

# **Equity Research Departments' Internal Review Policies and Analysts' Information Production: Evidence from Oversight Committees**

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**This Draft: April 2024**

## **Abstract**

I examine the effect of banks' internal review policies on analysts' information production. To overcome the lack of observability of these policies, I exploit a requirement in the Global Settlement that mandates sanctioned banks establish oversight committees to review material target price revisions. Some departments implemented 10% bright-line thresholds for these revisions, whereas others did not. I find that analysts at departments with bright-line thresholds issue absolute target price revisions below the 10% threshold to avoid triggering a review. I also find that the review process, when triggered, decreases the timeliness of analysts' revisions. However, despite oversight committees' mandate to monitor research quality, I find little evidence that the reviewed target price revisions are less optimistic.

*JEL Codes:* G24, G28

*Keywords:* Security Analysts, Equity Research, Global Settlement

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## 1. Introduction

Sell-side analysts contribute to the efficient functioning of capital markets. Banks' research departments that employ analysts are responsible for overseeing the content of analysts' research and implementing policies to ensure compliance with regulations and quality assurance (e.g., FINRA 2019). If these policies introduce frictions to the research process, analysts have incentives to minimize the influence on their work. While research documents variation in banks' internal policies, such as the benchmarks used to define recommendations (Kadan et al. 2020) and whether analysts can own stock in firms they cover (Chan et al. 2018), little empirical evidence addresses how analysts' research production changes in response to the implementation of banks' internal policies.

In this paper, I examine how the implementation of an equity research department's internal review policy affects analysts' information production. This issue has not received much attention, likely because banks' internal policies are generally unobservable to researchers. To overcome this challenge, I exploit a specific requirement in the Global Research Analyst Settlement of 2003 ("Global Settlement") that mandates that the 12 sanctioned banks establish oversight committees to review all stock recommendation revisions and material changes in target prices before analysts' reports can be disseminated (Global Settlement 2004, Addendum A, p. 11). I focus on target price revisions, given the discrete nature of analysts' recommendations. The requirement is the same for all sanctioned banks and remains in place today. Some banks instituted bright-line thresholds, whereas others did not; nevertheless, the impact of this requirement created frictions for analysts. For example, the oversight committee at Morgan Stanley "often sent reports back for revision, which led to analyst grumbling but ultimately resulted in a stronger product" (Srinivasan and Lane 2011, p. 7). UBS revealed publicly that it employs a bright-line threshold, stating that "Changes of 10% or more in rating and/or price target require [Investment Review

Committee] approval” (UBS 2021, p. 5). However, at other sanctioned banks, the implementation was viewed “as yet another regulatory burden, leading to a *check the box mentality* by both analysts and the committee itself” (Groysberg et al. 2008, p. 129, emphasis added).

I make three main predictions on how banks’ implementation of the oversight committee mandate affects analysts’ information production. First, my primary hypothesis is that banks’ use of bright-line thresholds causes analysts to alter their target price forecasts to avoid review. While a review should provide a quality check on analysts’ revisions, it may also delay the issuance of research reports and encroach on analysts’ other tasks (e.g., Srinivasan and Lane 2011). Consequently, I expect analysts facing bright-line thresholds to issue fewer absolute target price revisions above the threshold and more absolute revisions below it. Second, consistent with the conjecture that the reviews require significant time and effort, I predict that reviews, when triggered, decrease the timeliness of analysts’ target price revisions.<sup>1</sup> Third, as the regulatory purpose of the oversight committee is to monitor the quality of analyst research, I examine whether the review process increases the quality of analysts’ target prices, specifically decreases their optimism and increases their accuracy. The use of a bright-line threshold could reflect banks’ responsiveness to the Global Settlement’s mandate that banks monitor the quality of their analysts’ research, leading to an increase in research quality. However, banks without a bright-line threshold could have alternative means of monitoring research quality, such as strong corporate cultures (Pacelli 2019). In addition, banks without a bright-line threshold could review certain revisions. Thus it is unclear whether the implementation via a bright-line threshold increases the quality of analysts’ target prices.

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<sup>1</sup> A former senior analyst told me: “I do recall, on occasion, re-thinking my [target price] revision to ensure it didn’t exceed the threshold that required I meet with the committee because it would slow me down.”

The empirical challenge is sorting sanctioned banks by how they implemented the oversight committee mandate.<sup>2</sup> I argue that one important source of variation is the decision to use a bright-line threshold to define material target price revisions. Because UBS is the only sanctioned bank that publicly revealed a bright-line threshold, I first compare it to all other sanctioned banks to test whether the use of a bright-line threshold induces analysts to avoid exceeding the threshold. I use McCrary (2008) tests of analysts' absolute target price revisions in the years before (2000–2002) and after (2004–2006) the Global Settlement. I find that UBS's absolute target price revisions exhibit a significantly negative discontinuity at 10% in the post-settlement period but not in the pre-period. In contrast, I find no evidence of a significantly negative bright-line discontinuity in the pre- or post-periods for other banks. This analysis provides preliminary evidence that UBS analysts shifted their absolute target price revisions to fall below the bright line, consistent with an effort to avoid review.

I use a difference-in-differences research design to examine the effect of the bright-line threshold on the frequency of target price revisions. Employing a 10% bright-line threshold for all target price revisions, I compare absolute revision frequencies above and below this threshold across UBS and non-UBS analysts, in the pre- and post-settlement periods. In the post-period, I find a significant decline in the frequency of above-threshold absolute revisions among UBS analysts and a corresponding increase in the frequency of below-threshold absolute revisions that net out to an overall increase in the total frequency of target price revisions. The frequency results

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<sup>2</sup> I performed three main steps in an attempt to determine sanctioned banks' policies for defining material target price revisions. First, I searched for any public documents or announcements by the banks of their policies via websites (included archived versions using the [Wayback Machine](#)) and other news sources. Next, I contacted the head of research (or equivalent) at each bank via phone or email. Finally, I submitted Freedom of Information Act (FOIA) requests to the SEC for Independent Monitor reports, which contain findings on how the banks comply with the terms of the Global Settlement. Unfortunately, other than identifying UBS's policy, all three of these efforts were unsuccessful. The banks did not respond to my queries, indicating an unwillingness to discuss internal policies, and the SEC's FOIA Office denied requests due to a FOIA exemption for financial institutions.

indicate that the use of a bright-line threshold induces UBS analysts to substitute above-threshold absolute revisions with more frequent below-threshold ones to avoid review.

Although UBS is the only sanctioned bank to publicly disclose a bright-line threshold, I expect that others also employ bright-line thresholds. I employ inductive analysis to identify such banks, again by using bank-specific histograms of absolute target price revisions and McCrary (2008) tests.<sup>3</sup> Consistent with none of the banks using bright-line thresholds before the Global Settlement, there is no evidence of significantly negative absolute revision discontinuities in the pre-period. However, two other sanctioned banks—Morgan Stanley and Lehman Brothers—exhibit significantly negative discontinuities at 10% absolute target price revisions in the post-settlement period.<sup>4</sup> Thus, in my later tests, I classify the three banks with a significantly negative discontinuity as “Brightline” banks and the other banks without a significantly negative discontinuity as “Non-Brightline” banks.<sup>5</sup>

Comparing the two groups, my second set of tests examines whether analysts’ absolute target price revisions that exceed the bright-line threshold become relatively less timely. I employ the full sample of banks to compare the timeliness of target price revisions using a triple-differences analysis. Specifically, I compare the timeliness of revisions (i) by analysts at Brightline and Non-Brightline banks, (ii) pre and post Global Settlement, (iii) for absolute revisions above and below the 10% bright-line threshold. Following prior research, I measure the timeliness of revisions as the difference in trading days between the earnings announcement date and an

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<sup>3</sup> Prior research also uses distributions of observed variables to infer internal policies in other settings. For example, Guay et al. (2021) use insider trading data to infer the start and end dates of firms’ quarterly trading windows.

<sup>4</sup> I also test for discontinuities at 5% intervals from 5% to 30% and find no evidence of significantly negative discontinuities at these other thresholds.

<sup>5</sup> Although non-sanctioned banks are not subject to the oversight committee requirement, it is possible that non-sanctioned banks voluntarily adopted oversight committees and accompanying bright-line thresholds. I test for the existence of bright-line thresholds for large non-sanctioned banks (e.g., RBC) and find no evidence of significantly negative discontinuities at 5% intervals from 5% to 30% thresholds for non-sanctioned banks in the pre- or post-settlement periods.

analyst's subsequent earnings forecast issuance date (e.g., Zhang 2008; Driskill et al. 2020). I restrict my sample to earnings forecast revisions that contain a concurrent target price revision ("bundled revisions"), as I use target price revisions to identify whether the revisions require oversight review. Brightline above-threshold absolute revisions become significantly less timely in the post-period, relative to Brightline below-threshold absolute revisions and relative to Non-Brightline above-threshold absolute revisions. These results highlight that reviews, in fact, decrease the timeliness of analysts' revisions.

Next I provide cross-sectional tests to assess whether the review committees' effect on timeliness is less pronounced when the magnitude of the earnings news is large and more pronounced when the analyst's tenure at the bank is long. First, I expect oversight committee frictions are reduced when there is higher demand for timely research from banks' clients (e.g., large earnings surprises). Oversight committees likely do not want to impose high costs on analysts, especially when doing so has negative revenue implications for the bank (e.g., loss of trading commissions due to untimely research reports). Indeed, the decreased timeliness of reviewed revisions is concentrated in the subset of revisions that follow earnings announcements with small earnings surprises. My second cross-sectional test demonstrates that the decrease in timeliness resulting from the review process is more pronounced for analysts with above-median tenures at their banks. This finding is consistent with long-term analysts being reluctant to change their behavior in response to the Global Settlement (Corwin et al. 2017) or, in this specific case, their reluctance to participate in mandated reviews (e.g., requiring more rounds of review).

Finally, because the aim of the oversight requirement is to monitor research quality, I examine whether the use of a bright-line threshold affects the optimism and accuracy of analysts' target price revisions. I use measures of ex-post target price optimism and accuracy from prior

research (Bradshaw et al. 2013; Bradshaw et al. 2019). Unsurprisingly, given existing work on decreases in the optimism of sanctioned banks' recommendations post-settlement (e.g., Corwin et al. 2017), I find a general decrease in the optimism and absolute forecast errors for the target prices of the analysts of all sanctioned banks following the Global Settlement. Interestingly, however, I find no evidence of a significant decline in the optimism of *reviewed* target price revisions over those that were not reviewed. That is, Brightline analysts' above-threshold absolute target price revisions are no less optimistic than their below-threshold ones, nor are Brightline analysts' above-threshold absolute target price revisions any less optimistic than those issued by Non-Brightline analysts. Two implications arise from these findings.

First, while the quality of both Brightline and Non-Brightline banks' target prices increases significantly in the post-Settlement period, there is no evidence of a difference in quality of analysts' revisions associated with bright-line thresholds. One plausible explanation is that Non-Brightline banks have alternative means to monitor research, such as strong cultures (Pacelli 2019) or disciplinary mechanisms for analysts producing biased reports (Altinkılıç et al. 2019). Another plausible explanation is that Non-Brightline banks review certain large revisions.

Second, the lack of evidence of a significant increase in the quality of reviewed revisions over non-reviewed revisions suggests the review process per se does not reduce the optimism of analysts' target price revisions. My research design allows me to directly compare revisions that are reviewed by oversight committees to those that are not (e.g., Brightline analysts' absolute target price revisions above and below the threshold). Despite providing powerful and direct tests, I find no evidence that reviews per se reduce the optimism of analysts' target prices. Thus, while a statistically insignificant difference cannot prove that the effect does not exist, my evidence is consistent with a limited impact of oversight review on research quality. This is one of my

important contributions to the literature on analysts' target price optimism (e.g., Brav and Lehavy 2003; Bradshaw et al. 2019).

My study contributes to the accounting literature in three additional ways. First, I contribute to the literature on the determinants of analyst research by providing large sample evidence on how variation in the implementation of a regulatory mandate affects analysts' information production. Despite the importance of analysts' role as information intermediaries, there is limited evidence about how they generate their research (e.g., Bradshaw 2011). I extend research on banks' internal policies (e.g., Kadan et al. 2020; Chan et al. 2018) by showing that analysts decrease (increase) the frequency of above-threshold (below-threshold) absolute target price revisions to avoid reviews.

Second, I contribute to the literature on the impact of the Global Settlement and related regulations on analyst research. The regulatory reforms led to a decrease in the optimism of analysts' recommendations (e.g., Barber et al. 2006), especially for sanctioned banks (e.g., Corwin et al. 2017). These findings suggest the mandates enhanced the quality of analyst research. I contribute by documenting a potential negative outcome—a decrease in relative timeliness for analysts' revisions requiring oversight review. As timelier forecasts are associated with stronger market reactions (e.g., Cooper et al. 2001) and a decrease in post-earnings-announcement drift (Zhang 2008), the reduction in relative timeliness may harm capital market efficiency.

Finally, I contribute to accounting research documenting the effects of bright-line thresholds on firm behavior. Firms manage earnings to avoid reporting earnings decreases and losses (e.g., Burgstahler and Dichev 1997; Roychowdhury 2006). In addition, they manage size to avoid regulatory, disclosure, and audit requirements (e.g., Gao et al. 2009; Kepler et al. 2020;



Bernard et al. 2018; Kausar et al. 2016). I provide the first evidence that analysts as individual agents manage their target price revisions to avoid oversight.

## **2. Institutional Background, Related Literature, and Hypothesis Development**

### *2.1 Institutional Background: The Global Settlement and Oversight Committees*

New York Attorney General (NYAG) Eliot Spitzer's investigation of Merrill Lynch in the early 2000s represented the first major investigation into analyst conflicts of interest. The investigation uncovered inconsistencies between analysts' internal and public comments about covered companies, especially for investment banking clients (Spitzer 2002a). Spitzer announced a settlement with Merrill Lynch in May 2002, which included reforms reducing the influence of investment banking in the research department (Spitzer 2002b). While the attorney general's office concluded its investigation into practices at Merrill Lynch, it had an ongoing investigation into conflicts of interest at other large investment banks (Spitzer 2002b). This investigation evolved into a joint investigation in April 2002, when the SEC announced a formal investigation into conflicts of interest, which would be conducted with other regulatory bodies (Donaldson 2003).<sup>6</sup>

The result of the joint investigation was the Global Research Analyst Settlement. The settlement, approved by the U.S. District Court for the Southern District of New York in October 2003, enhanced the reforms from the Merrill Lynch settlement that separated investment banking and research.<sup>7</sup> For example, sanctioned banks agreed to physically separate the departments and prohibit investment bankers from influencing the research department's budget and compensation, evaluating analysts' performance, and participating in coverage decisions (SEC 2003). In addition, analysts were prohibited from participating in investment banking activities (e.g., roadshows).

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<sup>6</sup> The regulators included the SEC, the NYAG, the National Association of Securities Dealers (NASD), the New York Stock Exchange, the North American Securities Administrators Association (NASAA), and the states.

<sup>7</sup> In addition to structural reforms, the Global Settlement included enhanced disclosures, independent research requirements, and monetary sanctions. Corwin et al. (2017, pp. 616-617) provide an overview of these components.

