Analyst Coverage and Real Earnings Management: Quasi-Experimental Evidence*

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Abstract

We study how securities analysts influence managers' use of different types of earnings management. To isolate causality, we employ a quasi-experiment that exploits exogenous reductions in analyst following resulting from brokerage house mergers. We find that managers respond to the coverage loss by decreasing real earnings management, while increasing accrual manipulation. These effects are significantly stronger among firms with less coverage and for firms close to the zero-earnings threshold. Our causal evidence suggests that managers use real earnings management to enhance short-term performance in response to analyst pressure, effects that are not uncovered when focusing solely on accrual-based methods.

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1. Introduction

Do the recommendations and short-term earnings benchmarks emphasized by securities analysts pressure managers to manipulate reported earnings?[1] Firms failing to meet or beat quarterly expectations experience a loss of stock market valuation (Bartov et al., 2002). Managers of these firms experience declines in compensation (Matsunaga and Park, 2001) and a greater likelihood of turnover (Hazarika et al., 2012; Mergenthaler et al., 2012). Given these expected private costs to managers, a large literature emphasizes analysts' role in pressuring managers and in decreasing overall transparency.[2]

On the other hand, do securities analysts serve as effective external monitors? As accounting and finance professionals with industry expertise, analysts process and disseminate information disclosed by firms in financial statements and other sources as well as scrutinizing management during conference calls. Dyck et al. (2010) document the important role analysts play as whistle blowers, who are often the first to detect corporate fraud. In light of the adverse wealth, reputation, and career consequences management experience in the wake of such incidents (Karpoff et al., 2008a), an alternative view is that analysts deter misreporting and discipline managerial misbehavior by serving as monitors alongside traditional mechanisms of corporate governance (e.g., Yu, 2008).

These issues are at the center of a divisive debate over how analysts impact managers' behavior and whether they have a positive effect on firm value, relationships that have not yet been clearly established in the literature and warrant further research (Leuz, 2003). Moreover, understanding the causes of earnings manipulation is of particular importance, given the substantial direct adverse consequences of misreporting (Karpoff et al., 2008a,b),

[1] Earnings manipulation is suitably defined as follows: "Earnings management occurs when managers use judgment in financial reporting and in structuring transactions to alter financial reports to either mislead some stakeholders about the underlying economic performance of the company or to influence contractual outcomes that depend on reported accounting practices." (Healy and Wahlen, 1999, p.6).

[2] For example, see Fuller and Jensen (2002), Dechow et al. (2003), and Grundfest and Malenko (2012).

as well as potential macroeconomic distortions—excessive hiring and investment—that could accompany overstated performance (Kedia and Philippon, 2009).

In this paper, we examine how securities analysts impact managers' incentives to engage in earnings management activities. We follow a recent earnings management literature that proposes "real activities manipulation"—changing investments, advertising, or the timing and structure of operational activities—as a natural alternative to accrual-based methods (e.g., Chen and Huang, 2013; Cohen et al., 2008; Roychowdhury, 2006; Zang, 2012).[3,4] Our analysis expands the scope of previous studies on the impact of analysts on earnings management by incorporating real activities manipulation as an alternative earnings management mechanism. We argue that by focusing on one earnings management technique in isolation (e.g., accrual-based methods), it is not possible to provide a complete picture of how analysts influence earnings reporting.[5] Accordingly, the purpose of this paper is to provide the first observational empirical study into how securities analysts affects both accrual-based and real earnings management.

Recent evidence documents the importance of real activities manipulation as a way for managers to meet analysts' expectations. In a survey of 401 U.S. financial executives, Graham et al. (2005) find that a majority of executives were willing to use real activities manipulation to meet an earnings target, despite cash flow implications that may be value-destroying

[3]The terms "real earnings management" and "real activities manipulation" have the same meaning and are used interchangeably throughout this paper.

[4]These recent papers build off prior work emphasizing earnings manipulation via operational adjustments. For example, Bens et al. (2002), Dechow and Sloan (1991), and Bushee (1998) emphasize cutting R&D expenses as a means of managing earnings. In addition, Bartov (1993) and Burgstahler and Dichev (1997) provide evidence on the management of real activities other than through R&D.

[5]Recent research finds that greater analyst coverage results in fewer discretionary accruals used in corporate financial reporting (Irani and Oesch, 2013; Lindsey and Mola, 2013; Yu, 2008), concluding that analysts constrain earnings management and serve as external monitors of managers (as in Jensen and Meckling, 1976). However, these studies do not consider real activities manipulation as an alternative earnings management tool at managers' disposal.

from a shareholder perspective.[6,7] Thus, if analyst following pressures managers to meet earnings targets then this may induce managers to utilize real activities manipulations to boost short-term reported earnings. On the other hand, if analysts monitor companies' R&D investment, cost structure, and operational decisions then they may prioritize deterring managers' use of real actions to manipulate short run earnings, especially given the potentially great long-term loss of shareholder value.

This survey evidence also finds that managers may prefer to manage earnings using real activities, since accrual-based earnings management may be more likely to attract scrutiny from regulators, auditors, securities analysts or other market participants. Along these lines, Cohen et al. (2008) argue that managers prefer real activities manipulation because it may be harder to detect than accrual-based methods and thus entails lower expected private costs. In support of this argument, recent research documents a shift in earnings management behavior among U.S. corporations towards real activities manipulation and away from accrual-based methods in the wake of the Sarbanes-Oxley Act, a stricter regulatory regime (see also Chen and Huang, 2013).[8] Thus, if analysts monitor managers alongside regulators and other

[6] "We find strong evidence that managers take real economic actions to maintain accounting appearances. In particular, 80% of survey participants report that they would decrease discretionary spending on R&D, advertising, and maintenance to meet an earnings target. More than half (55.3%) state that they would delay starting a new project to meet an earnings target, even if such a delay entailed a small sacrifice in value." (Graham et al., 2005, p.32).

