrade. To address this issue, we focus on the first downgrade provided by each analyst with respect to each sued firm in column (2), and obtain similar results. Co-lead syndicate banks downgrade sued firms significantly earlier than co-manager syndicate banks, the F-test shows that the difference between these two types of banks is significant at the 5% level.

Our findings suggest that co-lead syndicate banks have better incentives to provide timely negative information than lead manager main banks. In contrast, reciprocal pressure appears to be the dominant effect on co-manager syndicate banks. Despite no direct ties to the covered firms, the need to maintain access to syndicates reduces their incentives to produce timely negative information during the class period. As a confirmation, co-manager main banks, which also collect fees from the covered firms, indeed behave like their lead managers.

As for the control variables, higher prior recommendations are associated with significantly more prompt downgrade revisions in the first regression. Although the sign is negative for the all-star dummy, it is not significant. A sued firm that raises more capital in the

debt or equity markets has a significantly higher tendency to receive later downgrades because the conflicts of interest problem is more severe. If banks also hold equity of sued firms, downgrades are issued significantly later than those without equity ownership. Firms involved in IPO allocation lawsuits also have significantly delayed first downgrades. The coefficient estimate on Tobin's Q suggests that firms with inflated prices are downgraded significantly earlier.[22]

4.3. Robustness Tests of Timeliness and Survival Analysis

4.3.1. Matched Non-sued Firms

One concern with our results is that the main bank, syndicate (co-lead and co-manager) banks, and independent group may all be fundamentally different banks other than their relationship to the covered firm. To address the concern, we conduct a placebo test by repeating columns (1) and (2) in Table 5 using our matched non-sued sample. Results are reported in columns (3) and (4). In general, there is little significant difference in the timeliness of downgrade revisions among different types of banks, suggesting that our findings are unique to firms being sued where differential incentive of providing negative information is relevant. One exception is that independent group provides downgrades *later* than lead manager main banks in the matched sample corresponding to column (4) in Table 5, and result is marginally significant at the 10% level. This result could be driven by the fact that independent group do not have as much timely information as those lead manager main banks that have a close connection with the covered firms via the underwriting relationship.

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[22] We report the reasons (not mutually exclusive) for filing lawsuits in Appendix A3. Among the 748 lawsuits, 145 are related to initial public offering (IPO) allocation and tie-in agreements between investment bankers and institutional investors. Presumably, analysts affiliated with these investment banks may have more pressure to withhold negative information about these IPO firms. However, we do not find significant results using this subsample (results not reported). The most popular reason for filing a lawsuit, about 86% of the cases, is that the firms artificially inflated securities prices during the class period. Therefore, stock prices typically increase substantially then decline gradually during the class period.

4.3.2. Excluding Potential Frivolous Lawsuits

To address the concern that our results might be driven by frivolous lawsuits, we follow Dyck, Morse, Zingales (2010) and apply additional filters as we construct the sample: excluding all cases where the judicial review process leads to their dismissal, only including those firms where the settlement is at least $3 million and different asset size restrictions. These results are reported in the Online-appendix B4. In the first regression, we repeat the regression column (1) in Table 5 using the subsample of lawsuits with additional filters described above but without imposing any asset size restriction. In the second (third) regression, we impose asset size restriction to be at least $750 ($500) million. The coefficient estimates remain robust regardless of the asset size restrictions. These results suggest that our main findings are not driven by frivolous lawsuits.

4.3.3. Alternative Event Windows for Information Production

This study focuses on analysts' private information production about corporate fraud. One concern is that class period starting date might not be the exact date when misconduct occurs, and class period ending date might not represent the first date corporate fraud is revealed to the public. We admit that the class period is not a perfect proxy for the event window, but it is a reasonable proxy due to the following reasons.

First of all, class period is defined legally as the time period during which the plaintiffs held the stock and during which the illegal activity took place. It is the time period in which possible money loss occurred due to the illegal actions of those being accused in the securities class action. Dyck, Morse, and Zingales (2010) also use the class period starting date as the main misconduct starting date. They also use class period ending date as the whistle blowing date when the firms themselves reveal the information.

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Although the class period has been used in literature for wrongdoing period (see, for example, Dyck, Morse, and Zingales (2010)), it may suffer from the problem of statute of limitations, i.e., the wrongdoings could actually happen earlier than the class period. To address this concern, we analyze 45 cases of our sample firms that are also subject to the SEC enforcement actions during September, 1995 to March, 2004. We find 10 cases having the class period beginning dates earlier than the SEC wrongdoing beginning dates and 12 cases the SEC dates are earlier but the differences are within one year. For the remaining 23 cases that SEC beginning dates are earlier for more than one year, the SEC wrongdoing *ending* dates are, on average, two years earlier than the class period beginning dates, which suggests that many SEC cases are not for the same wrongdoings, in particular for those ended more than three years ago. Because there are quite a few cases the SEC dates are earlier than the class periods, to address the statute of limitations concern, in the Online-appendix B5, we follow Dyck, Morse, and Zingales (2010) and start the event period three months prior to the class period beginning date. Online-appendix B5 column (1) shows that our results still hold. In column (2), we extend the wrongdoing beginning date back by six months prior to the class period beginning date, and find that our main results remain robust.

Secondly, we downloaded the Internet Appendix of Dyck, Morse, and Zingales (2010) and compared the class period ending date to the whistle blowing dates they hand collected. On average, the whistle blowing date is 30 days *later* than the class period ending date, and the median difference between the two dates is zero. The staleness of initial revelation date is also discussed in Karpoff, Koester, Lee, and Martin (2012). They report that, the median information revelation date is 23 days earlier than the lawsuit filing dates. However, the median class period ending date is 37 days earlier than the lawsuit filing date in our sample. Therefore, the revelation

dates we use are indeed slightly earlier than those reported in Karpoff et al. (2012). To address

the concern that the public might be aware of the misconducts before lawsuit filing dates, we

examine analyst recommendations up to one day following the class period ending date, which

indeed is still half month earlier than that reported by Karpoff et al. (2012). The main results

remain robust as reported in column (3) with broker fixed effects and column (4) without broker

fixed effects in the Online-appendix B5. As a last defense, even if the class period beginning date

is not a reliable indicator of when the misconduct began, unless this imprecision is systematically

correlated with the timeliness of a particular type of analysts, this imprecision will be random

noise and bias against us to find any significant results.

4.3.4. Survival Analysis

As an additional robustness check, we conduct the survival analysis that is also known as

duration model. We report the estimated coefficients of Cox semi-parametric proportional

hazards model in Table 6 for both sued and non-sued matched samples. The sued sample

includes recommendations issued during the period between the class period starting date and the

lawsuit filing date. For each firm-analyst pair, only the first downgrade is included and classified

as the failure event. For those do not provide any downgrades during our event window, they are

included as the non-failure event. The dependent variable is whether a bank provides a

downgrade for a firm at the time that such an action occurs. Our main findings remain robust. In

column (1) of the sued sample, the significant positive coefficient estimates on independent

analysts working for co-lead syndicate bank or independent group indicate that analysts

employed by both types of banks have a higher probability of downgrading sued firms than those

employed by main banks at a given time prior to the lawsuit filing date. The coefficient estimate

on co-manager syndicate bank dummy (i.e., dependent analyst) is insignificant, suggesting no

difference in the promptness of downgrades between co-manager syndicate banks and main banks. Nevertheless, such patterns do not exist among matched non-sued firms as shown in column (2) in Table 6.

We graph the failure functions in Figure 3, which shows that more than (about) 25% of independent (co-lead syndicate) banks have downgraded sued firms around scaled 50 days following the class period starting date (t=0), i.e., mid-class period. However, at the same time, much less than 25% of main banks downgrades sued firms, which translates into longer time to downgrade sued firms among main banks compared to co-lead syndicate banks and independent group. Figure 3 also shows that the order of timeliness among analysts from the most prompt to the least coincides with our prediction of the reciprocal pressure hypothesis – independent group followed by co-lead syndicate banks, then co-manager syndicate banks, and finally, the main banks. Surrounding class period ending date (t=100) – when the public learns about sued firms' wrongdoings – there is a jump of downgrades by all types of banks.