The Global Settlement required two monitoring mechanisms to ensure compliance. Sanctioned banks had to retain an external independent monitor to review compliance with the Global Settlement's reforms and submit a written report to the various regulatory bodies. In addition, "Addendum A" of the Global Settlement requires the creation of an internal "oversight/monitoring committee" to:

- a. "review (beforehand, where practicable) all changes in ratings, if any, and *material changes in price targets*, if any, contained in the firm's research reports;
- b. conduct periodic reviews of research reports to determine whether changes in ratings or price targets, if any, should be considered; and
- c. *monitor the overall quality and accuracy* of the firm's research reports" (Global Settlement 2004, Addendum A, p. 11, emphasis added).<sup>8</sup>

Oversight committees are required to be comprised of members of research management (e.g., head of research). Addendum A allows for other personnel to participate on the committees but prohibits investment banking personnel from participating (p. 11).

While some banks had oversight committees prior to the mandate, the Global Settlement gave the committees a regulatory function (Srinivasan and Lane 2011). Anecdotal evidence suggests that influential people within banks serve on the committees, including members of research management and senior analysts (Nanda et al. 2008). As part of the regulatory mandate to monitor the overall quality of analysts' research, the committees review and critique analysts' reports, and committees can send reports back for revisions (Srinivasan and Lane 2011). Oversight committees face costs and benefits in how seriously they conduct their reviews. Committees have an incentive to carefully review analysts' reports to comply with the requirements of the Global Settlement and maintain the bank's reputation. However, committees likely do not want to impose high costs that reduce analysts' abilities to generate revenue for the bank (e.g., hosting investor

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<sup>8</sup> Addendum A can be accessed at the following link: <https://www.sec.gov/litigation/litreleases/dbaddma.pdf>.

conferences). Anecdotal evidence supports the existence of these trade-offs: while analysts often view the reviews as improving the quality of their reports, they also raise concerns that the process is too time-consuming (Groysberg et al. 2008).

## *2.2 Related Literature*

The post-2002 period resulted in several significant changes to the characteristics of analysts' research, with these changes largely attributed to the Global Settlement, NASD Rule 2711, and NYSE Rule 472.<sup>9</sup> Given the requirements in NASD Rule 2711 and NYSE Rule 472 for banks to publicly disclose their recommendation distributions using three levels (percentages of buys, holds, and sells), many banks reduced their recommendation levels from five tiers to three (Barber et al. 2006; Kadan et al. 2008). The same public disclosure requirements contributed to the observed decrease in the favorableness of analysts' recommendations. Specifically, the percentage of buy (sell) recommendations decreased (increased) in the mid-2000s (Barber et al. 2006). Finally, a recent working paper by Kang et al. (2021) finds that analysts are more likely to issue a sell recommendation within a short window of issuing a buy recommendation, consistent with individual analysts concerning themselves with their own distribution of recommendations.

The sanctioned banks experienced the most significant changes in the characteristics of analysts' research in the post-reform period. The decrease in favorable recommendations is especially pronounced for sanctioned banks (Barber et al. 2006; Corwin et al. 2017), which all shifted to a three-tier system (Kadan et al. 2008). Analyst affiliation bias also decreased post-settlement but only for sanctioned banks (Corwin et al. 2017).

Recent research also examines variation in policies across banks. Banks use different benchmarks for defining stock recommendations (e.g., industry or market benchmarks), and

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<sup>9</sup> NASD Rule 2711 and NYSE Rule 472 were approved by the SEC in May 2002, with an effective date of no later than September 2002.

differences in benchmarks are associated with differences in distributions of recommendations (Kadan et al. 2020). Banks also vary in whether they allow analysts to own stock in firms they cover. Analysts who own stock in covered firms exert greater research effort (Chan et al. 2018).

Finally, bank culture and monitoring affects analyst research. Investment banks discipline analysts for issuing biased research through negative career outcomes (Altinkılıç et al. 2019). In addition, financial institution culture impacts research quality. Pacelli (2019) finds that analysts at financial institutions with weaker cultures, captured by FINRA violations unrelated to equity research, are more likely to cater to institutional clients.

### *2.3 Hypothesis Development*

I expect analysts' information production and the characteristics of their revisions to be affected by how banks implemented the oversight committee mandate. I argue that whether a bank employs a bright-line threshold to define material changes in target prices will affect the (i) frequency, (ii) timeliness, and (iii) optimism and accuracy of analysts' target price revisions.

#### *Frequency*

I predict that banks' use of bright-line thresholds leads analysts to change their target price forecasts to avoid review. While reviews have potential benefits, such as providing a quality check on analysts' revisions, they also include potential costs, such as delaying the issuance of research reports (e.g., Srinivasan and Lane 2011) and reducing time for other tasks (e.g., communicating with clients). Therefore, given that UBS uses a 10% bright-line threshold to define material target price revisions (UBS 2021, p. 5), I expect UBS analysts to issue fewer absolute target price revisions above the threshold to avoid the potentially costly review process and instead issue absolute revisions below it.<sup>10</sup>

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<sup>10</sup> For example, consider a UBS analyst with a \$100 target price who would like to increase the target price by \$15 ( $\$15/\$100 = 15\%$  increase). Under UBS's 10% bright-line threshold, the \$15 increase would require oversight

**H1(a):** UBS analysts decrease the frequency of above 10% absolute target price revisions after the Global Settlement.

**H1(b):** UBS analysts increase the frequency of below 10% absolute target price revisions after the Global Settlement.

If the bright-line threshold induces UBS analysts to substitute above-threshold absolute revisions with more frequent below-threshold ones, then the total frequency of UBS analysts' target price revisions would increase. However, UBS analysts could respond by shifting absolute revisions from above to below the threshold without issuing more frequent below-threshold absolute revisions, resulting in no change in total frequency. Thus the effect of the bright-line threshold on the total frequency of target price revisions is an empirical question.

**H1(c):** UBS analysts do not change the total frequency of target price revisions after the Global Settlement.

### *Timeliness*

Case studies discussing the role of oversight committees at Morgan Stanley and Lehman Brothers indicate that obtaining approval for material changes in target prices often requires considerable time and effort (Srinivasan and Lane 2011; Groysberg et al. 2008). Analysts must submit their draft reports to the committee, prepare for a meeting with the committee, and afterward incorporate any committee-mandated revisions to their reports prior to distribution. Consequently, I predict that the review process, when triggered, decreases the timeliness of analysts' target price revisions.

**H2:** The timeliness of above-threshold absolute revisions by Brightline analysts decreases after the Global Settlement.

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approval. Alternatively, the analyst could first issue a \$5 increase ( $\$5/\$100 = 5\%$  increase) and then issue another \$10 increase ( $\$10/\$105 = 9.5\%$  increase) to arrive at the \$115 desired target price without requiring oversight approval per the bright-line threshold. The use of two below-threshold revisions in this example is consistent with an increase in total revision frequency. UBS's public disclosure states that "the analyst may change the target by up to 10% *over a two-week period* without being required to publish a supporting comment. Changes of 10% or more in rating and/or price target require [Investment Review Committee] approval" (UBS 2021, p. 5, emphasis added).

I expect that the effect of bright-line thresholds on timeliness is less pronounced when the magnitude of the earnings news is large and more pronounced when the analyst's tenure at the bank is long. First, because client demand for timely analyst reports is likely heightened following large earnings surprises, I expect large earnings surprises to be associated with more timely reviews. Second, I predict that the decrease in timeliness resulting from the review process is more pronounced for analysts with above-median tenures at their banks. Research finds that shifts in banks' cultures and training after the Global Settlement have a weaker effect on more established analysts (Corwin et al. 2017). If long-term analysts are reluctant to participate in the mandated reviews, the decrease in timeliness will be more pronounced for these analysts.

### *Optimism and Accuracy*

Because the Global Settlement mandates oversight committees to monitor research quality, a natural next question is whether differences in implementation affect the optimism and accuracy of analysts' revisions. If the decision to use a bright-line threshold reflects banks' responsiveness to the Global Settlement's reforms, then bright-line thresholds should result in higher quality revisions.

**H3:** The quality of above-threshold absolute revisions by Brightline analysts increases after the Global Settlement, relative to above-threshold absolute revisions by Non-Brightline analysts.

However, there is tension whether implementation via a bright-line threshold increases research quality. Banks without a bright-line threshold could also respond but have alternative means of ensuring high quality research. For example, Pacelli (2019) finds that financial institutions' corporate cultures influence research quality. In addition, Altinkılıç et al. (2019) document that banks discipline analysts for producing biased research with a higher likelihood of moving to a lower-tier bank. Finally, bank without a bright-line threshold could review certain revisions.

### 3. Sample and Research Design

#### 3.1 Sample

I use a sample of individual analysts' target price forecasts from the IBES Price Target Detail dataset.<sup>11</sup> See Table 1 for an overview of the sample selection procedures I use. In Panel A, I start with 285,550 target price revisions issued by all IBES contributors from 2000–2002 (pre-Global Settlement period) or 2004–2006 (post-Global Settlement period).<sup>12</sup> I exclude 2003 revisions from the sample period as a transition period. I then restrict the sample to 107,429 target price revisions from the nine sanctioned banks with nonmissing IBES data.<sup>13</sup> These nine banks represent 37.6% of all target price revisions in IBES during my sample period. As all recommendation revisions require oversight review per the Global Settlement, regardless of the magnitude of the accompanying target price revision (if any), I exclude 8,298 target price revisions with concurrent recommendation revisions.<sup>14</sup> I use this sample of 93,911 revisions in the discontinuity tests to test whether UBS has a significant discontinuity at 10% and to infer whether any other sanctioned banks use a bright-line threshold for material target price revisions.

Next, as it is unclear (without knowing the policy) whether banks with a 10% bright-line threshold review revisions *greater than* 10% or *greater than or equal to* 10%, I exclude 1,815 revisions equal to exactly -10% or 10%. I also require that each firm-year has at least two revisions

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<sup>11</sup> I use an IBES Price Target Detail dataset downloaded prior to the changes Thomson-Reuters made to IBES in October 2018.

<sup>12</sup> As I require the analyst's prior target price forecast to determine the revision magnitude, some of the prior target price forecasts are from 1999 and 2003.

<sup>13</sup> The banks included in my sample are the following: UBS, Morgan Stanley, Lehman Brothers, Credit Suisse, Deutsche Bank, Citibank, Merrill Lynch, Bear Stearns, and Piper Jaffray. Three other investment banks are part of the Global Settlement: Goldman Sachs, JPMorgan, and Thomas Weisel Partners. Due to missing target price data from IBES in my sample period for these banks, I exclude these three sanctioned banks from my sample. I confirmed the existence of data irregularities for these banks in my sample years in conversations with representatives from WRDS and Refinitiv (data provider of IBES Estimates).

<sup>14</sup> To reduce the influence of outliers, I drop revisions above the 99<sup>th</sup> percentile for number of days between target price forecasts and revisions below (above) the first (99<sup>th</sup>) percentile for percentage change in target prices. I also drop 2,026 revisions with missing firm identifiers.

and that firms have revisions by both Brightline and Non-Brightline banks in the pre- and post-periods. I also require that revisions have nonmissing analyst, bank, and firm control variables. After applying these criteria, I have a final sample of 63,341 target price revisions, with 1,983 unique firms and 1,807 unique analysts. I use this sample of target price revisions for the optimism and accuracy tests.

To test for changes in target price revision frequency post-settlement, I count the number of revisions per analyst-bank-firm-year. Therefore the unit of observation in the frequency tests is at the analyst-bank-firm-year level. As shown in Panel B of Table 1, I have a final sample of 24,372 for frequency tests after keeping one observation per analyst-bank-firm-year.

Finally, to test for target price timeliness, I restrict the sample to the analyst's first earnings forecast following an earnings announcement that has a concurrent target price revision ("bundled revision"). As shown in Panel C of Table 1, applying this requirement drops my sample from 63,341 target price revisions to 13,900 bundled revisions.

### *3.2 Testing for Discontinuities: UBS and Other Sanctioned Banks*

As UBS is the only sanctioned bank that publicly discloses how it defines material target price revisions (those of 10% or more), I examine whether its analysts avoid exceeding the brightline threshold by issuing absolute revisions below 10% post-Global Settlement. To test whether there is a significant negative discontinuity at 10%, I plot the distribution of analysts' absolute target price revisions in the years before (2000–2002) and after (2004–2006) the Global Settlement, separately for UBS and for all other sanctioned banks.<sup>15</sup> I assess the statistical significance of the discontinuity using McCrary (2008) discontinuity tests.

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<sup>15</sup> To increase the power of my discontinuity tests, I use absolute target price revisions less than or equal to 35% in the distributions to test for discontinuities. This restriction drops fewer than 10% of absolute target price revisions (90<sup>th</sup> percentile = 31.7%).



Figure 1 presents histograms before and after the Global Settlement for UBS. Panel A shows UBS's distribution of absolute target price revisions in the pre-period. I find no evidence of a significantly negative discontinuity at 10% in the pre-period (t-stat. = 1.58), consistent with UBS not using a bright-line threshold in the period before the Global Settlement. In the post-settlement period (Panel B), however, there is evidence of a significantly negative discontinuity at 10% for UBS (t-stat. = -12.32). The number of absolute target price revisions in the bin to the left (right) of 10% is abnormally high (low) and consistent with UBS analysts issuing below-threshold absolute revisions to avoid reviews.

Figure 2 presents pooled histograms for the other sanctioned banks. In Panel A, I find no evidence of a significantly negative discontinuity at 10% in the pre-settlement period (t-stat. = 6.33). I include absolute revisions of exactly 10% in the bin to the right of 10% to be conservative, as research finds that analysts prefer to round their forecasts (Hermann and Thomas 2005; Dechow and You 2012). The significantly *positive* discontinuity at 10% is therefore consistent with analysts issuing absolute revisions of exactly 10%. In the post-period (Panel B), I also find no evidence of a significantly negative discontinuity at 10% (t-stat. = 3.69). I view the lack of a significantly negative discontinuity in the pooled distribution as inconsistent with a bright-line threshold of 10% being widely used across the sanctioned banks following the Global Settlement.