[7] Real activities manipulation can reduce firm value because actions taken to increase short run earnings can have a detrimental impact on future cash flows. For example, the use of price discounts to boost sales and meet earnings benchmarks may lead customers to expect such discounts in the future, implying lower margins sales going forward (Roychowdhury, 2006). Kedia and Philippon (2009) show that firms incur significant costs associated with the suboptimal operating decisions (excessive hiring and investment) they make to meet financial reporting goals. In addition, Bushee (1998) presents evidence that such short-termism can lead managers to forgo potentially valuable long-term investment in innovative projects that are highly risky and slow in generating revenues (see also He and Tian, 2013). On the other hand, accrual manipulation only involves changes to the accounting methods that are used to represent the underlying economic activities of the firm. If such changes are within the limits of Generally Accepted Accounting Principles (GAAP), this should not have a negative impact on firm value (e.g., Cohen and Zarowin, 2010; Zang, 2012).

[8] Dechow et al. (1996) present further evidence consistent with real activities manipulation being more difficult to detect than accrual manipulation. They conduct a comprehensive investigation of enforcement actions undertaken by the SEC for alleged violations of GAAP. None of the allegations they describe indicate that the enforcement action commenced because of some real economic decision.

stakeholders, as previous research ascertains (e.g., Chen et al., 2013; Irani and Oesch, 2013; Yu, 2008), then it is imperative that real activities manipulation be incorporated when attempting to measure the effect of analyst following on earnings management.

Empirical identification of the firm-level impact of analyst following on the use of real or accrual-based earnings management tools is severely hampered by endogeneity. Should a regression uncover a relationship between coverage and a measure of earnings management, it is difficult to rule out reverse causality, as corporate prospects and policies—including transparency (as in Healy et al., 1999; Lang and Lundholm, 1993)—inevitably drive decisions to initiate and terminate coverage. A further identification problem arises if some omitted factor attracts coverage and also influences earnings management (such as a seasoned equity offering, as in Cohen and Zarowin, 2010).

To address this serious endogeneity issue, we implement a quasi-experimental research design and examine the adjustment in managers' behavior to a plausibly exogenous decrease in analyst following caused by brokerage house mergers [originally proposed by Hong and Kacperczyk (2010)].[9] Following a brokerage house merger, the newly formed entity often will have several redundant analysts (due to overlapping coverage universes) and, as a result, one or more analysts might be let go (Wu and Zang, 2009). For instance, both merging houses might have an airline stock analyst covering the same set of companies. After the merger, in the newly-formed entity, it is likely that one of these stock analysts will be surplus to requirements. Thus, a loss of analyst coverage for the firms being covered by both houses arises due to these merger-related factors and not due to the prospects of these firms.

Our empirical approach makes use of 13 brokerage house merger events staggered over time from 1994 until 2005 and accommodates all publicly traded U.S. firms. Associated

[9] This quasi-experiment has been validated extensively in the literature in the process of studying security analyst coverage and analyst reporting bias (Fong et al., 2013; Hong and Kacperczyk, 2010), firm valuation and the cost of capital (Derrien et al., 2012; Kelly and Ljungqvist, 2007, 2012), real firm performance and corporate policies (Derrien and Kecskes, 2013), innovation (He and Tian, 2013), corporate governance (Chen et al., 2013; Irani and Oesch, 2013), and stock liquidity (Balakrishnan et al., 2013).

with these mergers are 1,266 unique firms that were covered in the year prior to the merger by both houses. These firms form our treatment sample. Using a difference-in-differences approach, we compare the adjustment in earnings management behavior of the treatment sample relative to a control group of observationally similar firms that were unaffected by the merger. Thus, we identify the causal change in earnings management strategies resulting from the loss of coverage.

We provide causal evidence that securities analysts influence earnings management. Using both discretionary accrual-based (Dechow et al., 1995; Jones, 1991) and real activities manipulation-based (Roychowdhury, 2006; Zang, 2012) measures of earnings management, we document two adjustments in behavior following an exogenous loss of analyst coverage. First, our estimates imply that a reduction in analyst coverage leads managers to use less real activities manipulation in their financial reporting. We find that the adjustment in real activities manipulation is coming primarily from a reduction in abnormal discretionary expenses, which includes R&D expenses. This suggests that analyst following pressures managers to meet outside expectations through real activities manipulation, for instance, by disincentivizing innovative activity.[10] Second, we find that the loss of coverage results in greater accrual manipulation. Taken together with the first result, this is consistent with managers preferring to use real activities manipulation in response to analyst pressure, perhaps because it is harder to detect and hence entails lower expected private costs to managers.

On further examination of the cross-section, we find that the treatment effect is nonlinear and more pronounced for treated firms with less analyst coverage prior to the merger, providing direct evidence that earnings management responds to large percentage drops in analyst coverage. We also observe a stronger treatment effect among "suspect" firms—those firms in close proximity to the zero earnings threshold (e.g., Degeorge et al., 1999)—and

[10]This finding fits into a broader literature that examines how earnings management through real activities impacts research and development (e.g., Baber et al., 1991; Bushee, 1998; Dechow and Sloan, 1991).

firms lacking experienced analysts. These three findings bolster our confidence in the plausibility of our quasi-experimental results, as we would expect the loss of coverage to matter most for these firms. In addition, following the coverage drop, we observe a stronger shift from real activities towards accrual-based earnings manipulation among treated firms with greater accounting flexibility or shorter auditor tenure; that is, those firms with lower costs of accrual manipulation. This suggests an important interaction effect between analyst following and other costs of accrual manipulation, which together impact managers' preferred mix of earnings management tools.