4.3.5. Additional Robustness Tests

Lastly, we conduct a list of robustness tests and obtain essentially the same significant results. The estimates are similar for the samples excluding financial firms, including all lawsuits, excluding brokers not engaged in the underwriting business, and including relative forecast error. Due to concerns over the objectivity of analyst recommendations and the Global Settlement, major banks changed their rating system in the summer of 2002. To address this issue, we drop observations from August 1, 2002 to July 31, 2003, and the main findings remain robust. Robustness tests including relative forecast error are reported in the Online-appendix B3, but other results for the robustness check are not reported for brevity.

4.4. The Effect of Rule 2711 on Downgrade Timeliness of Sued Firms

Chen and Chen (2009) examine the extent to which analysts incorporate intrinsic value estimates relative to stock prices surrounding Rule 2711 and find that analysts' independence improved. In this section, we will examine the effect of Rule 2711 on improving analyst independence in our context of the timeliness of revealing negative information. It is possible that the rule may not enhance the incentive to reveal negative information although, on average, the intrinsic value estimates relative to stock prices demonstrate a stronger relation to stock recommendations as shown in Chen and Chen (2009).

In Table 7, we split the sample of downgrades of sued firms from the class period starting date to the lawsuit filing date into two subsamples: those prior to the implementation of Rule 2711 on August 1, 2002 and those after August 1, 2002. Regression results for the timeliness of downgrades before and after Rule 2711 are reported in regressions (1) and (2), respectively. Following Rule 2711, lead manager main banks as well as other types of banks become more prompt in providing negative information than before the Rule. For example, prior to Rule 2711, lead manager main banks provide first downgrades 198.91 scaled days after wrongdoings occurred, but issue downgrades much earlier (61.75 scaled days) following Rule 2711. In addition, F-tests (at the bottom of Table 7) show that the difference between co-lead and co-manager syndicate banks is significant prior to Rule 2711 but becomes negligible post Rule 2711. The results indicate that reciprocal pressure is no longer coercing co-manager syndicate banks as much as prior to Rule 2711.

We formally test the differences of each type of banks before and after Rule 2711 in regressions (3) by including a Post-rule 2711 dummy variable and its interaction terms with all the right-hand-side variables. The coefficient estimates and standard errors of the intercept and right-hand-side variables are reported in Column (3), and those of the interaction terms are

reported in Column (4) with the label "*Post-rule 2711". The Post-rule 2711 dummy is significantly negative and its interactions with all other bank types are insignificant in the regression, suggesting that all banks become more prompt in providing downgrades. Finally, consistent with the subsample analysis, F-tests of the coefficient estimates on the two types of syndicate banks (reported at the bottom of Table 7, Column (3)) indicate that co-manager syndicate banks, which are more susceptible to pressure from main banks, are less prompt in providing negative information than co-lead syndicate banks prior to Rule 2711. The coefficient on the interaction term between a bank type dummy and Post-rule 2711 dummy reported in Column (4) formally examines the improvement of the bank type relative to lead-manager main banks. The F-test in Column (4) comparing the relative improvement between the two types of syndicate banks is significantly different suggesting that co-manager syndicate banks improve significantly more than co-lead syndicate banks, which has less room to improve in the first place. These coefficients indicate that the net improvement of co-manager syndicate banks relative to co-lead syndicate banks is about 25 scaled days (-5.08 – 20.04). These results suggest that Rule 2711 improves promptness in releasing negative information about their covered firms among analysts of all types of banks, mitigating the problems of conflict of interest as well as reciprocal pressure.

4.5. Deviation from Expected Social Reciprocity

In the previous sections, we find that dependent analysts (those working for co-manager syndicate banks) tend to delay issuing downgrade revisions as much as affiliated analysts (those working for the main banks) on firms sued for financial reporting fraud. In contrast, independent analysts (those working for co-lead syndicate banks) are significantly more prompt in providing negative information on sued firms. The results are consistent with the reciprocal pressure

hypothesis that co-manager syndicate banks are usually small and depending on main banks for participating in underwriting syndicates. Co-lead syndicate banks on the contrary are bolder since they are less subject to "reciprocal pressure" because they have the capacity to organize syndicates on their own. If co-manager syndicate banks are expected to be cooperative with main banks, deviation from this expectation can result in retaliation from main banks. In this section, we formally investigate whether providing early downgrades (lack of *Reciprocity*) by analysts from syndicate banks during the class period is associated with fewer future underwriting activities via the main banks. We demonstrate how the underwriting business opportunities provided by a main bank to a syndicate bank evolve when the syndicate bank deviates from expected reciprocity.

We proxy a syndicate bank's future underwriting business via a lead main bank using the syndication relationship between the two banks, which is computed by aggregating dollar amounts of all equity issuances lead-managed by the main bank while the syndicate bank participated as either a co-manager or a co-lead manager, then scaled as a percentage of total dollar amounts of all equity deals lead-managed by the main bank during the same period.[23] Appendix A5 demonstrates a time trend of increased syndication among banks during our sample period, thus we use a difference-in-differences (DID) test to control for the time trend. In the DID tests, the treatment group includes sued firms and the control group includes matched non-sued firms. Specifically, we identify syndicate banks that provided early downgrades prior to main banks during the class periods. We use the date of downgrade by the syndicate banks as the event date. We examine the change in syndication relationship between the syndicate bank

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[23] We only examine equity underwriting because most of co-manager syndicate banks are only equity underwriters as shown in Appendix A4 where the number of underwriters in the bond market is much smaller than that in the equity market.

and a lead manager main bank that provides the first follow-on downgrade. If no lead manager main bank provided downgrades during the class periods, we use the main banks that have the highest underwriting activities (in terms of dollar amount of deals) of the recommended firm. We apply the same procedure for the sued sample and the matched sample.

Table 8 reports the changes in future syndication relationship surrounding early downgrades made by syndicate banks prior to main banks. There are two types of syndicate banks that provide early downgrades: co-manager and co-lead syndicate banks. Co-lead syndicate banks however can serve as co-managers or co-lead managers with main banks in other deals. Syndication relationship in Panel A is computed for the co-manager syndicate banks with early downgrades and their paired main banks covering sued firms. That for matched non-sued firms is calculated the same way. Similarly, syndication relationship in Panel B is computed for the co-lead syndicate banks with early downgrades. We compute syndication relationship separately for other deals when the co-lead syndicate banks serve as co-managers or serve as co-lead managers. We examine the change in syndicate relationship over two time windows – one year surrounding and three years surrounding the early downgrade event date.

If co-manager syndicate banks are expected to cooperate with main banks by withholding downgrades, deviation from this expectation may result in "relatively" fewer invitations to participate in future deals (thereby lower their future syndication relationship with main banks). We find in Panel A that, relative to the matched sample, the mean syndication relationship of the sued sample drops significantly (4.86%) in the year following the downgrades, compared to the year prior to the downgrades. The difference-in-differences for the three-year window shows the same pattern but the result is not statistically significant. The findings suggest that main banks

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appear to retaliate *temporarily* when these co-manager syndicate banks deviates from expected reciprocity via providing early downgrades of main banks' sued clients.

In Panel B, we first examine the change in syndication relationship of co-lead syndicate banks in deals where they are invited as co-managers with the main banks. Because co-lead syndicate banks are more prominent (and larger) than co-manager syndicate banks, it is not surprising that the average syndication relationship is stronger for co-lead syndicate banks (23%) than co-manager syndicate banks (15%) for sued sample in the year prior to the event date. We find none of the difference-in-differences tests is significant, suggesting little evidence of retaliation from the main banks after co-lead syndicate banks issue early downgrades. The co-lead syndicate banks are much larger and are capable of organizing their own syndicates and, in many cases, the main banks may indeed participate in their other deals as co-managers. Therefore, we do not expect retaliation from the main banks, which explains why co-lead syndicate banks are bolder in providing negative recommendations of sued firms in the first place and may continue to downgrade these firms early.

In the second part of Panel B, we examine the change in syndicate relationship of co-lead syndicate banks in deals where they serve as co-lead managers with the main banks. We do not find any evidence of retaliation from the main banks after these syndicate banks issue early downgrades, which is consistent with the fact that co-lead syndicate banks are capable of organizing their own syndicates and in turn the main banks would not retaliate to their lack of reciprocity.