### *3.3 Effect of Bright-line Threshold on Frequency*

I use a difference-in-differences research design to test for the effect of the use of a bright-line threshold on the frequency of above- and below-threshold target price revisions. In particular, I compare the frequency of above 10% (*AboveFreq*) and below 10% (*BelowFreq*) absolute target price revisions by analysts at UBS to those of the other sanctioned banks, in the periods before (2000–2002) and after (2004–2006) the Global Settlement. A benefit of using analysts at other

sanctioned banks as a control group is that these banks are subject to other requirements of the Global Settlement that only apply to the sanctioned banks. In addition, the pre- and post-period design helps account for differences between UBS and the other sanctioned banks throughout the sample period. I measure *AboveFreq* (*BelowFreq*) as the number of absolute target price revisions above (below) 10% per analyst-bank-firm-year and estimate the model in Equation 1.

$$\begin{aligned}
 & \text{Log}(1 + \text{AboveFreq}_{ijkt}) \text{ or } \text{Log}(1 + \text{BelowFreq}_{ijkt}) \\
 & = \beta_1 \mathbf{Post}_t * \mathbf{UBS}_j + \beta_2 \text{FirmExp}_{ikt} + \beta_3 \text{Log\_Coverage}_{jkt} \\
 & + \beta_4 \text{Log\_BrokerSize}_{jt} + \text{Analyst-Bank FE} + \text{Firm-Year FE} \\
 & + \varepsilon_{ijkt}
 \end{aligned} \tag{1}$$

where  $i$  indexes firm,  $j$  indexes bank,  $k$  indexes analyst, and  $t$  indexes year. Variable definitions appear in Appendix A. I include analyst-bank fixed effects to capture time-invariant factors across analysts at a given bank (e.g., analyst ability) and firm-year fixed effects to capture time-varying factors at the firm level (e.g., financing needs). Note that *Post* is subsumed by firm-year fixed effects and *UBS* is subsumed by analyst-bank fixed effects. I double-cluster standard errors by firm and by analyst-bank. I also follow prior research and include time-varying analyst and bank control variables, as analyst and bank characteristics relate to analysts' forecasting performance (e.g., Clement 1999). I control for the analyst's experience covering the firm (*FirmExp*), the number of firms covered by the analyst (*Log\_Coverage*), and bank size (*Log\_BrokerSize*). As H1(a) predicts that UBS analysts decrease the frequency of above-threshold revisions post-settlement, I expect  $\beta_1$  in Equation 1 to be significantly negative for above-threshold frequency. As H1(b) predicts that UBS analysts increase the frequency of below-threshold revisions post-settlement, I expect  $\beta_1$  to be significantly positive for below-threshold frequency.

I also test for the effect of the use of a bright-line threshold on the total frequency of absolute target price revisions (*TotalFreq*), which I measure as the sum of *AboveFreq* and

*BelowFreq*. I examine the effect of UBS's use of a 10% bright-line threshold on total revision frequency by estimating Equation 2.

$$\begin{aligned}
& \text{Log}(1 + \text{TotalFreq}_{ijkt}) \\
&= \beta_1 \text{Post}_t * \text{UBS}_j + \beta_2 \text{FirmExp}_{ikt} + \beta_3 \text{Log\_Coverage}_{jkt} \\
&+ \beta_4 \text{Log\_BrokerSize}_{jt} + \text{Analyst-Bank FE} + \text{Firm-Year FE} \\
&+ \varepsilon_{ijkt}
\end{aligned} \tag{2}$$

As I argue that the effect of the bright-line threshold on the total frequency of target price revisions is an empirical question, H1(c) predicts that  $\beta_1$  in Equation 2 will differ insignificantly from zero. A significantly positive  $\beta_1$  would represent an increase in total frequency.

Finally, I test for the existence of pre-trends in total revision frequency between UBS and the other sanctioned banks by estimating Equation 3.

$$\begin{aligned}
& \text{Log}(1 + \text{TotalFreq}_{ijkt}) \\
&= \sum_{\substack{t=2001 \\ t \neq 2003}}^{2006} \beta_t \text{Year}_t * \text{UBS}_j + \beta_2 \text{FirmExp}_{ikt} + \beta_3 \text{Log\_Coverage}_{jkt} \\
&+ \beta_4 \text{Log\_BrokerSize}_{jt} + \text{Analyst-Bank FE} + \text{Firm-Year FE} \\
&+ \varepsilon_{ijkt}
\end{aligned} \tag{3}$$

In Equation 3, *Post* is replaced by individual year indicators. I exclude the year indicator for 2000 as the benchmark group. As any effect on total frequency from the use of a bright-line threshold is expected to materialize after the Global Settlement, I expect the coefficients on the interaction terms in the pre-period ( $\text{Year}_{2001} \times \text{UBS}$ ,  $\text{Year}_{2002} \times \text{UBS}$ ) to differ insignificantly from zero.

### 3.4 Inferring Brightline and Non-Brightline for Other Sanctioned Banks

Next, despite UBS being the only sanctioned bank to publicly disclose the use of a bright-line threshold, other banks may also employ undisclosed bright-line thresholds. To investigate this possibility, I plot the bank-specific distributions of analysts' absolute target price revisions in the

years after (2004–2006) the Global Settlement.<sup>16</sup> I test for significantly negative discontinuities using McCrary (2008) tests.

Figure 3 provides evidence that two other banks—Lehman Brothers and Morgan Stanley—have significantly negative discontinuities at 10% absolute target price revisions in the post-settlement period. Panel A shows Lehman Brother’s distribution of absolute revisions in the post-period. The discontinuity is significantly negative (t-stat. = -9.29), consistent with Lehman Brothers implementing a 10% bright-line threshold following the Global Settlement. Panel B presents the distribution for Morgan Stanley. Using all revisions, I find no evidence of a significantly negative discontinuity (t-stat. = -0.13).

However, as a bright-line threshold could require oversight review for absolute revisions *greater than* 10%, I also test for bank-specific discontinuities excluding absolute revisions of exactly 10%.<sup>17</sup> To minimize the likelihood that I mechanically create significantly negative discontinuities by excluding 10% absolute revisions, I require the discontinuity to be significantly negative at the 0.1% level (t-stat. < -3.09; one-tailed test) for the discontinuity to be classified as a bright-line threshold. Panel B of Figure 3 shows that, when excluding 10% absolute revisions, Morgan Stanley has a significantly negative discontinuity at 10% (t-stat. = -5.69). Therefore I classify Morgan Stanley as having a bright-line threshold.

Figure 4 presents bank-specific histograms and McCrary (2008) tests for the remaining six sanctioned banks. I find no evidence of significantly negative discontinuities at 10% for these banks. I also find no evidence of a significantly negative discontinuities when I exclude 10%

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<sup>16</sup> In an untabulated analysis, I use bank-specific McCrary (2008) tests of absolute target price revisions in the pre-settlement period to test for bright-line thresholds before the Global Settlement. I find no evidence of significantly negative discontinuities, consistent with none of the banks using bright-line thresholds before the settlement.

<sup>17</sup> If a bank were to use a bright-line threshold requiring review for absolute revisions *greater than* 10%, I would expect an abnormally high number of absolute revisions of exactly 10% (as these revisions would not be subject to review).

absolute revisions.<sup>18</sup> Therefore I classify these six banks without a significantly negative discontinuity as “Non-Brightline” banks.

### 3.5 Effect of Bright-line Threshold on Timeliness

I use a difference-in-difference-in-differences research design to test whether analysts’ absolute target price revisions exceeding the bright-line threshold become less timely. Specifically, I compare the timeliness of revisions (i) by analysts at Brightline and Non-Brightline banks, (ii) pre and post Global Settlement, and (iii) for absolute revisions above and below the 10% bright-line threshold. I follow prior research on the timeliness of analysts’ revisions (e.g., Zhang 2008; Driskill et al. 2020) and measure timeliness as the difference in trading days between the quarterly earnings announcement date and an analyst’s earnings forecast issuance date (*Forecast Lag*) or as an indicator variable equal to one if the analyst’s first earnings forecast is issued on the trading day of or the trading day after the earnings announcement date (*Timely*). As these timeliness measures require analysts’ earnings forecasts, I restrict my sample to analysts’ first earnings forecast revisions following earnings announcements that contain concurrent target price revisions (“bundled revisions”).<sup>19</sup> I then compare the timeliness of above- and below-threshold bundled absolute revisions, as shown in Equation 4.

$$\begin{aligned}
 & \textit{Timely}_{ijkt} \text{ or } \textit{Log}(1 + \textit{Forecast Lag}_{ijkt}) \\
 & = \beta_1 \textit{Post}_t * \textit{Brightline}_j + \beta_2 \textit{Above}_{ijkt} + \beta_3 \textit{Post}_t * \textit{Above}_{ijkt} \\
 & + \beta_4 \textit{Brightline}_j * \textit{Above}_{ijkt} + \beta_5 \textit{Post}_t * \textit{Brightline}_j * \textit{Above}_{ijkt} \\
 & + \beta_6 \textit{FirmExp}_{ikt} + \beta_7 \textit{Log\_Coverage}_{jkt} + \beta_8 \textit{Log\_BrokerSize}_{jt} \\
 & + \textit{Analyst-Bank FE} + \textit{Firm-Year FE} + \varepsilon_{ijkt}
 \end{aligned} \tag{4}$$

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<sup>18</sup> Merrill Lynch (Panel D) has a negative discontinuity when excluding 10% revisions (t-stat = -2.64). However, since the negative discontinuity is not significant at the 0.1% level, I do not classify Merrill Lynch as having a bright-line threshold.

<sup>19</sup> A limitation is that I do not consider EPS-only forecasts following earnings announcements. The use of a bright-line threshold could increase the prevalence of these forecasts.

As H2 predicts that the timeliness of Brightline analysts' above-threshold absolute revisions decreases post-settlement, I expect  $\beta_5$  to be significantly negative for *Timely* and significantly positive for  $\text{Log}(1 + \text{Forecast Lag})$ .

### 3.6 Effect of Bright-line Threshold on Forecast Quality

Finally, I consider the effect of the use of a bright-line threshold on the quality of analyst research. I use the triple-differences research design from the timeliness test and use the full sample of target price revisions. I follow prior research (Bradshaw et al. 2013; Bradshaw et al. 2019) and capture quality using measures of six-month ex-post target price optimism and accuracy from prior research. Specifically, I measure optimism (*Six-Month TP Optimism*) as the target price minus the six-month ahead stock price, scaled by the stock price three days before the target price revision date. I measure absolute target price error (*Six-Month Abs TP Error*) as the absolute value of *Six-Month TP Optimism*. I examine the effect of oversight review on research quality by estimating Equation 5.

$$\begin{aligned}
 & \text{Six-Month TP Optimism}_{ijkt} \text{ or Six-Month Abs TP Error}_{ijkt} \\
 & = \beta_1 \text{Post}_t * \text{Brightline}_j + \beta_2 \text{Above}_{ijkt} + \beta_3 \text{Post}_t * \text{Above}_{ijkt} \\
 & + \beta_4 \text{Brightline}_j * \text{Above}_{ijkt} + \beta_5 \text{Post}_t * \text{Brightline}_j * \text{Above}_{ijkt} \\
 & + \beta_6 \Delta \text{TP} / \text{TP}_{ijkt} + \beta_7 \text{FirmExp}_{ikt} + \beta_8 \text{Log\_Coverage}_{jkt} \\
 & + \beta_9 \text{Log\_BrokerSize}_{jt} + \beta_{10} \text{ROA}_{it} + \beta_{11} \text{Log\_MVE}_{it} + \beta_{12} \text{BTM}_{it} \\
 & + \beta_{13} \text{LogNumAnalysts}_{it} + \beta_{14} \text{Ret Std}_{it} + \beta_{15} \text{Momentum}_{it} \\
 & + \text{Analyst-Bank FE} + \text{Firm FE} + \text{Year FE} + \varepsilon_{ijkt}
 \end{aligned} \tag{5}$$

In Equation 5, I follow prior research on target price optimism and accuracy (e.g., Bradshaw et al. 2019) and use firm and year fixed effects. Firm and year fixed effects are more appropriate than firm-year fixed effects in this model, as firm-year fixed effects do not provide sufficient variation within a year with the ex-post optimism and accuracy variables being measured using six-month-ahead stock prices. In addition to firm and year fixed effects, I include several time-varying firm control variables from prior research on target price optimism and accuracy (Bradshaw et al. 2013;

Bradshaw et al. 2019), such as the standard deviation of the prior 12 months of daily returns (*Ret Std*) and the natural log of market capitalization (*Log\_MVE*). As I argue that the effect of the bright-line threshold on target price optimism and accuracy is an empirical question (as Non-Brightline banks could have other ways to monitor research quality), H3 predicts that  $\beta_5$  will differ insignificantly from zero. A significantly negative  $\beta_5$  would represent a decrease in optimism or absolute forecast errors.

## 4. Results

### 4.1 Descriptive Statistics

Table 2 reports descriptive statistics of target price revisions from analysts at sanctioned banks from 2000–2002 and 2004–2006. Panel A reports statistics for the target price revision frequency sample of 24,372 analyst-bank-firm-year observations. The mean (median) number of total target price revisions per analyst-bank-firm-year is 2.61 (2.00). The mean (median) number of revisions above 10% is 1.22 (1.00), and the mean (median) number of revisions below 10% is 1.39 (1.00). 56% of frequency observations are in the post-settlement period. Furthermore, 15% of analyst-bank-firm-year frequency observations are from UBS analysts.

Panel B of Table 2 reports descriptive statistics for the revision timeliness sample of 13,900 bundled revisions (EPS forecasts following earnings announcements that contain concurrent target price revisions). Seventy-two percent of revisions are issued in the post-Settlement period, and 39% are issued by analysts at Brightline banks. Forty-five percent of revisions include above-threshold absolute target price revisions. Sixty-nine percent of revisions are classified as *Timely*, and the mean (median) *Forecast Lag* is 8.14 (1.00) trading days. I follow Driskill et al. (2020) and take the natural log of one plus *Forecast Lag* to account for skewness in the variable.

Finally, Panel C of Table 2 reports descriptive statistics for the full sample of 63,341 target price revisions used in the forecast quality tests. Fifty-nine percent of total revisions are issued in the post-settlement period, and 44% are issued by analysts at Brightline banks. Forty-seven percent of absolute revisions are above the 10% threshold. The mean (median) percentage change in target price ( $\Delta TP/TP$ ) is 1.0% (4.0%). Target prices revisions are positively biased, as mean (median) *Six-Month TP Optimism* is 0.23 (0.15). In addition, the mean (median) *Six-Month Abs TP Error* is 0.34 (0.22). These optimism and absolute forecast error statistics are consistent with prior research on the optimistic bias and low accuracy of analysts' target price revisions (e.g., Bradshaw et al. 2013). Finally, on average, analysts have five years of firm-specific experience (*FirmExp*), cover 17 firms (*Coverage*), and work at banks employing 167 analysts (*BrokerSize*).

#### 4.2 Frequency Tests

I present the results on the effect of the bright-line threshold on target price revision frequency in Table 3. Panel A presents regression results for the effect of the bright-line threshold on the frequency of above-threshold absolute target price revisions. Column 1 provides the regression with no fixed effects and no control variables. The frequency of above-threshold absolute revisions decreases significantly in the post-settlement period (coef. = -0.236; t-stat. = -15.74). The decrease in above-threshold absolute revisions is more pronounced for UBS analysts, as *Post x UBS* is significantly negative (coef. = -0.121; t-stat. = -2.90). Column 2 includes analyst-bank and firm-year fixed effects. *Post x UBS* remains significantly negative (coef. = -0.136; t-stat. = -3.94) with the inclusion of fixed effects as well as with the inclusion of analyst and bank control variables in Column 3 (coef. = -0.136; t-stat. = -3.94). The relative decrease in the frequency of above-threshold absolute revisions for UBS analysts is economically significant, as it represents a



23.1% decrease relative to the sample mean.<sup>20</sup> The results indicate the frequency of above-threshold absolute revisions decreases significantly for UBS analysts, relative to non-UBS analysts, consistent with UBS analysts issuing relatively fewer above-threshold absolute revisions to avoid review.