We conduct a battery of tests to check the validity and robustness of our results. We mitigate the concern that our findings could be driven by systematic differences in industries, mergers, or firms by showing that our estimates are robust to the inclusion of the respective fixed effects. Additionally, we demonstrate that our estimates are not merely capturing ex ante differences in the observable characteristics of treated and control firms, by including a number of control variables in our panel regression framework and also by implementing difference-in-differences matching estimators. Consistent results also emerge when we consider alternative measures of accrual-based and real earnings management, including several non-regression-based measures of accruals. We also examine the validity of our quasi-experiment—particularly, the parallel trends assumption—by constructing placebo mergers that shift the merger date one year backward or forward.

We wrap up our empirical analysis by running a series of ordinary least squares (OLS) regressions of real and accrual-based earnings management on analyst coverage, without taking into account the endogeneity of coverage. These estimates imply that analyst following is largely uncorrelated with earnings management behavior.[11] This is in contrast to the robust directional effects we uncover using our identification strategy. Moreover, these OLS

[11] In a similar OLS framework, Roychowdhury (2006) finds weak evidence on the use of real activities manipulation to meet annual analyst forecasts.

results are tricky to interpret because analyst coverage is likely to be endogenous. These mixed findings underscore the importance of our quasi-experimental research design.

This paper makes two main contributions to the literature. First, it advances the empirical literature on the interaction between analyst coverage and earnings management. Of note, Yu (2008) examines earnings management and analyst following and finds evidence of a negative relationship, consistent with an external monitoring role of analysts. We develop this line of thought in two ways. First, we employ a quasi-experimental design, allowing us to establish a direct causal relationship and demonstrate that a reduction in analyst coverage causes an adjustment in earnings management.[12] Second, we consider firms' overall earnings management strategy (i.e., abnormal discretionary accruals, cash flows from operations, production costs, and discretionary expenses) rather than accrual manipulation in isolation (see also Irani and Oesch, 2013). As a consequence, and in contrast to studies that base inferences solely on accrual-based methods, we find that analysts may pressure managers to meet expectations via real activities manipulation, particularly through the reduction of discretionary expenses. Thus, our new evidence offers a more complete picture on how analysts influence earnings management, in a well-identified empirical setting.

Our second contribution is to the earnings management literature. In light of the Graham et al. (2005) survey findings that managers prefer real activities manipulation, several notable studies have emerged examining this form of earnings management and whether there is any complementary or substitute interaction with accrual-based practices.[13] Zang (2012)

[12]This evidence is based on correlations between the level of analyst coverage and discretionary accruals, as well as an instrumental variables strategy that uses S&P 500 index inclusion as an instrument for coverage. Unfortunately, this instrument is unlikely to satisfy the exclusion restriction, as index inclusion is likely to reflect news about fundamentals that both attracts coverage and affects the decision to manage earnings.

[13]It is a priori unclear that real and accrual-based earnings management methods are substitutes. For instance, in a theoretical model, Kedia and Philippon (2009) show that accrual manipulating firms need to hire and invest sub-optimally—excessively, in fact—to mimic highly productive firms, fool investors, and avoid detection. In a model of real and financial inter-temporal smoothing, Acharya and Lambrecht (2014) show that managers may choose to lower outsiders' expectations by underreporting earnings and underinvesting. In these asymmetric information frameworks, under certain conditions, the two earnings management tools are complements.

assesses the tradeoffs between accrual manipulation and real earnings management and, by focusing on the timing and costs of each strategy, concludes that managers treat the two strategies as substitutes. Consistent with the idea that regulatory scrutiny affects the costs of accrual-based strategies, recent studies on the impact of the Sarbannes-Oxley Act (SOX) on the use of accrual-based and real earnings management provide evidence that managers substitute towards real activities manipulation in the post-SOX era (Chen and Huang, 2013; Cohen et al., 2008). Our contribution is to analyze how securities analysts influence managers' preferred mix of accrual and real activities manipulation. In our context, we find corroborative evidence that these two earnings management techniques are substitutes.

The remainder of this paper is structured as follows. Section 2 describes the data and empirical design. Section 3 reports the results of the empirical analysis. Section 4 concludes.

2. Empirical strategy and data

2.1. Identification

In this section, we lay out the details of our identification strategy and difference-in-differences estimator.

The most straightforward way to examine the issue of how monitoring by securities analysts affects earnings management is to regress a measure of corporate financial reporting on analyst following. However, the estimates from such regressions are difficult to interpret as a consequence of endogeneity (omitted variables bias, reverse causality, etc.).[14] For example, if a positive relation between analyst following and the use of accruals were uncovered, this may reflect the fact that analysts are attracted to firms with higher quality financial reporting

[14]Given the inherent identification problem, empirical research on this relationship has produced ambiguous results so far. Lang and Lundholm (1993) and Healy et al. (1999), for instance, conclude that companies with high disclosure quality (less earnings management) are followed by more analysts. Of note, Anantharaman and Zhang (2012) find that firms increase the volume of public financial guidance in reaction to a loss of analyst coverage.

(as in Healy et al., 1999), as opposed to (the reverse) causal impact of analyst coverage on reporting.

To address this endogeneity concern and identify a casual effect, we use brokerage house mergers as a source of exogenous variation in analyst coverage. In order for our quasi-experiment to be relevant, we require that the two merging brokerage houses—both covering the same stock prior to the merger—are expected to let one of these analysts go, leading to a loss of analyst coverage for a given firm. Most importantly, the coverage termination is unlikely to be a choice made by the analyst and, thus, independent of firm prospects and other factors that have the potential to confound inference.