Overall, we document evidence of temporary retaliation towards co-manager syndicate banks by main banks after showing lack of *Reciprocity*. We observe no evidence of such action towards co-lead syndicate banks regardless of their roles as co-managers or co-lead managers.

5.   Conclusions

Prior literature has examined the conflicts of interest problem of affiliated analysts arising from direct underwriter-issuer relationships. In this study, we focus on the effect of reciprocity and the extent to which it affects analysts' behavior in producing negative information. Using a sample of firms sued for alleged financial reporting fraud, we find that dependent analysts, those subject to reciprocal pressure, tend to delay issuing downgrade revisions similar to affiliated analysts. On the other hand, independent analysts, those not susceptible to reciprocal pressure, are significantly more prompt in providing negative information on sued firms. Furthermore, we find evidence of temporary retaliation by major underwriters toward syndicate members with uncooperative dependent analysts.

Finally, we document that the Global Settlement and the adoption of NASD Rule 2711 improve the independence of analysts, particularly among banks subject to conflict of interests due to underwriter-issuer relationships and reciprocal pressure due to syndication relationships. This study suggests that network ties and reciprocity via underwriting syndicates significantly affect analysts' incentive to produce negative information.

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Appendix A1 Variable definitions

Panel A. Definition of bank types

Analyst/Bank type in the paper	Bank type in the extant literature	Definition
Affiliated analyst/Lead manager main bank	Affiliated bank	Banks that have underwritten securities (either debt or equity) offerings as lead managers within three years prior to issuing recommendations with respect to a client.
Affiliated analyst/Co-manager main bank	Affiliated bank	Banks that have underwritten securities (either debt or equity) offerings as co-managers within three years prior to issuing recommendations with respect to a client.
Dependent analyst/Co-manager syndicate bank	Unaffiliated bank	Banks that have not underwritten securities (either debt or equity) offerings within three years with respect to a client, but have syndication relationship(s) as a co-manager with the client's main banks within the prior three years. To better capture banks' incapacity to organize syndicates, we exclude co-manager syndicate banks with lead managing market share greater than or equal to 1% in either bond or equity underwriting.
Independent analyst/Co-lead syndicate bank	Unaffiliated bank	Banks that have not underwritten securities (either debt or equity) offerings within three years with respect to a client, but have syndication relationship(s) as a co-lead manager with the client's main banks within the prior three years. To better capture banks' capacity to organize syndicates, we exclude co-lead syndicate banks with lead managing market share less than 1% in either bond or equity underwriting.
Independent analyst/Independent group	Unaffiliated bank	Including both independent banks that have not served the client in the underwriting business and had no syndication relationship with any of its main banks within the previous three calendar years, as well as independent research firms.

Panel B. Definition of control variables

Variable	Definition
Analyst-specific	
Analyst seniority (years)	Analyst's seniority is the number of years since her first appearance in the IBES earnings forecasts and recommendation databases.
=1 if all-star analyst	The all-star analyst dummy variable equals one if the analyst was an all-star (i.e., ranked as a top-three or runner-up analyst in her industry) in the year prior to making the recommendation, and zero otherwise.
Relative forecast accuracy	Analyst forecast accuracy as in Hong and Kubik (2003).
Bank pressure	
Loyalty index in bond (equity) market	Following LMW (2006), we construct a loyalty index which, for each bank, measures how often it retains its clients in consecutive debt (equity) deals, divided by the number of clients.
=1 if no bond (equity) loyalty index	Because we include many smaller banks that did not underwrite any deals in a particular year thereby resulting in a missing value in the loyalty measure, we include a dummy variable for the missing values so as to differentiate it from observations with zero retained clients.
Fee pressure	
Firm's share of bank's debt (equity) deals in prior 5 years (%)	Firm's dollar amounts of securities issuance allocated to a bank as a percentage of the bank's total debt (equity) underwriting dollar amounts during the prior five calendar years.
Log issuer's bond (equity) proceeds in prior 5 years	Logarithm of firm's overall debt (equity) issuing amounts in millions during the prior 5 years.
Proxy for side payments	
Change in bond (equity) issue activities	Following LMW (2006), we compute the percentage difference in market-wide average proceeds raised during the current quarter and a 5-year quarterly moving average in the bond (equity) market.
Equity ownership by brokerage banks	
Institutional holdings (%)	Institutional holding is as a percentage of common shares outstanding.
=1 if bank holds stake in issuer's equity	Equals to one if a firm's equity is owned by a brokerage bank whose analyst provides coverage for the firm.
Bank reputation in underwriting and lending	
Bank's market share in debt (equity) underwriting (%)	Bank's aggregated total dollar amounts in lead underwriting as a percentage of all deal amounts in bond (equity) market during the calendar year prior to stock recommendation.
Bank's market share in loan market (%)	Bank's aggregated total dollar amounts of loans lead arranged as a percentage of all deal amounts in the loan market during the

	calendar year prior to stock recommendation.
Firm-specific	
Firm size	Total value of book assets
Tobin's Q	(Total assets -Book value of equity + Market value of equity) divided by Total assets.
Information opacity	Following Kim and Verrecchia (2001), we compute a proxy for firm's information opacity as the logarithm of the coefficient of the trading volume in the regression $$Ln\left|\frac{P_t - P_{t+1}}{P_{t+1}}\right| = \beta_0 + \beta_1\left(VOL_t - AVGVOL\right) + \varepsilon,$$ where, P_t: daily stock closing price, VOL_t: daily trading volume of the stock in thousands of shares, $AVGVOL$: the average daily stock trading volume within the last 6-months (we use 182 days) in thousands of shares. Kim and Verrecchia (2001) posit that, when the firm discloses more information, market makers rely on the disclosure itself, rather than on alternative sources of information about firm value, such as volume. Thus, as the firm commits to report information in a timely fashion, stock returns are less likely to be associated with trading volume. This predicts that firms with poor disclosure (or greater information opacity) would have a larger slope coefficient on trading volume (β_1).

Appendix A2 Broker characteristics by bank type

This table reports mean values of broker characteristics of different types of banks during our sample period (1996-2006) with one year lag. See Appendix A1, Panel A for the definitions of bank types, Panel B for variable definitions. Significance levels of tests of differences in means between lead manager main bank and other types of banks are denoted next to the statistics of other types of banks.

	Lead manager main bank	Co-manager main bank		Co-lead syndicate bank		Co-manager syndicate bank		Independent group	
Number of observations	6195	9544		8664		8739		8383	
Bank's market share in debt underwriting (%)	8.32	4.56	***	7.74	***	0.03	***	0.15	***
Bank's market share in equity underwriting (%)	8.26	3.81	***	7.26	***	0.15	***	0.21	***
Loyalty index in bond market	0.34	0.24	***	0.36	***	0.11	***	0.04	***
Loyalty index in equity market	0.54	0.48	***	0.53	***	0.35	***	0.13	***
=1 if no bond loyalty index	0.21	0.43	***	0.14	***	0.80	***	0.94	***
=1 if no equity loyalty index	0.10	0.13	***	0.09		0.25	***	0.72	***
=1 if all-star analyst	0.32	0.16	***	0.26	***	0.01	***	0.02	***
=1 if bank holds stake in issuer's equity	0.71	0.53	***	0.82	***	0.30	***	0.15	***

*, **, *** Significant at the 10, 5, and 1 percent levels, respectively, for a two-tailed test.

Appendix A3 Reasons for filing lawsuits

This table reports the sample distribution of the reasons for filing securities class action lawsuits during 1996 to 2006.