Panel B of Table 3 presents regression results for the effect of the bright-line threshold on the frequency of below-threshold absolute revisions. The frequency of below-threshold absolute revisions increases significantly post-settlement (coef. = 0.303; t-stat. = 17.82 in Column 1). I find that *Post x UBS* is significantly positive across the columns (e.g., in Column 3, coef. = 0.364; t-stat. = 8.67). The relative increase in the frequency of below 10% absolute revisions for UBS analysts is economically significant, representing a 75.5% increase relative to the sample mean.<sup>21</sup> The below-threshold results are consistent with UBS analysts issuing more below-threshold absolute revisions after the bright-line threshold is implemented.

In Panel C of Table 3, I consider the net effect of the bright-line threshold on total revision frequency. Total revision frequency increases significantly post-settlement (coef. = 0.048; t-stat. = 3.76 in Column 1). The increase in total frequency is stronger for UBS analysts; *Post x UBS* is significantly positive in Column 2 with no controls (coef. = 0.209; t-stat. = 5.60) and Column 3 with control variables (coef. = 0.200; t-stat. = 5.59). The relative increase in *TotalFreq* for UBS analysts represents a 30.6% increase relative to the sample mean and is consistent with UBS analysts substituting above-threshold absolute revisions with more frequent below-threshold ones.<sup>22</sup>

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<sup>20</sup> Economic significance for *AboveFreq* is calculated as  $[(\exp(-0.136) - 1) \times ((1 + 1.22) / 1.22)] = -23.1\%$ . The calculation adjusts for the outcome variable being log transformed (e.g., Bonsall, Green, and Muller 2020).

<sup>21</sup> Economic significance for *BelowFreq* is calculated as  $[(\exp(0.364) - 1) \times ((1 + 1.39) / 1.39)] = 75.5\%$ .

<sup>22</sup> Economic significance for *TotalFreq* is calculated as  $[(\exp(0.200) - 1) \times ((1 + 2.61) / 2.61)] = 30.6\%$ .

Finally, Panel D of Table 3 and Figure 5 report regression results for parallel trends in total revision frequency. I replace *Post* with individual year indicators and exclude the indicator for 2000 as the benchmark group. Consistent with the results in Panel D, I find that the interaction terms between *UBS* and the year indicators in the post-settlement period (2004–2006) are significantly positive. For example, in Column 2,  $2004 \times UBS$  has a coefficient of 0.291 (t-stat. = 5.48). However, I also find that  $2001 \times UBS$  is significantly positive (coef. = 0.114; t-stat. = 2.36 in Column 2). While the significant interaction term is consistent with UBS issuing a relatively greater number of total revisions in 2001, the magnitude of the coefficient is small, relative to the coefficients of the interaction terms in the post-period. In addition, the interaction term in 2002 differs insignificantly from zero (coef. = 0.071; t-stat. = 1.50). Finally, I find no evidence of a significantly negative discontinuity at 10% for UBS in the pre-settlement period (see Panel A of Figure 1). However, I acknowledge that the evidence of a significant difference in total revision frequency in 2001 is a limitation of my frequency results.

#### 4.3 Timeliness Tests

I present the results on the effect of the bright-line threshold on bundled forecast revision timeliness in Table 4. Columns 1 and 4 provide regression results with no fixed effects and no control variables for *Timely* and  $\text{Log}(1 + \text{Forecast Lag})$ , respectively. In Column 1, the proportion of timely revisions increases significantly post-settlement (coef. = 0.137; t-stat. = 5.55). The forecast lag measure in Column 4 also indicates an increase in timeliness, as *Post* is significantly negative (coef. = -0.495; t-stat. = -7.35). However,  $\text{Post} \times \text{Brightline} \times \text{Above}$  is significantly positive in Column 4 (coef. = 0.239; t-stat. = 2.04), indicating that there is a relative decrease in the timeliness of Brightline analysts' above-threshold absolute revisions. Consistent with the findings in Columns 1 and 4, I find that the triple interaction  $\text{Post} \times \text{Brightline} \times \text{Above}$  is

significantly negative for *Timely* (coef. = -0.081; t-stat. = -1.74 in Column 3). Using my *Forecast Lag* timeliness measure, I find that the triple interaction is significantly positive (coef. = 0.307; t-stat. = 2.52 in Column 6). Both timeliness results are economically significant; the triple interaction for *Timely* represents an 11.7% decrease relative to the sample mean, and the triple interaction for *Forecast Lag* represents a 40.3% increase relative to the sample mean.<sup>23</sup> These results indicate that Brightline analysts' above-threshold absolute revisions become relatively less timely in the post-period and are consistent with the oversight process decreasing the relative timeliness of revisions requiring review.

Figure 6 presents parallel trends in bundled revision timeliness for *Timely* (Panel A) and  $\text{Log}(1 + \text{Forecast Lag})$  (Panel B). The figure plots the coefficients of the interaction terms of individual year indicators and *Brightline x Above* (e.g.,  $2004 \times \text{Brightline} \times \text{Above}$ ). I find no evidence of pre-trend differences in timeliness, as the triple interaction terms in 2001 and 2002 differ insignificantly from zero. In the post-period, the triple interaction terms for *Timely* are negative, but only significant at the 5% level in 2005. Similarly, for  $\text{Log}(1 + \text{Forecast Lag})$  the triple interaction terms are positive but only significant at the 5% level for 2005.

#### 4.4 Cross-Sectional Timeliness Tests

I perform two cross-sectional tests to examine how the effect of bright-line thresholds on timeliness differs based on the magnitude of the news in the earnings announcement and on the analyst's tenure at the bank. I report results from the cross-sectional tests in Table 5. Columns 1 and 2 present results for the earnings news cross-sectional test. In this test, I compare the timeliness of revisions for above- and below-median absolute earnings surprises (*Above Median AUE*).<sup>24</sup> As

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<sup>23</sup> Economic significance for *Timely* is calculated as  $[-0.081 / 0.69] = -11.7\%$ . Economic significance for *Forecast Lag* is calculated as  $[(\exp(0.307) - 1) \times ((1 + 8.14) / 8.14)] = 40.3\%$ .

<sup>24</sup> I use the *Forecast Lag* timeliness measure for these tests, as it provides more variation than the *Timely* indicator.

the decrease in the relative timeliness of revisions requiring review is concentrated among Brightline banks, I restrict my sample to Brightline analysts' revisions. I also use firm and year (instead of firm-year) fixed effects, as this fixed effect structure increases the power of my tests (drops fewer singleton observations). I find no evidence of a significant difference in timeliness for above-median absolute earnings surprises (coef. = -0.025; t-stat. = -0.18 in Column 1). However, I find that *Post x Above* is significantly positive in the subsample of below-median absolute earnings surprises (coef. = 0.467; t-stat. = 3.17). In addition, an F-test indicates that the difference in coefficients is statistically significant (p-value = 0.010). These results are consistent with my cross-sectional prediction that large earnings surprises are associated with more timely reviews by oversight committees.

I present results for the analyst tenure cross-sectional tests in Columns 3 and 4 of Table 5. In this test, I compare the timeliness of revisions by analysts with above- and below-median tenure at the bank (*Above Median BankExp*). Similar to the earnings news cross-sectional tests, I restrict my sample to Brightline analysts' revisions and use firm and year fixed effects. I also use bank (instead of analyst-bank) fixed effects, as *Above Median BankExp* is relatively time invariant at the analyst-bank level. I find that *Post x Above* is significantly positive in the above-median analyst tenure subsample (coef. = 0.383; t-stat. = 3.07). However, I find no evidence of a significant difference in timeliness for the below-median subsample (coef. = 0.073; t-stat. = 0.53). An F-test indicates that the difference in coefficients is statistically significant at the 10% level (p-value = 0.098). The results indicate that the decrease in relative timeliness is concentrated among revisions by long-term employees of the bank. I view these results as consistent with long-term analysts being reluctant to change their behavior in response to changes in banks' internal policies following the Global Settlement (e.g., Corwin et al. 2017).

#### 4.5 Target Price Optimism and Accuracy Tests

I present the results on the effect of the bright-line threshold on the optimism and accuracy of analysts' target price revisions in Table 6. Panel A presents regression results for target price optimism. Column 1, with no fixed effects and no control variables, indicates that ex-post optimism decreases significantly post-settlement (coef. = -0.217; t-stat. = -11.78). The decrease in optimism is stronger on average for above-threshold absolute revisions, as *Post x Above* is significantly negative (coef. = -0.039; t-stat. = -2.43). However, I find no evidence of the change in optimism for above-threshold absolute revisions varying based on the use of a bright-line threshold; *Post x Brightline x Above* is not significant (coef. = -0.000; t-stat. = -0.02). Column 2 presents results with analyst-bank, firm, and year fixed effects, and Column 3 includes control variables. I find no evidence of a significant difference in relative optimism of Brightline analysts' above-threshold absolute revisions; none of the coefficients of *Post x Brightline x Above* are statistically significant across the three columns. I also find no evidence of a significant difference in the triple interaction in the target price absolute forecast error tests in Panel B.

I view the lack of evidence of significant differences in optimism and accuracy between Brightline and Non-Brightline analysts in Panels B and C as consistent with Non-Brightline banks having alternative means to monitor research quality post-settlement. For example, Non-Brightline could increase the quality of their analysts' research through strong corporate cultures (Pacelli 2019) or disciplining mechanisms (Altınkılıç et al. 2019). The lack of evidence of a significant difference could also be explained in part by Non-Brightline banks reviewing certain revisions without using bright-line thresholds. In addition, I find no evidence a significant decrease in the optimism of *reviewed* (above-threshold) target price revisions over those that are not reviewed

within Brightline banks.<sup>25</sup> I view the lack of evidence as consistent with the review process per se not reducing the optimism of analysts' target price revisions. While research documents significant optimism in analysts' target prices (e.g., Brav and Lehavy 2003), I find no evidence that internal reviews attenuate target price optimism.

## 5. Supplemental Analysis

### 5.1 Earnings Forecast Optimism and Accuracy Tests

As oversight committees review all of analysts' outputs included in reports requiring approval (e.g., Srinivasan and Lane 2011), I also examine whether the review process affects the characteristics of analysts' research beyond target prices. Specifically, I consider the optimism and accuracy of analysts' annual earnings forecast revisions. I restrict my sample to 30,743 target price revisions that contain concurrent annual earnings forecast revisions. I present the results in Table 7. Panel A reports regression results for the effect of the bright-line threshold on annual EPS forecast optimism. I find evidence of a decrease in the relative optimism of Brightline analysts' EPS forecasts with above-threshold absolute target price revisions. For example, in Column 2, *Post x Brightline x Above* is significantly negative (coef. = -0.196; t-stat. = -2.05). In the specification in Column 3 controlling for EPS forecast horizon, there is evidence of a relative decrease that is significant at the 10% level (coef. = -0.183; t-stat. = -1.93).<sup>26</sup> I view the finding of a decrease in the relative optimism of Brightline analysts' EPS forecasts with above-threshold

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<sup>25</sup> In an untabulated analysis, I restrict my sample to Brightline target price revisions and examine the change in ex-post optimism. *Post x Above* differs insignificantly from zero, suggesting that above-threshold absolute revisions do not experience a greater decrease in optimism, relative to below-threshold absolute revisions, post-settlement. In another untabulated analysis restricted to Brightline revisions, I find that *Post x Above* is significantly negative for absolute target price forecast errors, suggesting a greater increase in accuracy for above-threshold absolute revisions. However, *Post x Above* is also significantly negative in a sample of Non-Brightline absolute revisions. The difference in coefficients differs insignificantly from zero.

<sup>26</sup> In Panel B of Table 7, I find no evidence of a significant coefficient for *Post x Brightline x Above* for absolute EPS forecast errors.

absolute target price revisions as weak evidence of a potential benefit of banks' oversight of analysts' research.

## **6. Conclusion**

I examine the effect of banks' internal review policies on sell-side analysts' information production. To overcome the challenge that these policies are generally unobservable, I exploit a specific requirement in the Global Settlement that mandated the sanctioned banks to establish oversight committees to review all material changes in target prices. I identify three banks that implemented 10% bright-line thresholds for absolute target price revisions, over which all revisions are subject to review. The group of Brightline banks includes UBS, which publicly disclosed the use of a 10% bright-line policy, and Morgan Stanley and Lehman Brothers. I find that the use of a bright-line threshold leads affected analysts to substitute above-threshold absolute target price revisions with more frequent below-threshold ones to avoid review. I also find that reviews, when triggered, decrease the relative timeliness of analysts' revisions. However, I find little evidence that the oversight impacts research quality. Overall, my findings suggest that banks' use of bright-line thresholds arising from regulatory mandates results in analysts managing their target price revisions to avoid oversight.

My study is subject to several limitations. Other than for UBS, I do not observe the sanctioned banks' internal policies. Morgan Stanley and Lehman Brothers may have a significant discontinuity at 10% absolute target price revisions for reasons unrelated to the oversight requirement. It is also possible that the Non-Brightline banks do use bright-line thresholds to define material target price revisions. In this case, there are at least two reasons why I might not observe a significant discontinuity: (i) analysts at those banks do not manage their target price revisions,

despite the bright-line threshold, or (ii) the banks use another benchmark (other than percentage change in target price) to determine which revisions require oversight review.

Despite these limitations, this paper provides an important step in documenting how variation in the implementation of banks' internal policies affects analysts' incentives, forecasts, and the characteristics of their revisions. In addition, while research identifies a decrease in analyst optimism in the post-reform period and interprets this decrease as a consequence of the regulatory reforms, the ability to attribute changes to any one regulation represents a significant challenge facing the literature (Bradshaw 2009). By exploiting unique features of the details of the Global Settlement and how they are implemented differently across banks (e.g., the oversight committee requirement applying only to sanctioned banks and the ability to distinguish between absolute revisions above and below any bright-line threshold), I improve on the identification and thus the interpretation that this paper's findings result from the Global Settlement.



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## Appendix A: Variable Definitions

This table provides variable definitions. I winsorize all continuous variables at the 1/99% levels to reduce the influence of outliers.