We follow Hong and Kacperczyk (2010) to select the set of relevant mergers. We begin by gathering mergers in the Securities Data Company (SDC) Mergers and Acquisitions database involving financial institutions [firms with Standard Industrial Classification (SIC) code 6211, "Investment Commodity Firms, Dealers, and Exchanges"]. We keep mergers where there are earnings estimates in Thomson Reuters Institutional Brokers' Estimate System (I/B/E/S) for both the bidder and target brokerage houses. We retain merging houses that have overlapping coverage universes, that is, each house covers at least one identical company. This ensures the relevance of our empirical approach. Finally, we consider post-1988 mergers to make the calculation of our measures of earnings management feasible. These constraints yield 13 mergers, which are utilized in this paper.

To isolate the effects of each of these mergers on analyst career outcomes as well as stock coverage, we proceed as follows. First, we identify the I/B/E/S identifiers of the merging brokerage houses and the newly formed (merged) entity.[15] With these identifiers, we obtain the unique analyst identifiers for all analysts of the merging houses that provide an earnings forecast (in the year prior to the merger date) and all analysts that provide a forecast at

[15] We show these identifiers in Table 1, and they can also be found in the Appendix in Hong and Kacperczyk (2010).

the newly formed entity (in the year post-merger). The intersection of these two sets is a collection of analysts that were retained by the merged entity. Next, we obtain the lists of stocks covered by these analysts—one list for the bidder analysts and one for the target analysts—by compiling a list of unique stocks (identified by PERMNO) for which an earnings forecast was provided in the year prior to the merger date. The intersection of these two lists is the set of stocks covered by both houses pre-merger. There is overlapping coverage at the merging houses for this set of stocks. These are the ("treated") stocks that are the central focus of this paper.

Table 1 displays the key information on the 13 mergers. We indicate the names and I/B/E/S identification numbers of the merging brokerage houses, showing the bidding house in the top row of each partition. We provide a description of analyst employment at both houses both before and after the merger. We also detail stock coverage at each house, in particular, a count of the unique U.S. stocks followed by each house in the year before the merger, as well as the coverage overlap.

To illustrate our identification strategy, consider the Morgan Stanley and Dean Witter Reynolds merger, which took place on May 31, 1997. There was significant analyst turnover as a consequence of the merger. More precisely, Morgan Stanley had 89 analysts prior to the merger and Dean Witter Reynolds had 39. After the merger, the combined entity had a total of 84, retaining 78 analysts from Morgan Stanley and only six from Dean Witter Reynolds. We also see that there were 180 (treated) stocks that were covered by both firms prior to the merger. However, as evidenced by the final column of Table 1, following the merger the new entity had fewer analysts with coverage overlap. In particular, the six Dean Witter Reynolds analysts retained in-house only continued to cover 11 of the 180 treated stocks.

We replicate this procedure for each of the remaining 12 mergers and identify a total of 1,266 unique treated stocks. A similar pattern emerges for the full set of mergers, as in the case of Morgan Stanley's merger with Dean Witter Reynolds: On average, stocks with overlapping coverage tend to lose coverage following the merger and coverage tends to be kept by analysts at the acquiring house.[16] We verify this explicitly in Section 3 and use this variation to estimate a causal impact of analyst coverage on accrual-based and real earnings management.

In order to implement our identification strategy, we must select an event window around the merger to be able to isolate potential effects brought about by the merger. In contrast to short-term event studies that use daily stock market data, we use annual accounting data and require a longer event window. To this end, we follow other studies also using brokerage house mergers and financial statement data (e.g., Derrien and Kecskes, 2013; Irani and Oesch, 2013) and use a two-year window consisting of one year (365 days) prior to the merger and one year following the merger. To calculate the number of analysts covering a stock around the merger date, we use the same window. To calculate accounting ratios, we use financial statement data from the last fiscal year that ended before the merger as the pre-merger year and the first complete fiscal year following the merger as the post-merger year. For example, consider a treated firm with a December fiscal year-end and a November 28, 1997 merger date. In such a case, the pre-merger year ($t-1$) is set to the year ending on December 31, 1996 and the post-merger year ($t+1$) is set to the year ending on December 31, 1998. This yields two non-overlapping observations for all the firms included in our sample, one pre- and one post-merger.

The simplest way to test for differences in firms' earnings management behavior following a reduction in analyst coverage is to contrast the corporate financial reporting of treated

[16] Wu and Zang (2009) confirm that analyst turnover is concentrated at the target brokerage house and tends to reflect the acquirer's elimination of duplicate research coverage. This indicates that coverage terminations follow a clear pattern that is unrelated to analyst skill or firm performance. Hong and Kacperczyk (2010) examine a similar set of treated stocks and find that the mergers led to a decline in analyst performance, measured by annual earnings per share forecast updates and accuracy (forecast error variance). These findings alleviate the concern that the loss of coverage for treated stocks coincides with an improvement in the quality of analyst coverage.

firms before the merger shock to the reporting of treated companies after the merger. This approach disregards, however, potential trends that impact all stocks (regardless if they are included in the treatment sample or not). For example, new accounting regulations might limit the use of accrual-based accounting manipulation for all firms in a way that coincides with the pre- or post-period of a particular merger (e.g., the Sarbanes-Oxley Act in 2002 as in Chen and Huang, 2013; Cohen et al., 2008). By only considering the time-series (i.e., post minus pre) difference for treated firms, this could lead us to falsely attribute an adjustment in treated firms' reporting behavior to the merger. We adopt a commonly used method to address potential time trends: incorporating a control group and using a difference-in-differences (DiD) methodology. This method compares the difference in the variable of interest across the event window between the treated and control firms. In our setting, the set of control firms are all stocks that do not have overlapping coverage at the merging brokerage houses.