Reasons for filing lawsuits	No.	%
IPO allocation, Tie-in/laddering Agreements	145	19.4
General financial misreporting	541	72.3
Artificially inflate securities prices	640	85.6
Inadequate internal control	87	11.6
Bond issuance related	55	7.4
Equity issuance related	293	39.2
Mergers and acquisitions related	122	16.3
Insider trading	255	34.1
SEC 1934 Sections 10(b) and rule 10b-5	643	86.0
SEC 1933 Section 11	246	32.9
GAAP violation/improper accounting	239	32.0
Investment banks also sued in the same filing	208	27.8
Total number of lawsuits	748	100.0

Appendix A4 Total number of underwriters in the underwriting market over time

This figure reports the total number of underwriters over time during 1986 through 2006. The data are obtained from the Thomson Financial/SDC Platinum database of securities offerings. We plot the number of underwriters based on parent company names.
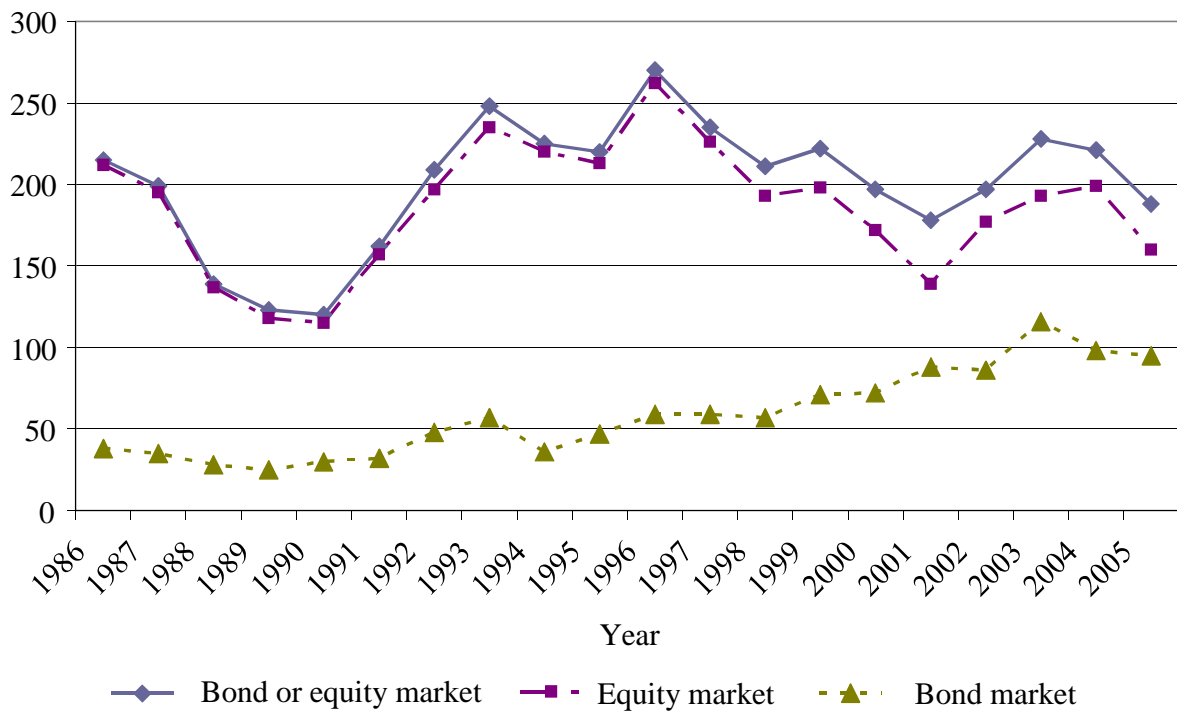
Appendix A5 Percentage of syndicated pairs based on possible pairs

This figure reports the percentage of observed pairs divided by possible pairs in bond or equity underwriting markets over time. The possible book-co manager pair is calculated based on the number of available book managers and co-managers in a market each year. For example, if the numbers of book managers and co-managers served in the bond market in a year are 10 and 20, respectively, then the possible pair is 190 (10*20-10) because book managers will not include themselves as co-managers in the same deals. In addition, book managers are active co-managers in deals organized by other book managers, 10 co-managers overlap with the book managers. The data are obtained from the Thomson Financial/SDC Platinum database of securities offerings.
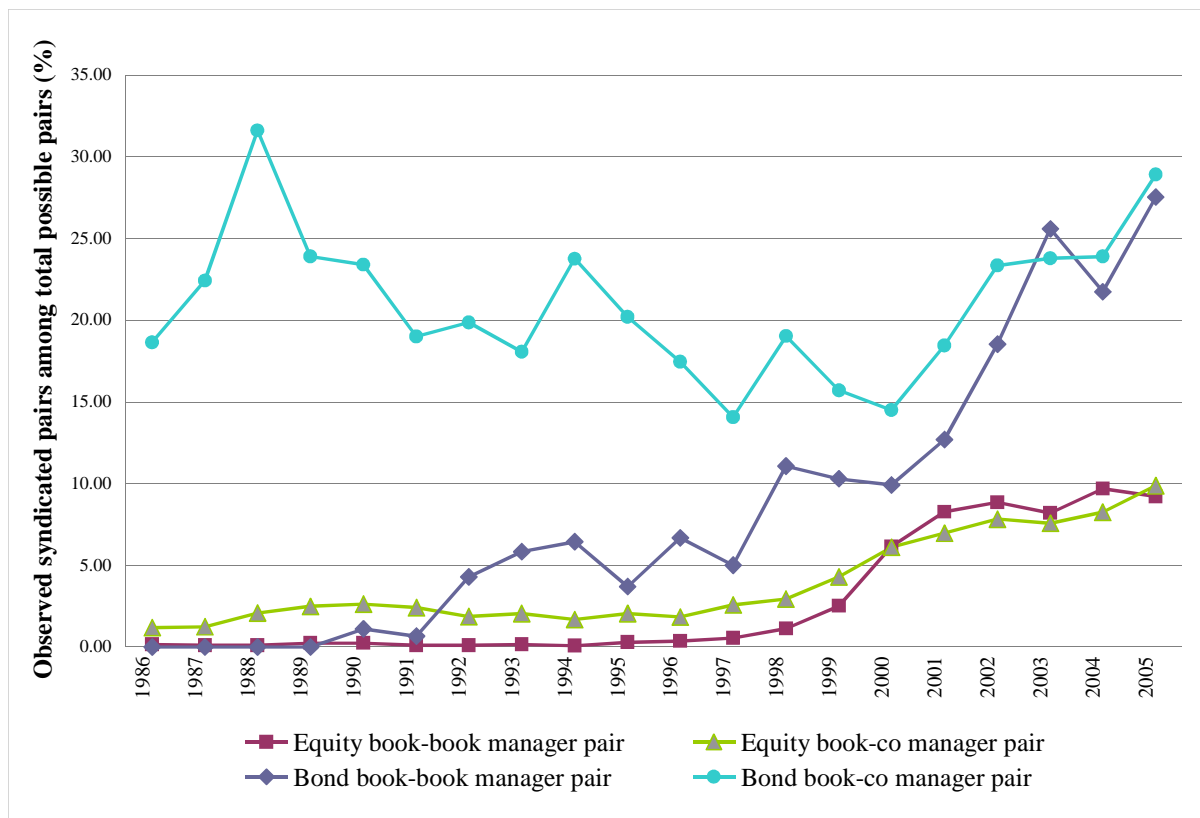
Figure 1 Bank types and the levels of conflicts of interest

See Panel A of Appendix A1 for the definitions of bank types and associated analyst types.

Underwriting Fees $$$

| Highest conflicts of interest |

Co-manager *main* bank

Lead manager *main* bank (i.e., lead underwriter)

Firm A

Securities underwriting services

- Does not serve firm A
- Shared lead role with main banks in other deals
- Have capacity to organize its own syndicates

- Does not serve firm A
- Relies on main banks to be in other deals

- Does not serve firm A
- Does not work with main banks

Co-Lead *syndicate* bank

Co-manager *syndicate* bank

Independent group

| Least conflicts of interest |

Conflicts of interest due to reciprocal pressure???