### Dependent Variables:

Variable	Definition
<i>TotalFreq</i>	The total number of target price revisions per analyst-bank-firm-year
<i>BelowFreq</i>	The number of target price revisions per analyst-bank-firm-year below 10% (in absolute value)
<i>AboveFreq</i>	The number of target price revisions per analyst-bank-firm-year above 10% (in absolute value)
<i>Timely</i>	An indicator variable equal to one if the analyst's first earnings forecast for quarter $t+1$ is issued on the trading day of or the trading day after the earnings announcement date for quarter $t$ , and zero otherwise
<i>Forecast Lag</i>	The difference in trading days between the analyst's first earnings forecast for quarter $t+1$ and the earnings announcement date for quarter $t$
<i>Six-Month TP Optimism</i>	The target price minus the six-month ahead stock price, scaled by the stock price three days before the target price revision date. A positive value is optimistic
<i>Six-Month Abs TP Error</i>	The absolute value of <i>Six-Month TP Optimism</i>
<i>EPS Optimism</i>	The one-year ahead EPS forecast minus actual EPS (per IBES), scaled by the stock price three trading days before the forecast date, all multiplied by 100
<i>EPS Forecast Error</i>	The absolute value of <i>EPS Optimism</i>

### Independent Variables:

Variable	Definition
<i>Post</i>	An indicator variable equal to one if the observation is in the post-Global Settlement period (2004–2006), and zero if in the pre-Global Settlement period (2000–2002)
<i>Brightline</i>	An indicator variable equal to one if the bank has a significantly negative discontinuity at 10% absolute target price revision in the post-Settlement period, and zero otherwise
<i>UBS</i>	An indicator variable equal to one for UBS, and zero otherwise
<i>Above</i>	An indicator variable equal to one for target price revisions greater than 10% (in absolute value), and zero for target price revisions less than 10% (in absolute value)

### Control Variables:

Variable	Definition
<i>FirmExp</i>	The number of years the analyst has issued earnings forecasts for a firm
<i>Log_Coverage</i>	The natural log of one plus the number of firms the analyst issues earnings forecasts for during the year
<i>Log_BrokerSize</i>	The natural log of one plus the number of analysts issuing earnings forecasts at the bank during the year
$\Delta TP/TP$	The percentage change in target price: the new target price minus the prior target price, scaled by the prior target price

$ \Delta TP/TP $	The absolute value of the percentage change in target price
<i>Log_Horizon</i>	The natural log of one plus the number of days between the fiscal year end date and one-year ahead EPS forecast date
<i>ROA</i>	Income before extraordinary items, scaled by total assets. Variable is measured using annual data
<i>MVE</i>	The stock price three days before the target price revision date times common shares outstanding (using annual data)
<i>BTM</i>	Total common equity (using annual data), scaled by <i>MVE</i>
<i>Num Analysts</i>	The number of analysts issuing earnings forecasts for a firm in a year
<i>Ret Std</i>	The standard deviation of daily stock prices over the 12 months prior to the target price revision date
<i>Momentum</i>	Cumulative buy-and-hold abnormal returns over the six months prior to the target price revision date. Abnormal returns are calculated using value-weighted returns

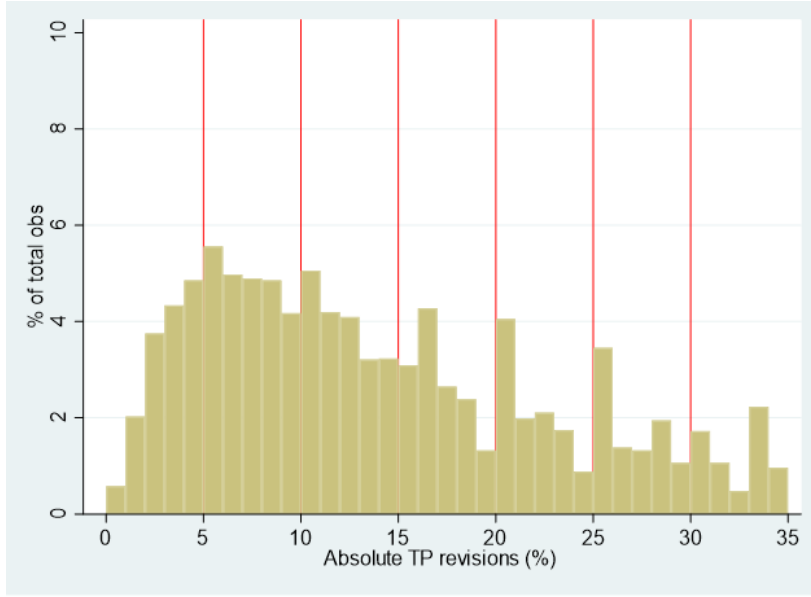
**Cross-Sectional Variables:**

<b>Variable</b>	<b>Definition</b>
<i>Above Median AUE</i>	An indicator variable equal to one if absolute unexpected earnings ( <i>AUE</i> ) is above the median <i>AUE</i> for the year, and zero otherwise. <i>AUE</i> is the absolute value of actual earnings per share minus the latest mean consensus forecast prior to the earnings announcement, scaled by the stock price at the fiscal quarter end date
<i>Above Median BankExp</i>	An indicator variable equal to one if the number of years the analyst has worked at the bank ( <i>BankExp</i> ) is above the median for the bank-year, and zero otherwise. <i>BankExp</i> is the number of years the analyst has issued forecasts (for any firm) at a bank

**Figure 1: Histograms and Discontinuity Tests for UBS around 10% Threshold**

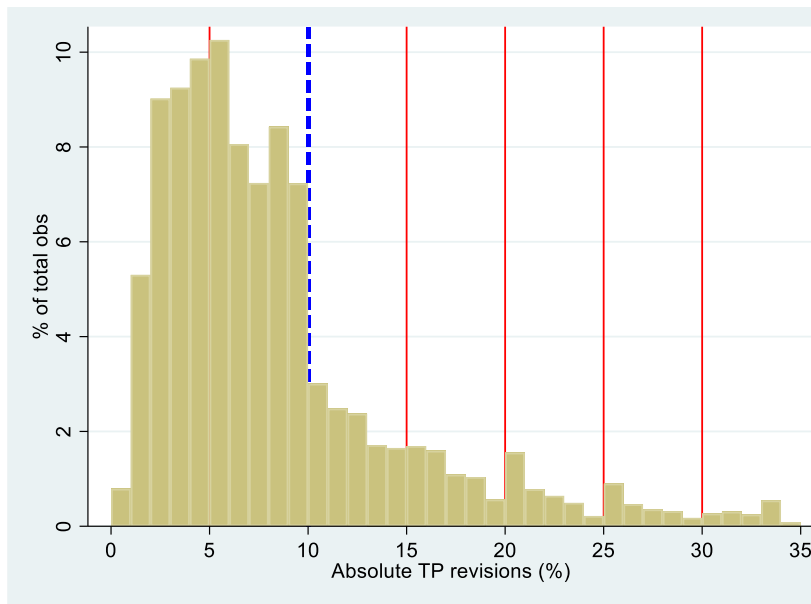
This figure presents histograms and McCrary (2008) tests of absolute target price revisions for UBS. Panel A presents the histogram for the pre-Global Settlement period (2000-2002) and Panel B presents the histogram for the post-Global Settlement period (2004-2006). A bold blue dotted line and “Yes” to  $t$ -stat < -3.09 indicate a significant negative discontinuity at that threshold.

**Panel A: UBS Pre-Global Settlement (2000-2002)**



Threshold	Log Diff. in Heights	Std. Error	$t$ -stat	$t$ -stat < -3.09?
10%	0.111	0.070	1.58	No

**Panel B: UBS Post-Global Settlement (2004-2006)**

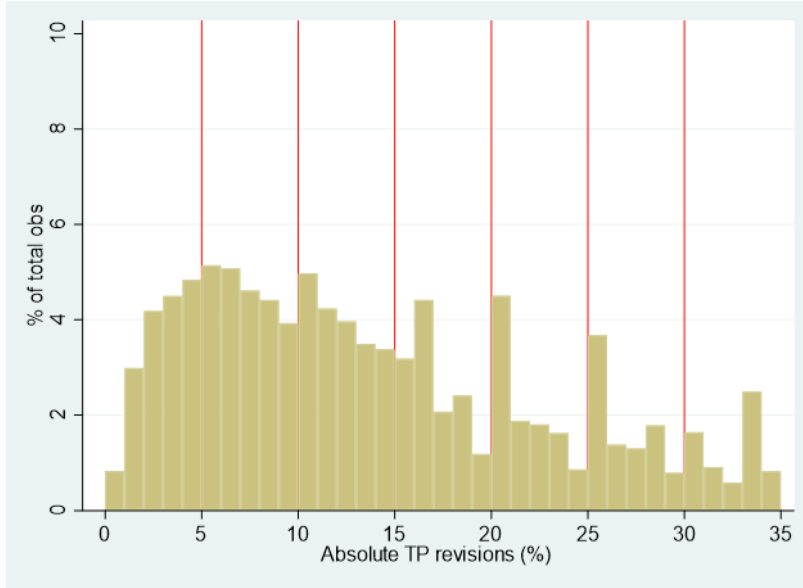


Threshold	Log Diff. in Heights	Std. Error	$t$ -stat	$t$ -stat < -3.09?
10%	-0.760	0.062	-12.32	Yes

**Figure 2: Histograms and Discontinuity Tests for Banks (excluding UBS) around 10% Threshold**

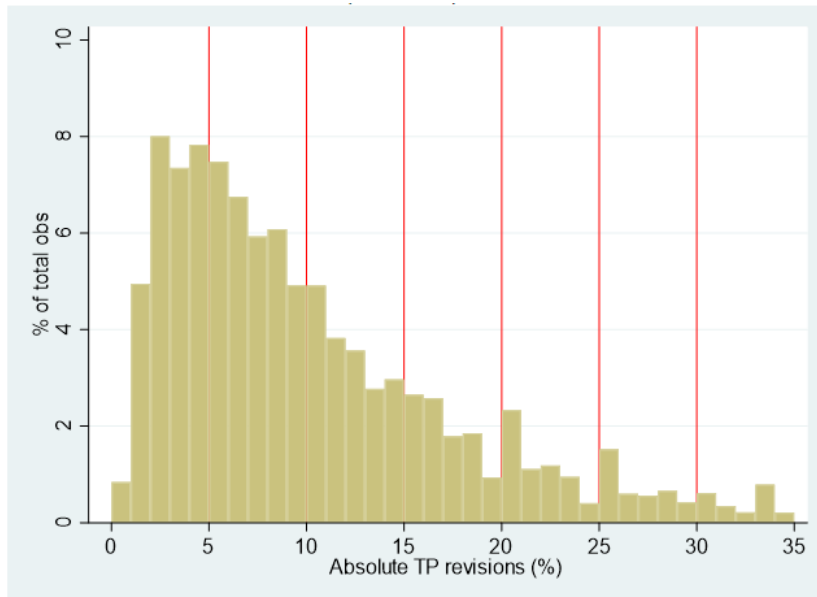
This figure presents pooled histograms and McCrary (2008) tests of absolute target price revisions for the sanctioned banks (excluding UBS). Panel A presents the histogram for the pre-Global Settlement period (2000-2002) and Panel B presents the histogram for the post-Global Settlement period (2004-2006). A bold blue dotted line and “Yes” to  $t$ -stat < -3.09 indicate a significant negative discontinuity at that threshold.

**Panel A: Sanctioned Banks (excluding UBS) Pre-Global Settlement (2000-2002)**



Threshold	Log Diff. in Heights	Std. Error	$t$ -stat	$t$ -stat < -3.09?
10%	0.182	0.029	6.33	No

**Panel B: Sanctioned Banks (excluding UBS) Post-Global Settlement (2004-2006)**

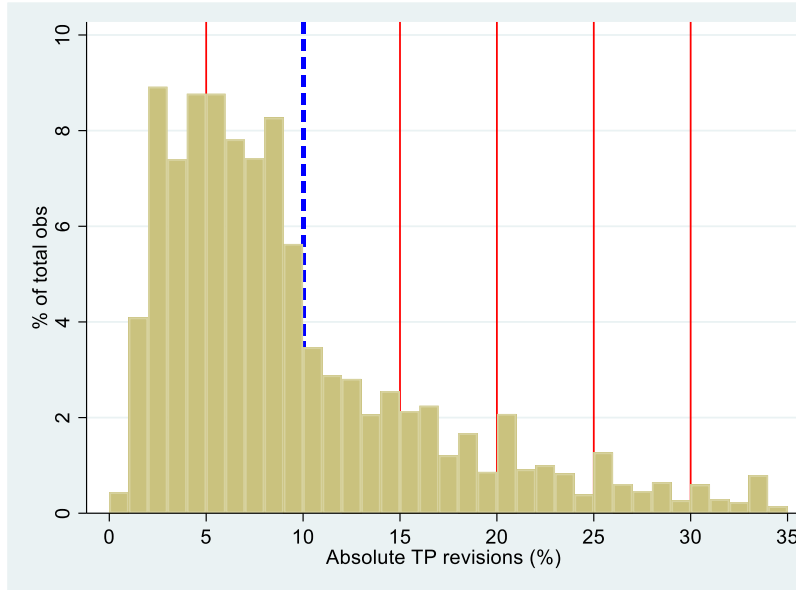


Threshold	Log Diff. in Heights	Std. Error	$t$ -stat	$t$ -stat < -3.09?
10%	0.108	0.029	3.69	No

**Figure 3: Histograms and Discontinuity Tests for Brightline Banks around 10% Threshold**

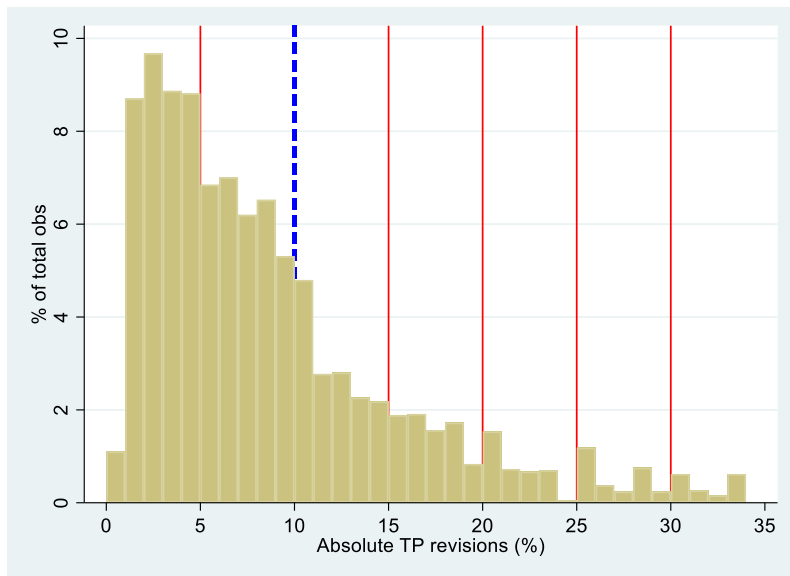
This figure presents histograms and McCrary (2008) tests of absolute target price revisions for Brightline banks in the post-Global Settlement period (2004-2006). Panel A presents the histogram for Lehman Brothers and Panel B presents the histogram for Morgan Stanley. A bold blue dotted line and “Yes” to  $t\text{-stat} < -3.09$  indicate a significant negative discontinuity at that threshold.