One residual concern with our identification strategy is that ex ante differences between treatment and control samples could affect the estimated impact of the coverage loss. In our context, this could be due to the fact that larger firms tend to be covered by more brokerage houses (and are thus more likely to be a treated firm), but that these larger firms are also less likely to manipulate earnings. Thus, it is important to control for such differences in characteristics in our empirical specification to ensure we are correctly identifying the effect of the coverage shock. We mitigate this concern using two different approaches. First, as detailed below, we incorporate control variables into our linear regression framework. Second, as detailed in Section 3.3, we implement a difference-in-differences propensity score matching estimator.

To empirically test how firms react to the exogenous coverage loss, we implement our quasi-experiment using the following panel regression specification

$$EM_i = \alpha + \beta_1 POST_i + \beta_2 TREATED_i + \beta_3 POST_i \times TREATED_i + \gamma' X_i + \epsilon_i, \qquad (1)$$

where EM_i denotes our measure of earnings management (i.e., accrual-based or real) for firm i, $POST_i$ denotes an indicator variable that is equal to one in the post-merger period and zero otherwise, and $TREATED_i$ is an indicator variable that identifies whether a firm is treated or not. The coefficient of interest is β_3, which corresponds to the DiD effect, namely, the impact of the merger on the earnings management behavior of treated firms relative to control firms.

We employ several versions of (1). Our preferred specification includes industry, merger, and firm fixed effects that account for time-invariant (potentially unobservable) factors particular to a merger, an industry, or a firm that may influence the earnings management behavior between units. This specification permits the inclusion of firm-specific control variables (to be defined below), which we incorporate as part of the vector X_i on the right-hand side of (1). This specification is estimated using heteroskedasticity-robust standard errors, which we cluster at the firm-level.[17]

2.2. Sample construction

In this section we detail how we construct our sample. First, we collect data on analyst coverage from I/B/E/S. For the 13 mergers that comprise our identification strategy, we consider a 365-day window around the brokerage house merger calendar date and keep all publicly traded U.S. companies that have an earnings forecast in this window. This yields 144,943 firm-year observations.

[17] We have experimented with various different clusterings (e.g., by merger, industry, merger and industry). Our results are robust to these various clustering schemes. Clustering at the firm-level tends to produce the largest—and thus most conservative—standard errors, so we elect to report these throughout.

Next, we merge this sample with financial statement data from Standard & Poor's Compustat. To this end, we assign fiscal years to the 365-day windows before and after the merger date. We assign the last completed fiscal year before the merger date to the 365-day window before the merger date and the first complete fiscal year after the merger date to the 365-day window after the merger date. We link 110,482 firm-year observations.

Next, we require that each firm-year observation has the variables necessary to calculate our primary measures of earnings management (AM and RM, as defined below). This requirement results in a final sample of 61,442 firm-year observations, which consists of 1,266 treated firms. This shrinkage in sample size results from missing accounting data or SIC-code, or a firm belonging to an industry-year with fewer than 15 observations.

In further specifications, we include control variables (defined below) which utilize both balance sheet and securities price data from the merged CRSP/Compustat database. Constructing these variables imposes data constraints that reduce the sample for these analyses to at most 60,758 firm-year observations.

2.3. Measuring earnings management

In our empirical analysis, our main dependent variables will be an accrual-based measure of earnings management (AM) and a measure of real activities manipulation (RM). We follow the extant earnings management literature when constructing these variables.

We construct AM in the following way. First, we estimate the "normal" level of accruals for a given firm, using coefficients obtained from an industry-level cross-sectional regression model of accruals.[18] To estimate the normal level of accruals, we use the Jones model (Jones, 1991) in its modified version (Dechow et al., 1995). To this end, we first run the following

[18]The advantage of such a cross-sectional approach is that it helps us deal with the severe data restrictions and survivorship bias that arise in time-series models. Moreover, given our focus on year-to-year changes around the merger dates, a time-series estimate would not be appropriate.

regression for each industry and year pair

$$\frac{TA_{it}}{A_{i,t-1}} = a_1 \frac{1}{A_{i,t-1}} + a_2 \frac{\Delta REV_{it}}{A_{i,t-1}} + a_3 \frac{PPE_{it}}{A_{i,t-1}} + \epsilon_{it}, \tag{2}$$

where TA_{it} denotes total accruals of firm i in year t, computed as the difference between net income (Compustat item *ni*) and cash flow from operations (item *oancf*), ΔREV is the difference in sales revenues (item *sale*), and PPE is gross property, plant, and equipment (item *ppegt*). These variables are all normalized by lagged total assets (item *at*).[19]

The estimated coefficients from (2) are then used to calculate normal accruals (NA) for each firm

$$\frac{NA_{it}}{A_{i,t-1}} = \widehat{a}_1 \frac{1}{A_{i,t-1}} + \widehat{a}_2 \frac{\Delta REV_{it} - \Delta AR_{it}}{A_{i,t-1}} + \widehat{a}_3 \frac{PPE_{it}}{A_{i,t-1}}, \tag{3}$$

where ΔAR is the change in receivables (item *rect*) and the other variables are the same as above. Finally, we calculate our measure of accruals management, AM, as the absolute difference between total accruals and the predicted firm-level normal accruals ("abnormal accruals"). Large absolute abnormal accruals reflect high differences between the cash flows and the earnings of a firm, relative to an industry-year benchmark. We attenuate the distortions arising from extreme outliers by winsorizing our AM variable at the 1% and 99% levels.[20]

In robustness tests, we consider a number of alternative measures of accrual-based earnings management. First, we use two non-regression-based measures of current accruals.