Figure 2 Time line of events associated with lawsuits, changes in buy and hold stock value of lawsuit firms, and data construction
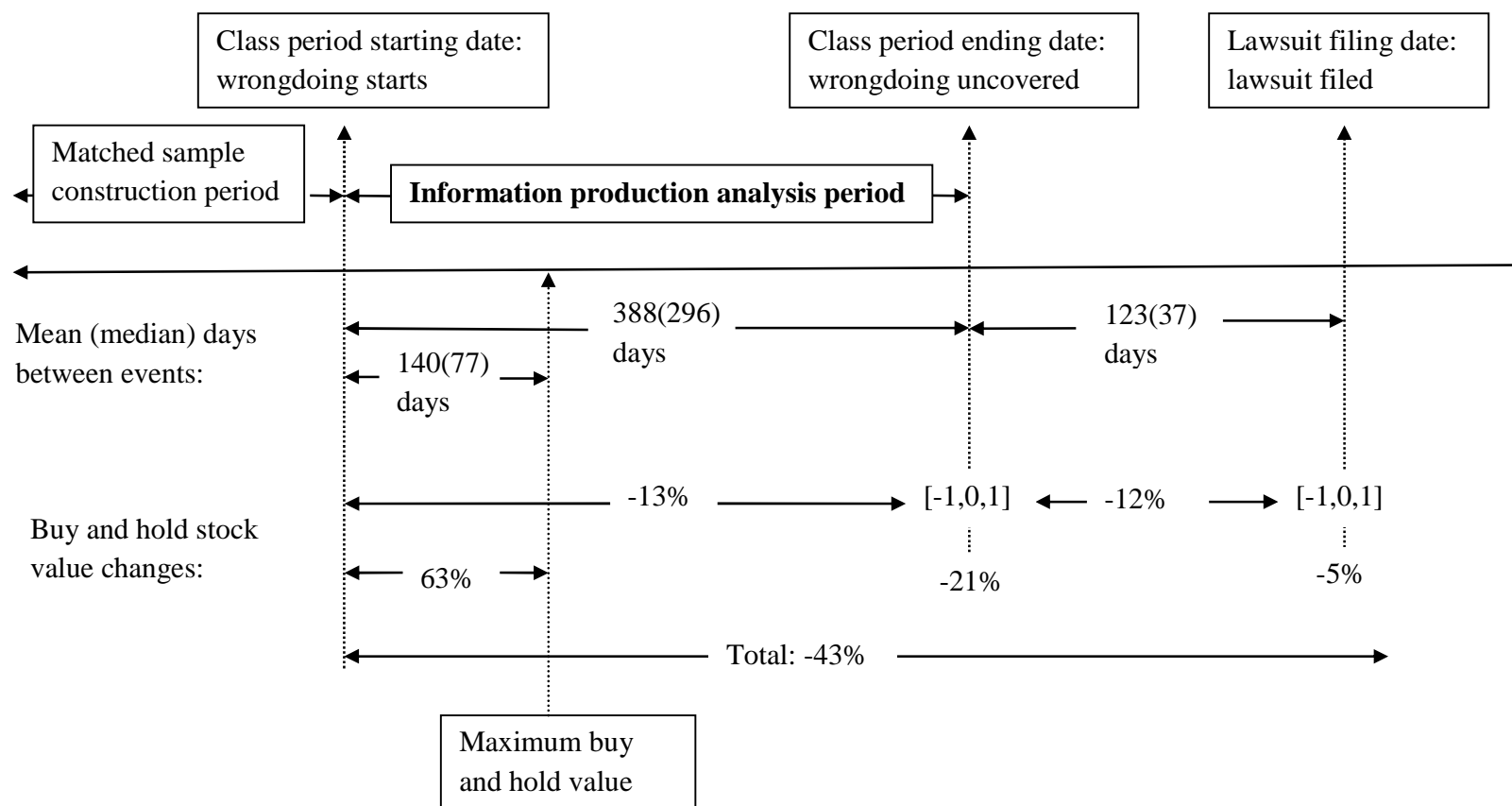
Figure 3 Failure functions of downgrades by analyst type

"Time to downgrade" represents "Scaled # days," which is the number of days that the current recommendation is issued after the class period starting date (i.e., wrong doing beginning date) divided by the duration of a class period and multiplied by 100. Time 0 (100) corresponds to class period starting (ending) date. See Appendix A1 for the definitions of analyst/bank types and Table 6 for sample description. The y-axis shows failure (downgrade) rates.
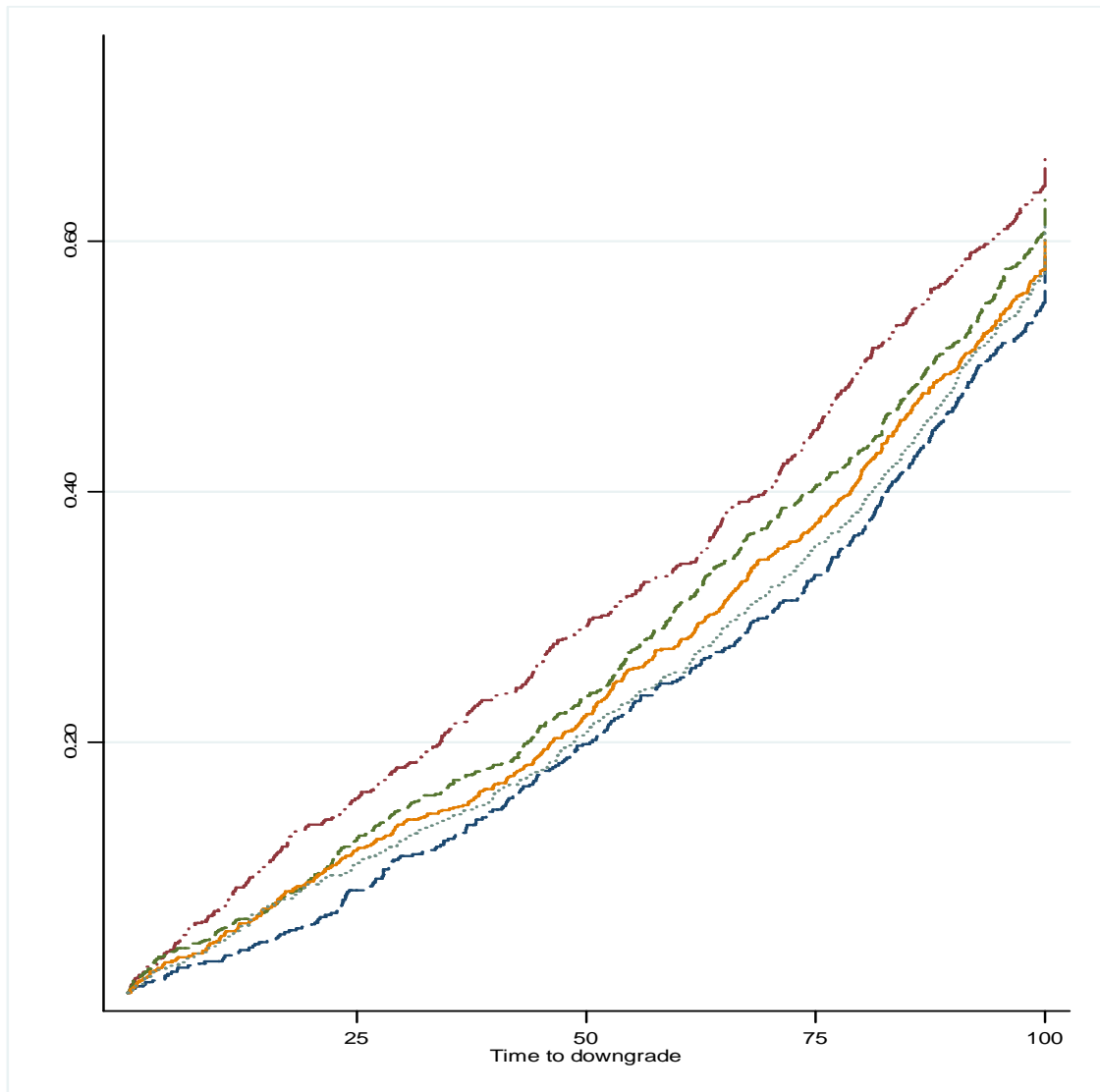
Table 1 Buy and hold value of investing in stocks of sued firms during event windows

The sample consists of firms being sued in securities class action lawsuits from 1996 to 2006. Class period is specified in each lawsuit dating the beginning and the end of a sued firm's wrongdoing. The class period starting date to the max value day denotes the time period that a buy and hold investment reaches maximum value staring from the beginning of class period. The class ending date denotes the day when wrongdoing is uncovered. Lawsuit date is the day when the lawsuit is filed. This table reports the buy and hold value of the sued firms or the CRSP market indexes at the end of the time interval if $1 was invested at the beginning of the interval. Significance levels of tests of differences between sued firms and the market index are indicated next to the means and medians of sued firms.