**Panel A: Lehman Brothers Post-Global Settlement (2004-2006)**



Threshold	Log Diff. in Heights	Std. Error	$t\text{-stat}$	$t\text{-stat} < -3.09?$
10%	-0.551	0.059	-9.29	<b>Yes</b>

**Panel B: Morgan Stanley Post-Global Settlement (2004-2006)**

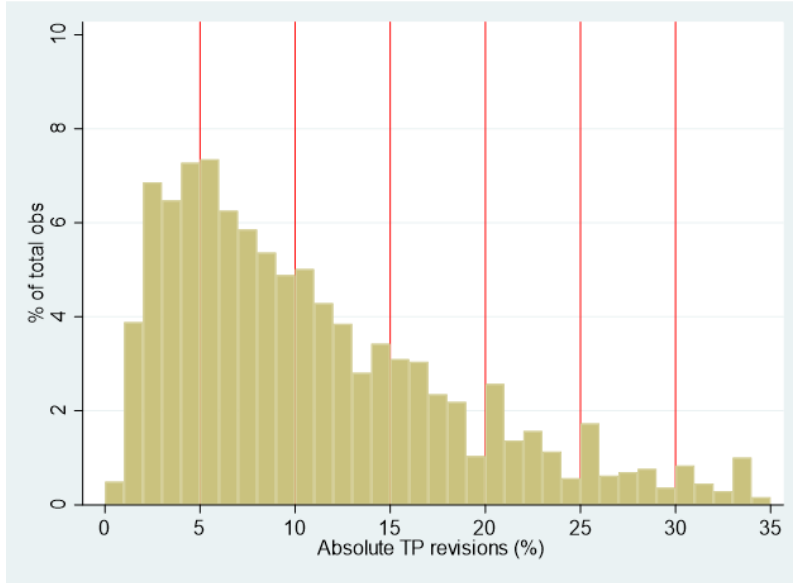


Threshold	Log Diff. in Heights	Std. Error	$t\text{-stat}$	$t\text{-stat} < -3.09?$
10%	-0.013	0.105	-0.13	No
10% (excluding 10%)	-0.732	0.129	-5.69	<b>Yes</b>

**Figure 4: Histograms and Discontinuity Tests for Non-Brightline Banks around 10% Threshold**

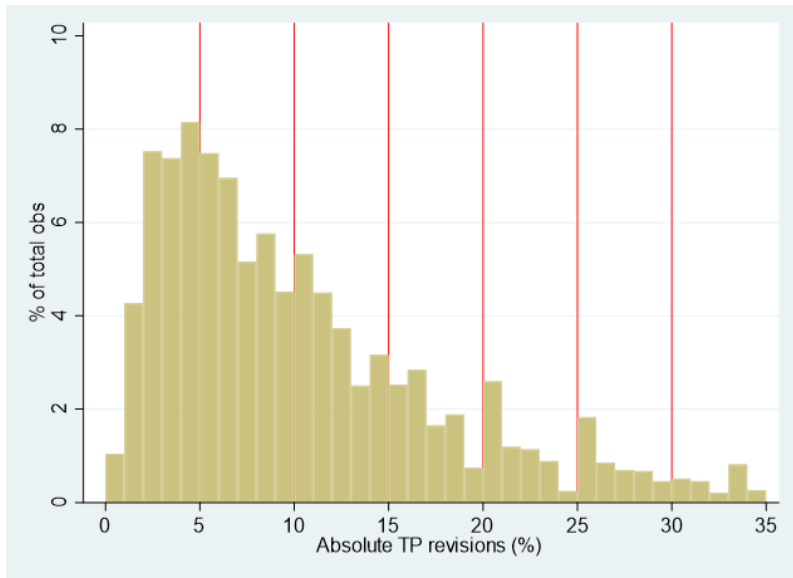
This figure presents histograms and McCrary (2008) tests of absolute target price revisions for Non-Brightline banks in the post-Global Settlement period (2004-2006). A bold blue dotted line and “Yes” to  $t$ -stat  $< -3.09$  indicate a significant negative discontinuity at that threshold.

**Panel A: Credit Suisse Post-Global Settlement (2004-2006)**



Threshold	Log Diff. in Heights	Std. Error	$t$ -stat	$t$ -stat $< -3.09$ ?
10%	0.132	0.072	1.84	No
10% (excluding 10%)	-0.140	0.083	-1.68	No

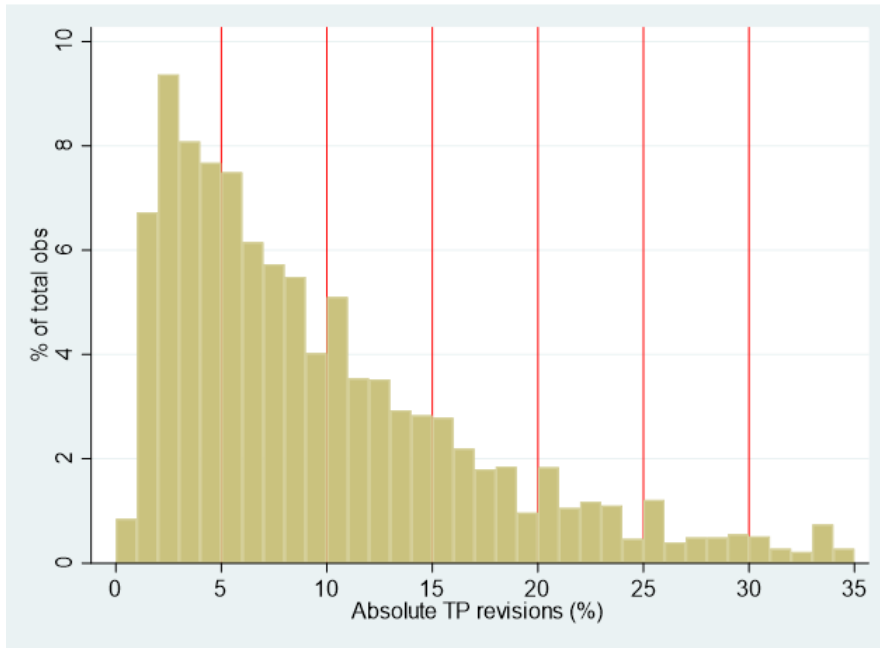
**Panel B: Deutsche Bank Post-Global Settlement (2004-2006)**



Threshold	Log Diff. in Heights	Std. Error	$t$ -stat	$t$ -stat $< -3.09$ ?
10%	0.361	0.082	4.42	No
10% (excluding 10%)	-0.044	0.075	-0.58	No

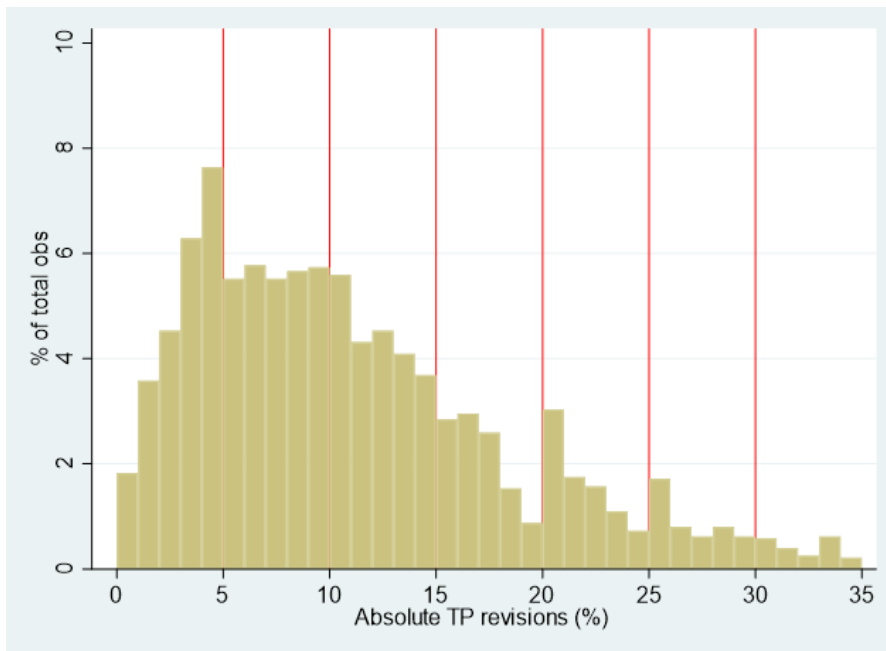


**Panel C: Citibank Post-Global Settlement (2004-2006)**



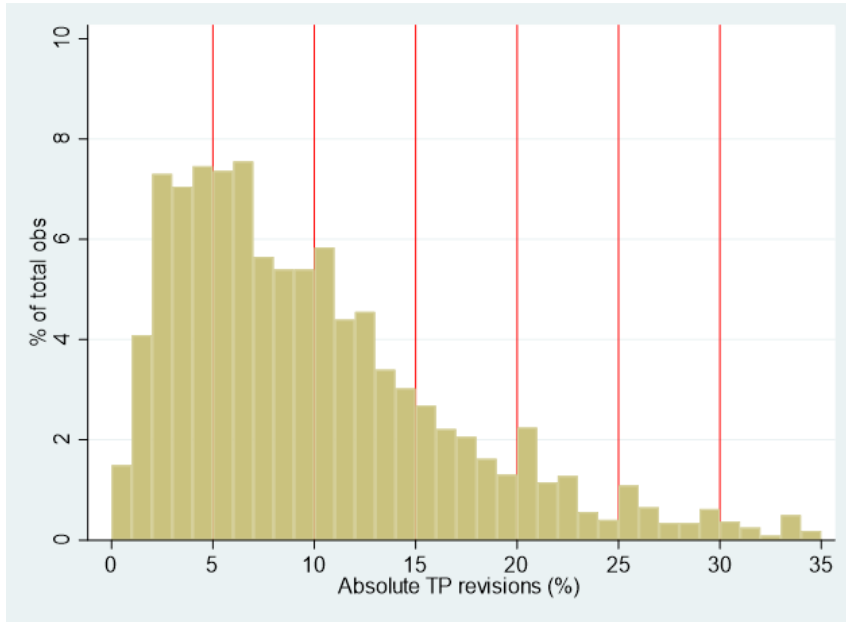
Threshold	Log Diff. in Heights	Std. Error	<i>t</i> -stat	<i>t</i> -stat < -3.09?
10%	0.346	0.074	4.71	No
10% (excluding 10%)	0.026	0.071	0.36	No

**Panel D: Merrill Lynch Post-Global Settlement (2004-2006)**



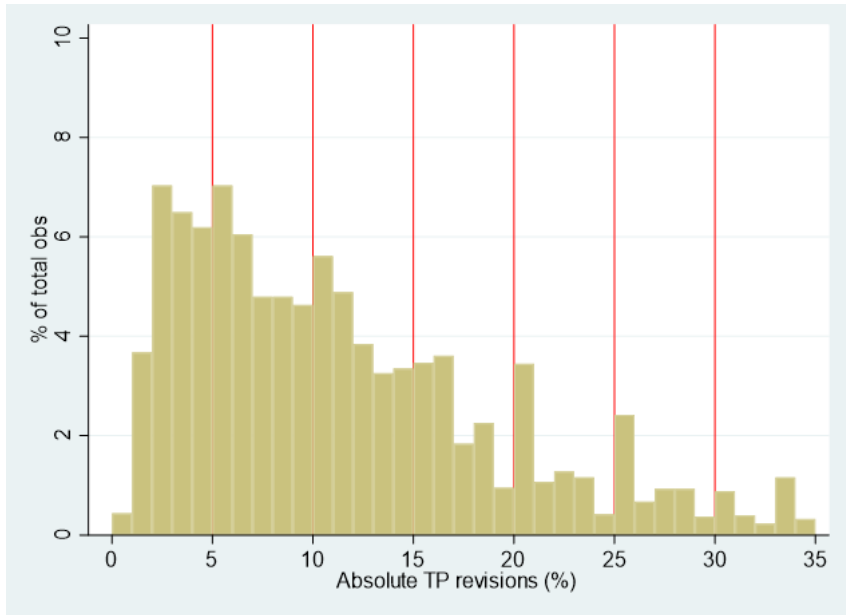
Threshold	Log Diff. in Heights	Std. Error	<i>t</i> -stat	<i>t</i> -stat < -3.09?
10%	0.062	0.099	0.62	No
10% (excluding 10%)	-0.288	0.109	-2.64	No

**Panel E: Bear Stearns Post-Global Settlement (2004-2006)**



Threshold	Log Diff. in Heights	Std. Error	<i>t</i> -stat	<i>t</i> -stat < -3.09?
10%	0.234	0.107	2.19	No
10% (excluding 10%)	-0.198	0.108	-1.84	No

**Panel F: Piper Jaffray Post-Global Settlement (2004-2006)**



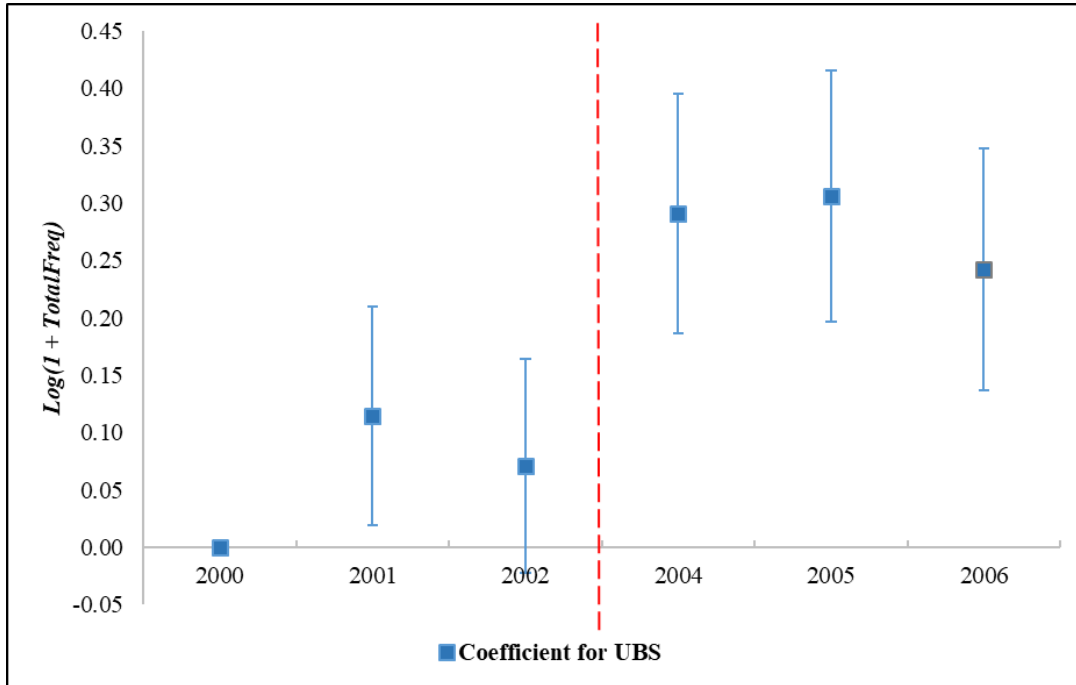
Threshold	Log Diff. in Heights	Std. Error	<i>t</i> -stat	<i>t</i> -stat < -3.09?
10%	0.335	0.083	4.05	No
10% (excluding 10%)	-0.072	0.096	-0.75	No

**Figure 5: Parallel Trends for Total Frequency of Target Price Revisions**

This figure presents results of tests of parallel trends in total frequency surrounding the Global Settlement. The coefficient estimates ( $\beta_t$ ) and their 95% confidence intervals are from the following model:

$$\begin{aligned} & \text{Log}(1 + \text{TotalFreq}_{ijkt}) \\ &= \sum_{\substack{t=2001 \\ t \neq 2003}}^{2006} \beta_t \text{Year}_t * \text{UBS}_j + \beta_2 \text{FirmExp}_{ikt} + \beta_3 \text{Log\_Coverage}_{jkt} \\ &+ \beta_4 \text{Log\_BrokerSize}_{jt} + \text{Analyst-Bank FE} + \text{Firm-Year FE} + \varepsilon_{ijkt} \end{aligned}$$