[19]In our baseline results, we use the 48 Fama-French industries. In Section 3.3, we show that our results are robust to using the two-digit SIC industry classification.

[20]A potential concern with this measure is that standard Jones-type models of discretionary accruals are not able to adequately control for firm growth. In robustness tests, we follow the procedure outlined in Collins et al. (2012) and adjust the discretionary accruals for sales growth. We find our results to be unaffected by this adjustment. The same is also true when we use performance-matched discretionary accruals, as advocated by Kothari et al. (2005).

Following Sloan (1996), we calculate the current accruals as

$$CA_{it} = \frac{\Delta CAT_{it} - \Delta CL_{it} - \Delta CASH_{it} - DEP_{it}}{A_{i,t-1}}, \tag{4}$$

where ΔCAT is the change in current assets (item *act*), ΔCL is the change in current liabilities (item *lct*), $\Delta CASH$ is the change in cash holdings (item *che*), and DEP is the depreciation and amortization expense (item *dp*). We exclude short-term debt from current liabilities, since managers will lack discretion over this item in the short run (Richardson et al., 2005). We take the absolute value of these current accruals as an alternative measure of accruals manipulation.

We also consider a variant of this accruals measure, "CA (exc. Depr)," calculated by removing depreciation from (4). We do so following Barton and Simko (2002), who argue that managers have limited discretion over depreciation schedules in the short run.

The third non-regression-based measure of accrual manipulation follows Hribar and Collins (2002) and is based on data from the income and cash flows statement, as opposed to the balance sheet. These authors show that using balance sheet information to calculate accruals relies on a well-defined mapping between the statement of cash flows and the balance sheet. However, non-operating events such as M&A activity or foreign currency transactions can lead to a breakdown in basic relationships among financial statements.[21] Specifically, changes in current assets and liabilities brought about by such events will show up on the balance sheet, but do not flow through the income statement as earrings are unaffected. As a consequence, Hribar and Collins (2002) show that using a balance sheet approach to estimate abnormal accruals can lead to the incorrect conclusion that earnings management

[21] This is commonly referred to as a "non-articulation" problem (e.g., Wilkins and Loudder, 2000).

exists when in fact it does not. A measure not subject to this problem can be computed as

$$CA\text{ (Cash Flow)}_{it} = \frac{EBXI_{it} - CFO_{it}}{A_{i,t-1}}, \tag{5}$$

where $EBXI$ denotes earnings before extraordinary items and discontinued operations (item *ibc*) and CFO is the operating cash flows from continuing operations taken from the statement of cash flows (item *oancf* − item *xidoc*). This measure is conceptually similar to a balance-sheet accruals measure in that it aims to capture the difference between earnings and cash flows. The key difference is that it is calculated using data from the income statement and the statement of cash flows, rather than the balance sheet.

Construction of a valid RM proxy uses the model introduced by Dechow et al. (1998), as implemented by Roychowdhury (2006) among others (e.g., Chen and Huang, 2013; Cohen et al., 2008; Zang, 2012). We follow these earlier works and consider the abnormal levels of cash flow from operations (CFO), discretionary expenses (DISX), and production costs (PROD) that arise from the following three manipulation methods. First, sales manipulation achieved by acceleration of the timing of sales via more favorable credit terms or steeper price discounts. Second, the reduction of discretionary expenditures, which include SG&A expenses, advertising, and R&D. Third, reporting a lower cost of goods sold (COGS) by increasing production.[22]

As a first step we generate the normal levels of CFO, DISX, and PROD. We express normal CFO as a linear function of sales and change in sales. We estimate this model with the following cross-sectional regression for each industry and year combination:

$$\frac{CFO_{it}}{A_{i,t-1}} = b_1 \frac{1}{A_{i,t-1}} + b_2 \frac{SALES_{it}}{A_{i,t-1}} + b_3 \frac{\Delta SALES_{it}}{A_{i,t-1}} + \epsilon_{it}. \tag{6}$$

[22]Roychowdhury (2006) provides a detailed description of the mechanics of these real activities manipulation methods.

Abnormal CFO (RM_{CFO}) is actual CFO minus the normal level of CFO calculated using the estimated coefficients from (6). CFO is cash flow from operations in period t (item $oancf$ minus item $xidoc$).

Production costs are defined as the sum of cost of goods sold (COGS) and change in inventory during the year. We model $COGS$ as a linear function of contemporaneous sales:

$$\frac{COGS_{it}}{A_{i,t-1}} = c_1 \frac{1}{A_{i,t-1}} + c_2 \frac{SALES_{it}}{A_{i,t-1}} + \epsilon_{it}. \tag{7}$$

Next, we model inventory growth as:

$$\frac{\Delta INV_{it}}{A_{i,t-1}} = d_1 \frac{1}{A_{i,t-1}} + d_2 \frac{\Delta SALES_{it}}{A_{i,t-1}} + d_3 \frac{\Delta SALES_{i,t-1}}{A_{i,t-1}} + \epsilon_{it}. \tag{8}$$

Using (7) and (8), we estimate the normal level of production costs as:

$$\frac{\Delta PROD_{it}}{A_{i,t-1}} = e_1 \frac{1}{A_{i,t-1}} + e_2 \frac{SALES_{it}}{A_{i,t-1}} + e_3 \frac{\Delta SALES_{it}}{A_{i,t-1}} + e_4 \frac{\Delta SALES_{i,t-1}}{A_{i,t-1}} + \epsilon_{it}. \tag{9}$$

$PROD$ represents the production costs in period t, defined as the sum of COGS (item $cogs$) and the change in inventories (item $invt$). The abnormal production costs (RM_{PROD}) are computed as the difference between the actual values and the normal levels predicted from equation (9).