	Sued firms					Value weighted index		Equally weighted index	
Event window	Obs.	Mean		Median		Mean	Median	Mean	Median
Class period starting date to two days prior to class period ending date	519	0.87	***	0.82	***	1.09	1.07	1.29	1.17
Class period starting date to max value day	519	1.63	***	1.30	***	1.09	1.05	1.17	1.10
Three days around class period ending date [-1,0,1]	686	0.79	***	0.81	***	1.00	1.01	1.00	1.01
Class period ending date to two days prior to lawsuit date	541	0.88	***	0.94	***	1.00	1.00	1.11	1.05
Three days around lawsuit date [-1,0,1]	682	0.95	***	0.98	***	1.00	1.00	1.00	1.01

*, **, *** Significant at the 10, 5, and 1 percent levels, respectively, for a two-tailed test.

Table 2 Distribution of stock recommendation revisions by year and by period

This table presents the sample distribution of stock recommendation revisions for sued firms and matched, non-sued firms during 1993-2006. We only include the recommendations of interests, i.e., those issued within two years prior to a sued firm's class period up to lawsuit filing date. Class period is specified in each lawsuit dating the beginning and the end of a sued firm's wrongdoing. Matched sample firms are non-sued firms in the same industry (classified by 2-digit SIC code) as sued firms with the closest number of analysts followed by the closest firm size (total assets) during the year prior to class period.

Time period	All	Sued sample		Matched sample	
		No.	% of all	No.	% of all
Total	28,211	16,225	57.5	11,986	42.5
1993	8	3	37.5	5	62.5
1994	447	217	48.5	230	51.5
1995	1,043	576	55.2	467	44.8
1996	1,718	982	57.2	736	42.8
1997	2,079	1,083	52.1	996	47.9
1998	2,993	1,528	51.1	1,465	48.9
1999	3,423	1,688	49.3	1,735	50.7
2000	4,325	2,533	58.6	1,792	41.4
2001	3,679	2,496	67.8	1,183	32.2
2002	3,691	2,265	61.4	1,426	38.6
2003	2,092	1,184	56.6	908	43.4
2004	1,455	846	58.1	609	41.9
2005	806	519	64.4	287	35.6
2006	452	305	67.5	147	32.5
Prior to class period	13,314	6,473	48.6	6,841	51.4
During class period	10,291	6,354	61.7	3,937	38.3
Class period to lawsuit filing date	4,606	3,398	73.8	1,208	26.2
Before Rule 2711	21,636	12,296	56.8	9,340	43.2
Post Rule 2711	6,575	3,929	59.8	2,646	40.2

Table 3 Mean value of analysts' and brokers' characteristic variables

This table reports the mean values of characteristic variables of analysts and brokerage banks issued stock recommendations for sued and matched, non-sued firms issued within two years prior to a sued firm's class period and up to the lawsuit filing date. Matched sample firms are non-sued firms in the same industry (classified by 2-digit SIC code) as sued firms with the closest number of analysts followed by the closest firm size (total assets) during the year prior to class period. Tests of differences in means between groups are reported next to the matched sample. All variables are defined in Panel B of Appendix A1.

Variable	Class period starting date to lawsuit filing date (sample period)			Within 2-year prior to class period (benchmark period)		
	Sued	Matched		Sued	Matched	
Level of prior recommendation	4.05	3.98	***	3.98	3.97	
Analyst seniority (year)	4.19	4.30	**	3.77	3.58	***
=1 if all-star analyst	0.13	0.15	***	0.14	0.12	***
Relative forecast accuracy	50.69	50.45		51.00	50.44	
Loyalty index in bond market	0.21	0.22		0.21	0.20	**
=1 if no bond loyalty index	0.53	0.51		0.53	0.56	***
Loyalty index in equity market	0.40	0.41	*	0.40	0.40	
=1 if no equity loyalty index	0.26	0.26		0.28	0.27	
Firm's share of bank's debt deals prior 5 years (%)	0.04	0.02		0.03	0.03	
Firm's share of bank's equity deals prior 5 years (%)	0.23	0.32	*	0.23	0.31	
Log issuer's bond proceeds in prior 5 years (in $mil)	1.75	1.94	***	2.35	1.85	***
Log issuer's equity proceeds in prior 5 years (in $mil)	4.27	4.24		3.85	4.11	***
Change in bond issue activities	0.22	0.16	***	0.18	0.22	***
Change in equity issue activities	0.06	0.09	***	0.17	0.19	**
Institutional holdings (%)	59.64	58.99		65.54	57.00	***
=1 if bank holds stake in issuer's equity	0.50	0.48	*	0.47	0.41	***
Bank's market share in debt underwriting (%)	4.00	3.97		3.75	3.44	***
Bank's market share in equity underwriting (%)	3.58	3.64		3.39	3.14	***
Bank's market share in loan market (%)	1.19	1.19		1.04	1.02	
Total assets ($Bn)	13.03	9.08	***	11.17	6.12	***
Tobin's Q	4.43	3.06	***	3.52	3.25	***
Information opacity	-10.87	-10.66	***	-11.08	-10.67	***
Number of recommendations on the same day	1.63	3.67	***	1.28	2.97	***
No. of observation	9519	5041		6376	6745	

*, **, *** Significant at the 10, 5, and 1 percent levels, respectively, for a two-tailed test.

Table 4 Univariate analysis of stock recommendations, revisions, and timeliness

This table reports summary statistics on the level, revision, and timeliness of analyst recommendations of sued (Panel A) and matched non-sued (Panel B) firms issued by analysts from various types of brokerage banks. The sample includes recommendations issued during the period between the class period starting date and the lawsuit filing date. Current recommendations are those issued during the sample period. Prior recommendation indicates the latest recommendation issued by the same broker for the same firm prior to the current recommendation. Change in recommendation indicates the difference between current and prior recommendations. The column labeled "Scaled # days" is the number of days that the current recommendation is issued after the class period starting date (i.e., wrong doing beginning date) divided by the duration of a class period and multiplied by 100. Matched sample firms are non-sued firms in the same industry (classified by 2-digit SIC code) as sued firms with the closest number of analysts followed by the closest firm size (total assets) during the year prior to the class period. Significance levels of tests of differences in percentages between lead manager main bank and other types of banks are denoted next to the statistics of other types of banks. The tests of differences between the sued and matched samples for all types of banks are reported at the bottom of the table.

| | | All revisions | | | | | Downgrade revision only | | |
| | | Prior | Change | Current | Scaled # days | | | Current | Scaled # days |
	N	Mean	Mean	Mean	Median	N	% of all	Mean	Median
Panel A: Sued sample									
Affiliated analyst:									
Lead manager main bank	1,508	4.13	-0.46	3.67	82.4	798	52.92	3.18	95.2
Co-manager main bank	2,516	4.16	-0.50	3.66	84.2	1,431	56.88 **	3.17	93.4
Dependent analyst:									
Co-manager syndicate bank	2,064	4.05 ***	-0.38 **	3.67	84.1 **	1,127	54.6	3.14	93.1
Independent analyst:									
Co-lead syndicate bank	1,833	3.88 ***	-0.34 ***	3.54 ***	80.8	956	52.15	3.07 ***	91.7 **
Independent group	1,831	4.01 ***	-0.35 ***	3.66	73.8 ***	953	52.05	3.06 ***	83.5 ***
All types	9,752	4.05	-0.41	3.64	81.3	5,265	53.99	3.13	91.9
Panel B: Matched sample									
Lead manager main bank	766	4.10	-0.28	3.82	65.8	345	45.04	3.29	65.8
Co-manager main bank	1,190	4.05	-0.30	3.75 *	66.1	574	48.24	3.19 ***	67.4
Co-manager syndicate bank	1,025	4.04	-0.27	3.77	59.7 **	519	50.63 **	3.21 **	59.8 **
Co-lead syndicate bank	1,081	3.81 ***	-0.17 *	3.64 ***	64.6	459	42.46	3.14 ***	67.7
Independent group	1,083	3.97 ***	-0.18	3.78	54.2 ***	511	47.18	3.16 ***	54.2 ***
All types	5,145	3.99	-0.24	3.75	61.8	2,408	46.8	3.19	63.2
Diff. for all types: *t*/z-test		-4.28 ***	8.50 ***	6.81 ***	-15.5 ***		-8.34 ***	4.04 ***	-15.9 ***

*, **, *** Significant at the 10, 5, and 1 percent levels, respectively, for a two-tailed test.