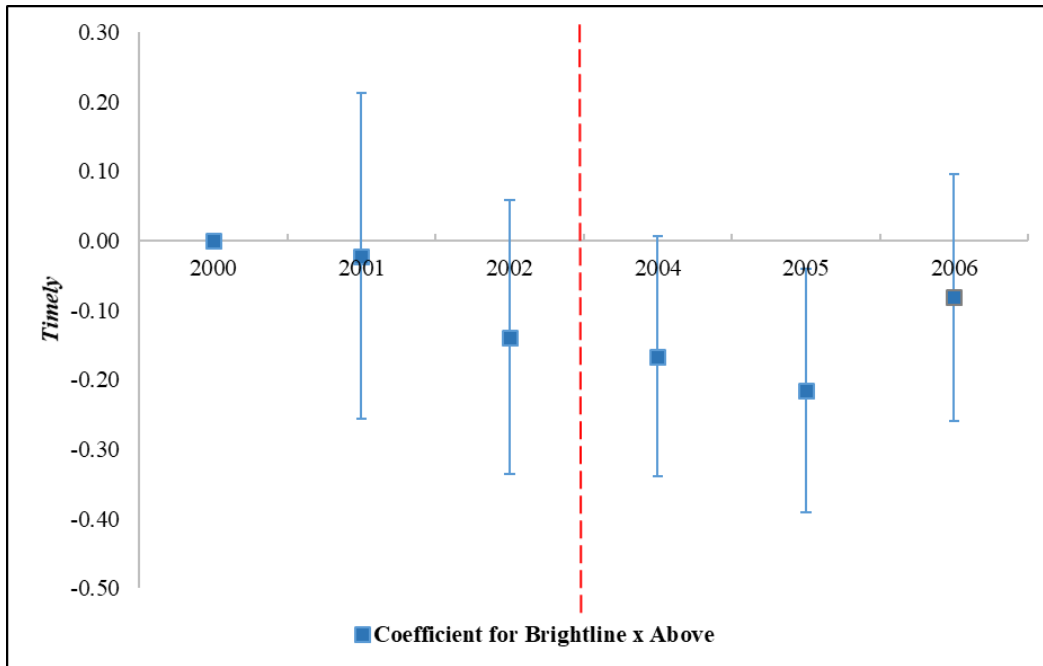
The indicator for 2000 is excluded as the benchmark group. Refer to Table 3 (Panel E) for the full regression results. All variables are defined in Appendix A.



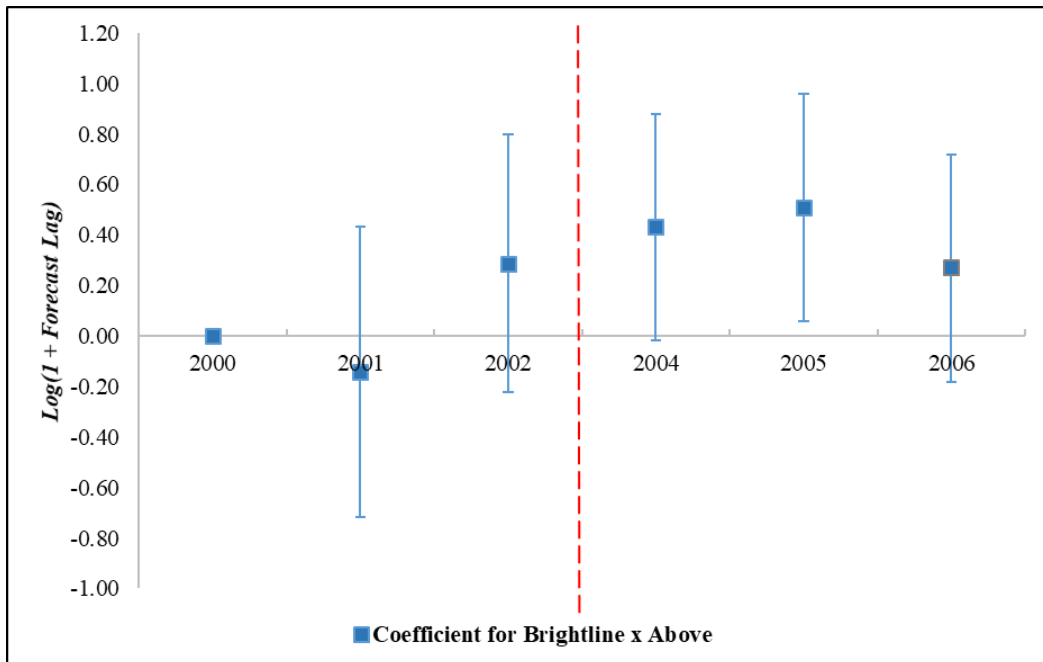
**Figure 6: Parallel Trends for Bundled Forecast Revision Timeliness**

This figure presents results of tests of parallel trends in bundled forecast revision timeliness surrounding the Global Settlement. The coefficient estimates ( $\beta_t$ ) and their 95% confidence intervals are from the triple interaction of individual year indicators and *Brightline x Above*. The indicator for 2000 is excluded as the benchmark group. Panel A presents the results for *Timely* and Panel B presents the results for *Log(1 + Forecast Lag)*. All variables are defined in Appendix A.

**Panel A: Parallel Trends for *Timely***



**Panel B: Parallel Trends for *Log(1 + Forecast Lag)***



**Table 1: Sample Selection**

This table reports sample selection criteria used for the three main samples used in this study. Panel A reports sample selection criteria for the full sample of target price revisions, Panel B reports sample selection criteria for the target price frequency sample, and Panel C reports selection criteria for the target price timeliness sample.

**Panel A: Full target price revision sample**

<b>Sample Selection Criteria</b>	<b>Dropped Obs.</b>	<b>Sample Size</b>
IBES target price (TP) revisions issued between 2000-2002 and 2004-2006		285,550
Keep: observations from the nine sanctioned banks with non-missing data	(178,121)	107,429
Drop: observations above 99th percentile for days between TPs	(1,072)	106,357
Drop: observations below (above) 1st (99th) percentile for $\Delta TP/TP$	(2,122)	104,235
Drop: observations with missing firm identifiers (permno and gvkey)	(2,026)	102,209
Drop: observations with concurrent stock recommendation revisions	(8,298)	93,911
<b>Number of TP revisions used in discontinuity tests</b>		<b>93,911</b>
Drop: observations with absolute TP revisions equal to 10%	(1,815)	92,096
Drop: firm-year observations with fewer than two TP revisions	(2,322)	89,774
Keep: firm observations with TP revisions by Brightline and Non-Brightline banks in pre- and post-periods	(13,249)	76,525
Drop: observations with missing analyst and bank control variables	(6,018)	70,507
Drop: observations above 99th percentile for <i>Forecast Lag</i>	(146)	70,361
Drop: observations with TP/P ratio in bottom one percentile or TP/P ratio greater than 4	(2,206)	68,155
Drop: observations missing valid one-year ahead stock price following TP revision date	(2,374)	65,781
Drop: observations with missing firm control variables	(2,183)	63,598
Drop: singleton observations from fixed effect structure	(257)	63,341
<b>Final number of TP revisions in full sample</b>		<b>63,341</b>
Number of unique firms		1,983
Number of unique analysts		1,807

**Panel B: Target price frequency sample**

<b>Sample Selection Criteria</b>	<b>Dropped Obs.</b>	<b>Sample Size</b>
Full sample of TP revisions from Panel A		63,341
Keep: one observation per analyst-bank-firm-year	(37,645)	25,696
Drop: singleton observations from fixed effect structure	(1,324)	24,372
<b>Final number of observations in TP frequency sample (at analyst-bank-firm-year level)</b>		<b>24,372</b>
Number of unique firms		1,983
Number of unique analysts		1,807

**Panel C: Target price timeliness sample**

<b>Sample Selection Criteria</b>	<b>Dropped Obs.</b>	<b>Sample Size</b>
Full sample of TP revisions from Panel A		63,341
Keep: TP revisions "bundled" with first EPS forecast after firms' earnings announcements	(47,478)	15,863
Drop: singleton observations from fixed effect structure	(1,963)	13,900
<b>Final number of observations in TP timeliness sample (bundled forecast unit of observation)</b>		<b>13,900</b>
Number of unique firms		1,699
Number of unique analysts		1,255

**Table 2: Descriptive Statistics**

This table reports descriptive statistics of target price revision observations from 2000-2002 and 2004-2006. Panel A reports statistics for the target price frequency sample, Panel B reports statistics for the target price timeliness sample, and Panel C reports statistics for the full sample of target price revisions. Refer to Appendix A for variable definitions. Unlogged version of variables (e.g., *TotalFreq*) are reported for ease of interpretation.

**Panel A: Target price frequency sample**

<b>Variables</b>	<b>N</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>P25</b>	<b>P50</b>	<b>P75</b>
<i>TotalFreq</i>	24,372	2.61	1.74	1.00	2.00	3.00
<i>AboveFreq</i>	24,372	1.22	1.16	0.00	1.00	2.00
<i>BelowFreq</i>	24,372	1.39	1.59	0.00	1.00	2.00
<i>Post</i>	24,372	0.56	0.50	0.00	1.00	1.00
<i>UBS</i>	24,372	0.15	0.35	0.00	0.00	0.00
<i>FirmExp</i>	24,372	4.70	5.31	1.00	3.00	6.00
<i>Coverage</i>	24,372	16.78	9.11	11.00	15.00	20.00
<i>BrokerSize</i>	24,372	170.41	75.52	127.00	153.00	196.00

**Panel B: Target price timeliness sample**

<b>Variables</b>	<b>N</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>P25</b>	<b>P50</b>	<b>P75</b>
<i>Timely</i>	13,900	0.69	0.46	0.00	1.00	1.00
<i>Forecast Lag</i>	13,900	8.14	15.71	1.00	1.00	2.00
<i>Post</i>	13,900	0.72	0.45	0.00	1.00	1.00
<i>Brightline</i>	13,900	0.39	0.49	0.00	0.00	1.00
<i>Above</i>	13,900	0.45	0.50	0.00	0.00	1.00
<i>FirmExp</i>	13,900	4.92	5.38	1.00	3.00	6.00
<i>Coverage</i>	13,900	17.14	7.79	12.00	16.00	21.00
<i>BrokerSize</i>	13,900	148.88	54.04	120.00	137.00	169.00
<i>AUE</i>	13,900	0.003	0.008	0.000	0.001	0.002
<i>Above Median AUE</i>	13,900	0.49	0.50	0.00	0.00	1.00
<i>BankExp</i>	13,900	7.87	6.31	3.00	6.00	12.00
<i>Above Median BankExp</i>	13,900	0.52	0.50	0.00	1.00	1.00
$\Delta TP/TP$	13,900	0.014	0.167	-0.075	0.033	0.097
$ \Delta TP/TP $	13,900	0.125	0.112	0.048	0.087	0.167

**Panel C: Full target price revision sample**

<b>Variables</b>	<b>N</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>P25</b>	<b>P50</b>	<b>P75</b>
<i>Six-Month TP Optimism</i>	63,341	0.23	0.44	-0.03	0.15	0.39
<i>Six-Month Abs TP Error</i>	63,341	0.34	0.37	0.10	0.22	0.43
<i>Post</i>	63,341	0.59	0.49	0.00	1.00	1.00
<i>Brightline</i>	63,341	0.44	0.50	0.00	0.00	1.00
<i>Above</i>	63,341	0.47	0.50	0.00	0.00	1.00
<i>FirmExp</i>	63,341	5.03	5.41	1.00	3.00	7.00
<i>Coverage</i>	63,341	17.12	9.70	12.00	15.00	20.00
<i>BrokerSize</i>	63,341	167.42	71.69	127.00	151.00	196.00
<i><math>\Delta TP/TP</math></i>	63,341	0.01	0.18	-0.08	0.04	0.11
<i>ROA</i>	63,341	0.05	0.09	0.02	0.05	0.09
<i>MVE</i>	63,341	14642.59	27909.79	1850.72	4907.03	13486.24
<i>BTM</i>	63,341	0.42	0.31	0.21	0.35	0.54
<i>Num Analysts</i>	63,341	21.57	10.17	14.00	20.00	28.00
<i>Ret Std</i>	63,341	0.03	0.01	0.02	0.02	0.03
<i>Momentum</i>	63,341	0.06	0.29	-0.12	0.04	0.20



**Table 3: Target Price Revision Frequency**

This table presents results from tests of the effect of the use of a bright-line threshold on the frequency of target price revisions. Panels A, B, and C present results from estimating the following model:

$$\begin{aligned} & \text{Log}(1 + \text{AboveFreq}_{ijkt}) \text{ or } \text{Log}(1 + \text{BelowFreq}_{ijkt}) \text{ or } \text{Log}(1 + \text{TotalFreq}_{ijkt}) \\ & = \beta_1 \text{Post}_t * \text{UBS}_j + \beta_2 \text{FirmExp}_{ikt} + \beta_3 \text{Log\_Coverage}_{jkt} + \beta_4 \text{Log\_BrokerSize}_{jt} \\ & + \text{Analyst-Bank FE} + \text{Firm-Year FE} + \varepsilon_{ijkt} \end{aligned}$$

Refer to Appendix A for variable definitions. t-statistics are reported in parentheses, and standard errors are double clustered by analyst-bank and by firm. p-values are two-tailed, and \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

**Panel A: Regression results for frequency of absolute target price revisions above 10% threshold**

	Pr. Sign	<i>Log(1 + AboveFreq)</i>		
		(1)	(2)	(3)
<i>Post</i>		-0.236*** (-15.741)		
<i>UBS</i>		0.043 (1.322)		
<i>Post x UBS</i>	-	-0.121*** (-2.898)	-0.136*** (-3.941)	-0.136*** (-3.937)
<i>FirmExp</i>				0.003*** (3.448)
<i>Log_Coverage</i>				0.075*** (4.138)
<i>Log_BrokerSize</i>				-0.023 (-0.571)
Analyst-Bank FE		No	Yes	Yes
Firm-Year FE		No	Yes	Yes
Observations		24372	24372	24372
Adj. R-sq		0.064	0.378	0.379

**Panel B: Regression results for frequency of absolute target price revisions below 10% threshold**

	Pr. Sign	<i>Log(1 + BelowFreq)</i>		
		(1)	(2)	(3)
<i>Post</i>		0.303*** (17.816)		
<i>UBS</i>		0.026 (0.716)		
<i>Post x UBS</i>	+	0.421*** (9.960)	0.378*** (9.327)	0.364*** (8.668)
<i>FirmExp</i>				0.003*** (3.483)
<i>Log_Coverage</i>				0.103*** (5.067)
<i>Log_BrokerSize</i>				0.117** (2.401)
Analyst-Bank FE		No	Yes	Yes
Firm-Year FE		No	Yes	Yes
Observations		24372	24372	24372
Adj. R-sq		0.136	0.427	0.429