We model discretionary expenses as a function of lagged sales and estimate the following model to derive normal levels of discretionary expenses

$$\frac{\Delta DISX_{it}}{A_{i,t-1}} = f_1 \frac{1}{A_{i,t-1}} + f_2 \frac{SALES_{i,t-1}}{A_{i,t-1}} + \epsilon_{it}, \tag{10}$$

where $DISX$ represents the discretionary expenditures in period t, defined as the sum of advertising expenses (item xad), R&D expenses (item xrd), and SG&A (item $xsga$). Abnormal discretionary expenses (RM_{DISX}) are computed as the difference between the actual values and the normal levels predicted from equation (10).

Finally, throughout our analysis we consider two aggregate measures of real earnings management activities that incorporate the information in RM_{CFO}, RM_{PROD}, and RM_{DISX}. These measures are computed following Zang (2012) and Cohen and Zarowin (2010) as

$$RM_1 = RM_{PROD} - RM_{DISX}, \tag{11}$$

$$RM_2 = -RM_{CFO} - RM_{DISX}. \tag{12}$$

Higher values of RM_1 and RM_2 imply that the firm is more likely to have used real activities manipulation.[23,24]

2.4. Control variables

To mitigate concerns regarding observable differences among treated and control firms we incorporate control control variables in empirical specification (1). In this section, we describe these control variables.

To select appropriate control variables, we follow prior research that also uses measures of accrual-based and real earnings management as dependent variables (e.g., Anantharaman and Zhang, 2012; Armstrong et al., 2012; Li, 2008; Yu, 2008; Zang, 2012). These variables include the logarithm of a firm's market capitalization ($LNSIZE$), where a firm's market capitalization is calculated as the number of common shares outstanding times price. We include a company's return on assets (ROA) as a measure of profitability, computed by

[23] RM_{PROD} is not multiplied by minus one, as higher production costs suggest excess production and lower COGS. Moreover, as discussed in Cohen and Zarowin (2010) and Roychowdhury (2006), we do not combine abnormal cash flow from operations and abnormal production costs, as it is likely that the same activities will give rise to abnormally low CFO and high PROD, and a double counting problem as a consequence.

[24] We have also experimented with performance-matched measures of real earnings management, in the spirit of Kothari et al. (2005) and Cohen et al. (2013). We found our results to be robust to these alternative measures.

dividing a company's net income by its total assets. We include the natural logarithm of a company's book value divided by its market capitalization (MTB). We include a company's sales growth ($SALESGR$) computed as the yearly growth in sales. All of these variables are based on information obtained from Compustat. Finally, from I/B/E/S, we include the number of unique analysts covering a particular firm in a given fiscal year ($COVERAGE$). All continuous non-logarithmized variables are winsorized at the 1% and 99% levels.

The data constraints imposed by these additional variables reduce the sample from 61,442 to 60,758 firm-year observations. Summary statistics for these variables for both treatment and control samples are shown in Table 2. Panel A of Table 2 presents the summary statistics for the earnings management variables. Panels B and C summarize the control and other variables used in cross-sectional analyses, respectively.

Treated firms are larger in size and have greater coverage than the average Compustat firm. These differences occur for two reasons. First, treated firms must be covered by at least two brokerage houses. Second, the majority of treated firms are involved with the large brokerage house mergers (i.e., mergers 1, 2, 3, 9, and 10, as detailed in Table 1) and large houses tend to cover large firms (Hong and Kacperczyk, 2010). In addition, the treatment and control samples differ along several other observable dimensions, as displayed in Table 2. To validate our empirical design, we will demonstrate in tests below that our results are not driven by these ex ante differences.

3. Results

This section starts by confirming the validity of the quasi-experiment and then quantifies the average effect of an exogenous loss of analyst coverage on earnings management (Section 3.1). In Section 3.2, we conduct a series of cross-sectional tests to further assess what is driving the estimated average treatment effect. In particular, we investigate how this treatment effect varies with proximity to important earnings thresholds, analyst experience, and the costs of earnings management. In Section 3.3, we conclude our empirical analysis with a series of robustness tests.

3.1. Average effect of analyst following on earnings management

Table 3 presents the main results and contribution of this paper. We first validate the key premise of the experiment: on average, treated firms should lose analyst coverage relative to non-treated firms in the year following a brokerage house merger. We examine whether this is the case by replacing earnings management (EM) with analyst coverage ($COVERAGE$) on the left-hand side in equation (1). The first column of Table 3 confirms that our quasi-experiment is relevant. The estimated coefficient is -0.633 with a t-value of -4.44. This is consistent in terms of size and significance with research using a similar experimental design (e.g., Derrien and Kecskes, 2013; Hong and Kacperczyk, 2010), in spite of sample differences occurring due to various data restrictions across these studies.

Next, we investigate the effects of this loss of coverage on the earnings management behavior of the firm. The remaining columns of Table 3 display these results. Column 2 shows the outcome of estimating (1) with accrual manipulation (AM) as the dependent variable without any fixed effects. The results indicate that the DiD coefficient, β_3, is positive and statistically significant. The point estimate on the DiD term in Column 2 is 0.043, indicating that a drop in coverage among treated firms causes an increase in the use of abnormal discretionary accruals that is about 9% of one standard deviation. Thus, the effect we document is both statistically significant and economically meaningful.