Table 5 Analysis of timeliness of downgrade revisions

In this table, we examine in multiple regressions the effect of various types of brokerage banks on the timeliness of the downgrades of sued firms from the class period starting date to the lawsuit filing date in regressions (1) and (2). Regressions (3) and (4) are for the matched non-sued sample. Regression (1) is based on all downgrades within the specified periods, while regression (2) is based on only the first downgrade of each broker-firm pair. The dependent variable is "Scaled # days," which is the number of days that the current recommendation is issued after the class period beginning date (i.e., wrong doing starting date) divided by the duration of a class period, multiplied by 100. Matched sample includes non-sued firms in the same industry (classified by 2-digit SIC code) as sued firms with the closest number of analysts followed by the closest firm size (total assets) during the year prior to the class period. To formally examine whether the coefficients are different between co-lead syndicate and co-manager syndicate banks, we conduct F-tests and report the results at the bottom of the table. Standard errors (S.E.) are clustered allowing correlations within brokers.

	Sued sample						Matched sample					
	All downgrades			First downgrades per broker-firm			All downgrades			First downgrades per broker-firm		
	(1)			(2)			(3)			(4)		
	Coef.	S.E.		Coef.	S.E.		Coef.	S.E.		Coef.	S.E.	
Affiliated analyst:												
Intercept (=lead manager main bank)	146.31	30.71	***	126.83	36.08	***	24.43	80.72		-119.30	158.06	
=1 if co-manager main bank	-7.15	12.78		-5.95	9.66		17.76	17.17		41.11	29.71	
Dependent analyst:												
=1 if co-manager syndicate bank	-8.64	12.01		-0.31	10.11		13.59	14.69		28.48	22.66	
Independent analyst:												
=1 if co-lead syndicate bank	-25.81	10.01	**	-18.18	7.77	**	-1.31	9.83		-2.73	15.06	
=1 if independent group	-25.48	17.68		-23.88	17.90		19.88	14.61		40.72	22.54	*
Level of prior recommendation	-9.09	3.53	**	-4.85	5.22		-2.66	5.63		3.64	12.79	
Analyst seniority (year)	-0.06	1.20		0.22	1.37		1.12	1.11		1.23	2.10	
=1 if all-star analyst	-13.96	10.08		-2.47	9.83		9.86	17.90		21.60	31.68	
Loyalty index in bond market	26.90	32.25		34.47	25.59		20.67	26.03		24.50	39.42	
=1 if no bond loyalty index	-10.12	7.11		-0.51	7.75		-14.92	12.26		-19.01	20.58	
Loyalty index in equity market	-23.78	16.69		-33.16	20.51		-29.59	16.75	*	-59.09	24.04	**
=1 if no equity loyalty index	6.65	8.40		3.63	9.66		-1.50	12.99		-6.47	25.65	
Firm's share of bank's debt deals (%)	0.29	0.93		0.74	0.90		-1.18	2.78		4.33	5.15	
Firm's share of bank's equity deals (%)	0.99	0.87		1.11	0.77		-0.19	0.78		-0.17	2.43	
Log issuer's bond proceeds in prior 5 yrs	5.13	1.00	***	4.58	1.13	***	1.41	1.30		-0.46	2.36	
Log issuer's equity proceeds in prior 5 yrs	5.26	1.41	***	4.02	1.40	***	4.14	1.09	***	6.24	1.93	***
Change in bond issue activities	-2.96	7.25		-2.72	7.23		13.60	7.78	*	16.20	10.11	
Change in equity issue activities	-5.89	3.31	*	-6.11	3.55	*	-28.22	7.02	***	-26.26	10.90	**
Institutional holdings (%)	-0.14	0.09		-0.09	0.09		0.14	0.14		0.01	0.16	
=1 if bank holds stake in issuer's equity	11.64	6.77	*	13.55	7.80	*	-3.69	11.74		-9.49	20.15	

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Bank's market share in debt underwriting (%)	-0.52	0.42		-0.52	0.35		-1.77	1.09		-1.67	1.18	
Bank's market share in equity underwriting (%)	-1.11	0.75		-0.59	0.76		-0.12	1.07		0.61	1.94	
Bank's market share in loan market (%)	-0.62	2.40		-2.55	2.26		3.33	6.61		3.18	7.56	
Log (total assets)	0.75	2.05		0.09	1.93		-6.90	3.61	*	-10.86	6.28	*
Tobin's Q	-0.62	0.21	***	-0.72	0.22	***	1.98	1.24		1.22	1.94	
Information opacity	1.03	2.63		1.37	2.45		-7.90	6.13		-17.05	11.64	
=1 if IPO allocation lawsuit	56.87	7.76	***	59.99	7.47	***						
Ln(no of recommendations on the same day)	16.55	5.11	***	21.80	5.86	***	-4.97	5.05		-9.56	8.49	
R^2	0.08			0.09			0.09			0.13		
Number of observations	3,301			2,598			1,471			821		
Number of broker clusters	215			215			165			151		
Broker fixed effects	Yes			Yes			Yes			Yes		
F-test (co-manager syndicate = co-lead syndicate)	3.01*			4.86**			0.80			1.32		
P-value of F-test	0.084			0.0285			0.373			0.253		

*, **, *** Significant at the 10, 5, and 1 percent levels, respectively, for a two-tailed test.

Table 6 Survival analysis of the timeliness of downgrades

The sued sample includes recommendations issued during the period between the class period starting date and the lawsuit filing date. For each firm-broker pair, only the first downgrade is included and classified as the failure event. For those do not provide any downgrades during our event window, they are included as the non-failure event. The time variable is "Scaled # days," which is the number of days that the current recommendation is issued after the class period starting date (i.e., wrong doing beginning date) divided by the duration of a class period and multiplied by 100. Matched sample firms are non-sued firms in the same industry (classified by 2-digit SIC code) as sued firms with the closest number of analysts followed by the closest firm size (total assets) during the year prior to the class period. The dependent variables are whether a broker provides a downgrade for a firm at the time that such an action occurs. Wald tests reported at the bottom of the table examine whether the coefficients are different between co-lead syndicate and co-manager syndicate. Standard errors (S.E.) are clustered within brokers.

	(1) Sued sample			(2) Matched sample		
	Coef.	S.E.		Coef.	S.E.	
Affiliated analyst:						
=1 if co-manager main bank	0.058	0.063		0.058	0.142	
Dependent analyst:						
=1 if co-manager syndicate bank	0.095	0.078		0.236	0.156	
Independent analyst:						
=1 if co-lead syndicate bank	0.195	0.055	***	0.085	0.128	
=1 if independent group	0.146	0.082	*	0.119	0.168	
Level of prior recommendation	0.286	0.034	***	0.357	0.045	***
Analyst seniority (year)	0.005	0.009		-0.004	0.011	
=1 if all-star analyst	-0.008	0.061		-0.063	0.071	
Loyalty index in bond market	-0.175	0.111		0.279	0.233	
=1 if no bond loyalty index	-0.150	0.073	**	0.180	0.145	
Loyalty index in equity market	-0.108	0.096		0.235	0.114	**
=1 if no equity loyalty index	-0.037	0.078		0.147	0.113	
Firm's share of bank's debt deals (%)	0.000	0.009		-0.049	0.049	
Firm's share of bank's equity deals (%)	-0.005	0.004		-0.009	0.017	
Log issuer's bond proceeds in prior 5 yrs	-0.046	0.010	***	-0.012	0.013	
Log issuer's equity proceeds in prior 5 yrs	-0.016	0.011		-0.052	0.013	***
Change in bond issue activities	-0.087	0.048	*	-0.224	0.068	***
Change in equity issue activities	0.049	0.042		0.292	0.071	***
Institutional holdings (%)	0.000	0.001		-0.001	0.001	
=1 if bank holds stake in issuer's equity	-0.036	0.049		-0.016	0.089	
Bank's market share in debt underwriting (%)	-0.001	0.003		0.002	0.005	
Bank's market share in equity underwriting (%)	0.002	0.003		0.005	0.010	
Bank's market share in loan market (%)	0.003	0.014		-0.004	0.018	
Log (total assets)	0.078	0.020	***	-0.013	0.031	
Tobin's Q	0.016	0.003	***	-0.030	0.010	***
Information opacity	0.074	0.022	***	0.016	0.037	
=1 if IPO allocation lawsuit	-0.723	0.072	***			
Ln(no of recommendations on the same day)	-0.170	0.034	***	0.006	0.036	
Number of observations	3,219			1,720		
Number of broker clusters	230			183		
Log Likelihood	-17696			-7446		
Wald test (co-manager syndicate = co-lead syndicate)	2.639		*	1.926		
P-value of Wald test	0.104			0.165		

*, **, *** Significant at the 10, 5, and 1 percent levels, respectively, for a two-tailed test.