**Panel C: Regression results for total frequency of target price revisions**

	Pr. Sign	<i>Log(1 + TotalFreq)</i>		
		(1)	(2)	(3)
<i>Post</i>		0.048*** (3.763)		
<i>UBS</i>		0.051* (1.812)		
<i>Post x UBS</i>	+/0	0.239*** (6.660)	0.209*** (5.997)	0.200*** (5.585)
<i>FirmExp</i>				0.004*** (5.774)
<i>Log_Coverage</i>				0.136*** (7.012)
<i>Log_BrokerSize</i>				0.051 (1.195)
Analyst-Bank FE		No	Yes	Yes
Firm-Year FE		No	Yes	Yes
Observations		24372	24372	24372
Adj. R-sq		0.042	0.321	0.327

**Panel D: Parallel trends for total frequency of target price revisions**

	Pr. Sign	<i>Log(1 + TotalFreq)</i>	
		(1)	(2)
<i>2001 x UBS</i>	0	0.107** (2.228)	0.114** (2.359)
<i>2002 x UBS</i>	0	0.055 (1.131)	0.071 (1.501)
<i>2004 x UBS</i>	+	0.287*** (5.395)	0.291*** (5.478)
<i>2005 x UBS</i>	+	0.300*** (5.398)	0.306*** (5.497)
<i>2006 x UBS</i>	+	0.233*** (4.335)	0.242*** (4.515)
<i>FirmExp</i>			0.004*** (5.742)
<i>Log_Coverage</i>			0.137*** (7.052)
<i>Log_BrokerSize</i>			0.020 (0.418)
Analyst-Bank FE		Yes	Yes
Firm-Year FE		Yes	Yes
Observations		24372	24372
Adj. R-sq		0.322	0.328

**Table 4: Bundled Forecast Revision Timeliness**

This table presents results on the effect of use of a bright-line threshold on bundled forecast revision timeliness from estimating the following model:

$$\begin{aligned}
 & \text{Timely}_{ijkt} \text{ or } \text{Log}(1 + \text{Forecast Lag}_{ijkt}) \\
 & = \beta_1 \text{Post}_t * \text{Brightline}_j + \beta_2 \text{Above}_{ijkt} + \beta_3 \text{Post}_t * \text{Above}_{ijkt} + \beta_4 \text{Brightline}_j * \text{Above}_{ijkt} + \beta_5 \text{Post}_t * \text{Brightline}_j \\
 & \quad * \text{Above}_{ijkt} + \beta_6 \text{FirmExp}_{ikt} + \beta_7 \text{Log\_Coverage}_{jkt} + \beta_8 \text{Log\_BrokerSize}_{jt} + \text{Analyst-Bank FE} + \text{Firm-Year FE} + \varepsilon_{ijkt}
 \end{aligned}$$

Refer to Appendix A for variable definitions. t-statistics are reported in parentheses, and standard errors are double clustered by analyst-bank and by firm. p-values are two-tailed, and \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Pr. Sign	Timely			Log(1 + Forecast Lag)		
		(1)	(2)	(3)	(4)	(5)	(6)
<i>Post</i>		0.137*** (5.547)			-0.495*** (-7.348)		
<i>Brightline</i>		0.007 (0.222)			-0.073 (-0.899)		
<i>Post x Brightline x Above</i>	-,+	-0.042 (-0.942)	-0.080* (-1.731)	-0.081* (-1.743)	0.239** (2.039)	0.305** (2.514)	0.307** (2.524)
<i>Post x Brightline</i>		-0.043 (-1.174)	0.012 (0.251)	0.012 (0.260)	0.119 (1.275)	-0.038 (-0.317)	-0.039 (-0.322)
<i>Above</i>		0.000 (0.019)	-0.023 (-0.902)	-0.023 (-0.918)	0.078 (1.210)	0.153** (2.250)	0.154** (2.273)
<i>Post x Above</i>		-0.017 (-0.643)	-0.015 (-0.516)	-0.014 (-0.504)	0.011 (0.156)	-0.042 (-0.555)	-0.043 (-0.569)
<i>Brightline x Above</i>		-0.031 (-0.875)	0.014 (0.344)	0.015 (0.351)	0.036 (0.408)	-0.090 (-0.857)	-0.092 (-0.869)
<i>FirmExp</i>				-0.001 (-1.064)			0.003 (0.826)
<i>Log_Coverage</i>				0.010 (0.358)			-0.041 (-0.503)
<i>Log_BrokerSize</i>				-0.037 (-0.705)			0.140 (0.993)

Analyst-Bank FE	No	Yes	Yes	No	Yes	Yes
Firm-Year FE	No	Yes	Yes	No	Yes	Yes
Observations	13900	13900	13900	13900	13900	13900
Adj. R-sq	0.017	0.261	0.260	0.031	0.254	0.253

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**Table 5: Cross-Sectional Timeliness Tests**

This table presents results from cross-sectional tests of the effect of the use of a bright-line threshold on bundled forecast revision timeliness. Columns 1 and 2 present results for earnings news (Brightline only) and Columns 3 and 4 present results for analyst tenure (Brightline only). Refer to Appendix A for variable definitions. t-statistics are reported in parentheses, and standard errors are double clustered by analyst-bank and by firm. p-values are two-tailed, and \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Pr. Sign	<i>Log(1 + Forecast Lag)</i>			
		<i>Above Med</i>		<i>Above Med</i>	
		<i>AUE=1</i>	<i>AUE=0</i>	<i>BankExp=1</i>	<i>BankExp=0</i>
		(1)	(2)	(3)	(4)
<i>Post x Above</i>	0,+,+,0	-0.025	0.467***	0.383***	0.073
		(-0.182)	(3.170)	(3.065)	(0.527)
[p-value]		[0.010]**		[0.098]*	
<i>Above</i>		0.114	0.092	0.035	0.134
		(0.967)	(0.789)	(0.377)	(1.204)
<i>FirmExp</i>		-0.011	0.005	0.006	-0.007
		(-1.344)	(0.453)	(0.608)	(-0.873)
<i>Log_Coverage</i>		-0.173	-0.039	-0.094	-0.059
		(-0.751)	(-0.220)	(-0.506)	(-0.693)
<i>Log_BrokerSize</i>		0.066	-0.752**	-0.542*	-0.071
		(0.207)	(-2.064)	(-1.781)	(-0.236)
Analyst-Bank FE		Yes	Yes	No	No
Bank FE		No	No	Yes	Yes
Firm FE		Yes	Yes	Yes	Yes
Year FE		Yes	Yes	Yes	Yes
Observations		2337	2342	2557	2393
Adj. R-sq		0.180	0.181	0.132	0.146

**Table 6: Target Price Optimism and Accuracy**

This table presents results from tests of the effect of the use of a bright-line threshold on target price optimism and accuracy. Panels A and B present results from estimating the following model:

$$\begin{aligned}
 & \text{Six-Month TP Optimism}_{ijkt} \text{ or Six-Month Abs TP Error}_{ijkt} \\
 & = \beta_1 \text{Post}_t * \text{Brightline}_j + \beta_2 \text{Above}_{ijkt} + \beta_3 \text{Post}_t * \text{Above}_{ijkt} + \beta_4 \text{Brightline}_j \\
 & * \text{Above}_{ijkt} + \beta_5 \text{Post}_t * \text{Brightline}_j * \text{Above}_{ijkt} + \beta_6 \Delta TP/TP_{ijkt} + \beta_7 \text{FirmExp}_{ikt} \\
 & + \beta_8 \text{Log\_Coverage}_{jkt} + \beta_9 \text{Log\_BrokerSize}_{jt} + \beta_{10} \text{ROA}_{it} + \beta_{11} \text{Log\_MVE}_{it} \\
 & + \beta_{12} \text{BTM}_{it} + \beta_{13} \text{LogNumAnalysts}_{it} + \beta_{14} \text{Ret Std}_{it} + \beta_{15} \text{Momentum}_{it} \\
 & + \text{Analyst-Bank FE} + \text{Firm FE} + \text{Year FE} + \varepsilon_{ijkt}
 \end{aligned}$$

Refer to Appendix A for variable definitions. t-statistics are reported in parentheses, and standard errors are double clustered by analyst-bank and by firm. p-values are two-tailed, and \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

**Panel A: Regression results for target price optimism**

	Pr. Sign	Six-Month TP Optimism		
		(1)	(2)	(3)
<i>Post</i>		-0.217*** (-11.775)		
<i>Brightline</i>		-0.053*** (-2.671)		
<i>Post x Brightline x Above</i>	-/0	-0.000 (-0.020)	0.001 (0.057)	0.017 (1.277)
<i>Post x Brightline</i>		0.002 (0.091)	-0.021 (-1.222)	-0.024 (-1.354)
<i>Above</i>		0.021 (1.470)	-0.027*** (-3.440)	-0.016** (-2.219)
<i>Post x Above</i>		-0.039** (-2.425)	0.003 (0.338)	-0.007 (-0.832)
<i>Brightline x Above</i>		0.048** (2.566)	0.004 (0.346)	-0.005 (-0.485)
<i>ΔTP/TP</i>				0.148*** (11.754)
<i>FirmExp</i>				0.001* (1.734)
<i>Log_Coverage</i>				0.001 (0.118)
<i>Log_BrokerSize</i>				0.002 (0.088)
<i>ROA</i>				0.008 (0.110)
<i>Log_MVE</i>				0.240*** (18.533)

<i>BTM</i>			-0.012 (-0.524)
<i>Log_Num Analysts</i>			0.030 (1.579)
<i>Ret Std</i>			3.109*** (4.632)
<i>Momentum</i>			-0.179*** (-12.037)
Analyst-Bank FE	No	Yes	Yes
Firm FE	No	Yes	Yes
Year FE	No	Yes	Yes
Observations	63341	63341	63341
Adj. R-sq	0.077	0.583	0.623

**Panel B: Regression results for target price accuracy**

	Pr. Sign	<i>Six-Month Abs TP Error</i>		
		(1)	(2)	(3)
<i>Post</i>		-0.176*** (-11.138)		
<i>Brightline</i>		-0.050*** (-2.838)		
<i>Post x Brightline x Above</i>	-/0	-0.025 (-1.307)	-0.015 (-1.366)	-0.009 (-0.823)
<i>Post x Brightline</i>		0.033* (1.711)	-0.001 (-0.040)	-0.002 (-0.201)
<i>Above</i>		0.066*** (5.222)	-0.003 (-0.549)	-0.008 (-1.357)
<i>Post x Above</i>		-0.045*** (-3.245)	-0.017** (-2.273)	-0.011 (-1.581)
<i>Brightline x Above</i>		0.047*** (2.823)	0.011 (1.171)	0.008 (0.821)
$\Delta TP/TP$				0.095*** (9.094)
<i>FirmExp</i>				0.000 (0.722)
<i>Log_Coverage</i>				0.008 (1.122)
<i>Log_BrokerSize</i>				0.017 (1.004)
<i>ROA</i>				-0.024 (-0.440)
<i>Log_MVE</i>				0.093***



			(9.625)
<i>BTM</i>			0.021
			(1.252)
<i>Log_Num Analysts</i>			0.015
			(1.069)
<i>Ret Std</i>			3.305***
			(7.059)
<i>Momentum</i>			-0.150***
			(-14.238)
<hr/>			
Analyst-Bank FE	No	Yes	Yes
Firm FE	No	Yes	Yes
Year FE	No	Yes	Yes
Observations	63341	63341	63341
Adj. R-sq	0.083	0.643	0.657
<hr/>			

**Table 7: EPS Forecast Optimism and Accuracy**

This table presents results from tests of the effect of the use of a bright-line threshold on EPS forecast optimism and accuracy. Panels A and B present results from estimating the following model:

$$\begin{aligned}
 & \text{EPS Optimism}_{ijkt} \text{ or EPS Abs Forecast Error}_{ijkt} \\
 & = \beta_1 \text{Post}_t * \text{Brightline}_j + \beta_2 \text{Above}_{ijkt} + \beta_3 \text{Post}_t * \text{Above}_{ijkt} + \beta_4 \text{Brightline}_j \\
 & * \text{Above}_{ijkt} + \beta_5 \text{Post}_t * \text{Brightline}_j * \text{Above}_{ijkt} + \beta_6 \text{FirmExp}_{ikt} \\
 & + \beta_7 \text{Log\_Coverage}_{jkt} + \beta_8 \text{Log\_BrokerSize}_{jt} + \text{Analyst-Bank FE} + \text{Firm-Year FE} \\
 & + \varepsilon_{ijkt}
 \end{aligned}$$

Refer to Appendix A for variable definitions. t-statistics are reported in parentheses, and standard errors are double clustered by analyst-bank and by firm. p-values are two-tailed, and \*, \*\*, and \*\*\* indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

**Panel A: Regression results for EPS forecast optimism**

	Pr. Sign	<i>EPS Optimism</i>		
		(1)	(2)	(3)
<i>Post</i>		-0.397*** (-2.630)		
<i>Brightline</i>		-0.106 (-0.854)		
<i>Post x Brightline x Above</i>	-/0	-0.354 (-1.554)	-0.196** (-2.045)	-0.183* (-1.929)
<i>Post x Brightline</i>		0.082 (0.477)	0.079 (1.038)	0.073 (0.975)
<i>Above</i>		0.314*** (2.950)	-0.119** (-2.123)	-0.106* (-1.929)
<i>Post x Above</i>		0.012 (0.080)	0.042 (0.572)	0.020 (0.270)
<i>Brightline x Above</i>		0.427** (2.304)	0.090 (1.347)	0.083 (1.254)
<i>FirmExp</i>				-0.001 (-0.378)
<i>Log_Coverage</i>				0.016 (0.186)
<i>Log_BrokerSize</i>				0.027 (0.189)
<i>Log_Horizon</i>				0.231*** (5.791)
Analyst-Bank FE		No	Yes	Yes
Firm-Year FE		No	Yes	Yes
Observations		30743	30743	30743
Adj. R-sq		0.009	0.734	0.736

**Panel B: Regression results for EPS forecast accuracy**

	Pr. Sign	<i>EPS Abs Forecast Error</i>		
		(1)	(2)	(3)
<i>Post</i>		-0.211 (-1.119)		
<i>Brightline</i>		-0.090 (-0.722)		
<i>Post x Brightline x Above</i>	-/0	-0.268 (-1.061)	-0.111 (-1.242)	-0.072 (-0.836)
<i>Post x Brightline</i>		0.169 (1.087)	0.182** (2.267)	0.165** (2.120)
<i>Above</i>		0.634*** (4.357)	-0.076 (-1.419)	-0.036 (-0.701)
<i>Post x Above</i>		0.063 (0.327)	0.017 (0.250)	-0.050 (-0.768)
<i>Brightline x Above</i>		0.437* (1.955)	0.108 (1.542)	0.087 (1.285)
<i>FirmExp</i>				-0.001 (-0.276)
<i>Log_Coverage</i>				0.074 (1.067)
<i>Log_BrokerSize</i>				-0.053 (-0.478)
<i>Log_Horizon</i>				0.682*** (14.782)
Analyst-Bank FE		No	Yes	Yes
Firm-Year FE		No	Yes	Yes
Observations		30743	30743	30743
Adj. R-sq		0.010	0.808	0.820