Table 7 Analysis of timeliness of downgrade revisions of sued firms surrounding Rule 2711

This table reports the effect of various types of banks on the timeliness of the downgrades of sued firms before and after the implementation of Rule 2711. The full sample period is from the class period starting date to the lawsuit filing date. Columns (1) and (2) are based on a subsample prior to and following August 1, 2002 when Rule 2711 was adopted, respectively. Column (3)-(4) are based on the full sample but incorporate a Post-rule 2711 dummy and its interaction terms with all the right-hand side variables. The coefficient estimates and standard errors of the intercept and right-hand-side variables are reported in Column (3), and those of the interaction terms are reported in Column (4) with the label "*Post-rule 2711". Post-rule 2711 is a dummy variable that equals one for downgrades provided after August 1, 2002, and zero otherwise. The dependent variable is "Scaled # days," which is the number of days that the current recommendation is issued after the class period beginning date (i.e., wrong doing starting date) divided by the duration of a class period, multiplied by 100. To formally examine whether the coefficients are different between co-lead syndicate and co-manager syndicate banks, we conduct F-tests and report the results at the bottom of the table. Independent group include independent banks and independent research firms. Standard errors (S.E.) are clustered allowing correlations within brokers.

	Subsample						Full Sample with interaction terms			
	(1) Before Rule 2711			(2) Post Rule 2711			(3) Base			(4) *Post-rule 2711
	Coef.	S.E.		Coef.	S.E.		Coef.	S.E.		Coef.	S.E.
Affiliated analyst:											
Intercept (=lead manager main bank)	198.91	45.46	***	61.75	40.79		177.75	40.80	***	-79.05	41.75	*
=1 if co-manager main bank	-12.19	17.57		-4.45	5.90		-11.82	16.82		9.01	17.22
Dependent analyst:											
=1 if co-manager syndicate bank	-10.30	16.06		-13.13	12.34		-10.34	14.82		-5.08	17.45
Independent analyst:											
=1 if co-lead syndicate bank	-35.11	15.00	**	-11.93	5.72	**	-34.23	14.22	**	20.04	16.37
=1 if independent group	-27.77	23.16		-17.87	16.19		-27.14	20.33		9.98	20.32
Level of prior recommendation	-14.57	6.05	**	-9.78	2.73	***	-13.77	5.83	**	5.59	6.92
Analyst seniority (year)	0.30	1.86		-0.57	0.76		0.28	1.81		-0.56	1.70
=1 if all-star analyst	-14.88	13.82		0.88	6.01		-14.53	13.19		9.03	15.04
Loyalty index in bond market	20.07	40.02		28.49	25.53		17.48	35.03		-3.41	33.71
=1 if no bond loyalty index	-15.96	11.20		7.69	22.08		-17.11	10.61		20.53	17.64
Loyalty index in equity market	-44.09	28.57		-5.69	20.39		-36.62	23.44		26.51	22.60
=1 if no equity loyalty index	1.56	15.19		17.53	12.71		0.83	13.04		16.69	19.12
Firm's share of bank's debt deals (%)	-0.18	1.30		21.03	3.25	***	-0.29	1.29		24.09	4.52	***
Firm's share of bank's equity deals (%)	1.20	0.94		0.26	0.72		1.12	0.94		-1.47	1.42
Log issuer's bond proceeds in prior 5 yrs	6.90	1.52	***	-0.05	1.11		6.94	1.49	***	-7.40	2.04	***
Log issuer's equity proceeds in prior 5 yrs	6.84	1.95	***	2.12	0.98	**	6.70	1.92	***	-5.04	2.01	**
Change in bond issue activities	-17.45	9.95	*	17.49	6.18	***	-17.05	9.86	*	36.13	10.68	***
Change in equity issue activities	-10.13	3.95	**	1.74	8.89		-10.28	3.87	***	7.03	9.73
Institutional holdings (%)	-0.11	0.11		-0.24	0.11	**	-0.12	0.11		-0.10	0.14

=1 if bank holds stake in issuer's equity	16.88	8.67	*	-1.21	6.81		15.99	8.15	*	-14.38	8.66	*
Bank's market share in debt underwriting (%)	-0.62	0.55		1.42	1.87		-0.57	0.43		-2.04	1.19	*
Bank's market share in equity underwriting (%)	-0.33	1.37		-1.05	0.67		-0.31	1.08		-0.56	0.67	
Bank's market share in loan market (%)	-0.11	3.37		12.99	9.45		0.01	2.89		7.28	4.06	*
Log (total assets)	-0.84	2.58		2.63	2.96		-1.12	2.43		5.06	3.83	
Tobin's Q	-0.70	0.27	***	-4.00	1.99	**	-0.70	0.26	***	-3.76	1.90	**
Information opacity	1.62	3.32		-4.09	3.40		0.90	3.22		-4.47	4.50	
=1 if IPO allocation lawsuit	58.76	7.83	***				58.90	7.75	***			
Ln(no of recommendations on the same day)	21.52	6.25	***	2.59	3.45		21.46	6.23	***	-16.61	8.07	**
R^2	0.09			0.29			0.09					
Number of observations	2,464			837			3,301					
Number of broker clusters	179			124			215					
Broker fixed effects	Yes			Yes			Yes					
F-test (co-manager syndicate = co-lead syndicate)	3.84**			0.01			5.43**			4.10**		
P-value of F-test	0.0515			0.919			0.0207			0.0440		

*, **, *** Significant at the 10, 5, and 1 percent levels, respectively, for a two-tailed test.

Table 8 Difference-in-differences tests of the mean changes of syndication relationships surrounding syndicate bank downgrades issued earlier than main bank downgrades

This table reports mean values of changes in syndication relationship (in %) surrounding early downgrades made by syndicate banks prior to main banks. Syndication relationship in Panel A is calculated by aggregating dollar amounts of equity deals during an event window lead-managed by the main banks while the co-manager syndicate banks serving as co-managers. This aggregated number is then scaled as a percentage of total dollar amounts of all equity issuances lead-managed by main banks during the same period. Panel B includes equity deals that the co-lead syndicate banks work with the main banks. We first report the deals in which these co-lead syndicate banks serve as co-managers followed by the deals that both main banks and syndicate banks share the lead roles. Column 1 (2) covers the year prior to (following) downgrades made by syndicate banks. Column 3 (4) covers three years prior to (following) downgrades made by syndicate banks. The treatment group includes the sued firms and the control group includes matched non-sued firms.

Time period during	No. of Obs.	(1) Prior year	(2) Following year	Difference: (2) – (1)		(3) prior three years	(4) following three years	Difference: (4) – (3)	
Panel A. Co-manager syndicate banks									
Sued sample	291	14.99	13.39	-1.61		12.35	13.61	1.27	
Matched sample	141	12.44	15.70	3.26	*	11.59	14.83	3.24	**
Diff.: Sued - Matched		2.55	-2.31	-4.86	**	0.76	-1.21	-1.97	
Panel B. Co-lead syndicate banks									
Serving as co-managers									
Sued sample	261	23.25	25.07	1.83		25.52	22.85	-2.67	*
Matched sample	193	23.07	24.49	1.42		26.04	21.20	-4.83	***
Diff.: Sued - Matched		0.18	0.59	0.40		-0.51	1.65	2.17	
Serving as co-lead managers									
Sued sample	235	7.65	10.03	2.38	***	7.43	11.88	4.45	***
Matched sample	163	7.28	7.07	-0.21		6.23	8.98	2.75	***
Diff.: Sued - Matched		0.37	2.96	2.59	*	1.20	2.90	1.70	

*, **, *** Significant at the 10, 5, and 1 percent levels, respectively, for a two-tailed test.