In Columns 3 to 5, we run the same analysis but now include a battery of fixed effects. These fixed effects mitigate the concern that time-invariant factors could be driving the observed relationship between coverage and earnings management behavior between units. In Column 3, we include merger fixed effects. We then additionally include industry and, finally, industry and firm fixed effects. None of these steps change the overall picture: For all of these specifications, the estimated partial effect of the merger on the treated firms remains statistically significant and on the same order of magnitude. This confirms that the estimated impact of coverage on accrual manipulation is not due to time-invariant heterogeneity between mergers, industries, or firms.

Thus, after the merger and coverage loss, consistent with greater accrual manipulation, treated firms' accounting figures reflect a higher amount of absolute abnormal accruals, i.e., a larger gap between cash flows and earnings relative to industry peers. This outcome mirrors prior empirical research that infers a monitoring role of securities analysts when studying their impact accrual manipulation (Irani and Oesch, 2013; Lindsey and Mola, 2013; Yu, 2008).

In columns 6 and 7, we examine the impact of coverage on real earnings management. We consider the two composite measures of real activities manipulation used in Cohen and Zarowin (2010) and defined in (11) and (12). The estimated DiD coefficient in the RM_1 equation is -0.095 with a t-value of -3.46. We arrive at this estimate when we include the full set of merger, industry, and firm fixed effects. A similar result holds when we exclude these fixed effects (omitted for brevity) and also in the RM_2 equation, although the magnitude is slightly smaller in the latter case. Thus, the point estimate indicates that a loss of coverage causes a reduction in the use of real earnings management among treated firms. This reduction in real activities manipulation is both relative to control firms and relative to the level of real manipulation within-firm in the period prior to the coverage shock.

These estimates are the key findings of this paper. They indicate that managers decrease the use of real activities to manipulate reported earnings in response to an exogenous loss of coverage. This positive relationship is consistent with analyst following pressuring managers to manage earnings and doing so via real activities manipulation. The use of real activities to manipulate reported earnings can be rationalized by observing that it may be harder to detect and punish such actions and may therefore be characterized by lower expected private costs for managers (Cohen et al., 2008; Graham et al., 2005).

Consistent with prior literature (e.g., Yu, 2008), we find a negative relationship between analyst following and accrual-based earnings management. While this relationship is in line with analysts constraining accrual-based earnings management, by considering managers' overall earnings management strategy our results indicate that managers use real activities manipulation as a natural alternative way to handle pressure from analysts. Indeed, our findings indicate that a reduction in analyst following leads to a shift in managers' preferred mix of earnings management tools, in particular, a substitution from real activities manipulation towards accrual-based earnings management. Thus, considering both methods of earnings management is informative and enables us to uncover a more complete picture of how securities analysts influence earnings management practices.

Next, we examine how the adjustment in earnings management varies with initial analyst coverage. We reasonably expect firms experiencing a large percentage reduction in coverage to adjust their earnings management behavior more sharply. Moreover, if securities analysts do affect earnings management then we would also expect to observe the greatest adjustment in reporting behavior among firms experiencing a large percentage loss in analyst coverage (i.e., those firms with a low pre-merger level of coverage). This is an important way to test the validity of our identification strategy.

The results of this investigation are shown in Panel B of Table 3. We split our treatment sample into two groups and define an indicator variable that is equal to one depending on whether coverage in the year prior to the merger is above ($LOWCOVERAGE = 0$) or below ($LOWCOVERAGE = 1$) the median among treated firms. Mean coverage in the below(above)-median pre-merger coverage subgroup is 12.1 (28.3). We then estimate our baseline model allowing the treated effect to differ among these two groups. The point estimates indicate that the cross-sectional effect is concentrated among firms with low pre-merger coverage, which are firms where the loss of one analyst represents a larger percentage drop in analyst following. For this group, the estimated DiD coefficient for the accrual manipulation regression is positive and statistically significant, and negative and significant for the real activities manipulation regressions. This is not the case for the high coverage subgroup. We conduct two additional sample splits (unreported) that are based on the pre- and post-merger level of coverage, respectively. First, following Hong and Kacperczyk (2010), we compare treated firms that were covered by fewer than five analysts pre-merger with those that were covered by more than five. Using this alternative cutoff, we find a consistent result that treated firms with low pre-merger coverage experience a stronger treatment effect. Second, we condition on firms losing all coverage post-merger and once again find that treatment effect is stronger for this subset of firms. Thus, the effect of coverage on earnings management is strongest among firms experiencing a large percentage drop in coverage, which is consistent with our expectation and also reassures us that our experiment is well-designed.

In our next set of tests, we disaggregate our composite real activities manipulation measure and repeat our baseline tests on each separate component (RM_{PROD}, RM_{CFO}, and RM_{DISX}). Our aim is to understand which of the three methods of real manipulation described in Section 2.3 features most prominently.

These results can be found in Table 4. We reestimate (1) using each of the three real activities manipulation components as left-hand side variables.[25] Panel A displays the results for RM_{DISX}, Panel B for RM_{CFO}, and Panel C for RM_{PROD}. In Column 1 to 4 of each panel, we repeat the analysis starting with no fixed effects and then incorporating merger, industry, and firm fixed effects sequentially. This demonstrates the robustness of the point estimates to these potential sources of heterogeneity.

[25]The left-hand side variables in the regressions are $-RM_{DISX}$, $-RM_{CFO}$, and RM_{PROD}, respectively, for ease of interpretation.

Looking across these panels and focusing on the $POST \times TREATED$ interaction, the point estimates indicate that the adjustment in real activities manipulation following the coverage drop is coming primarily from abnormal discretionary expenses. The increase in abnormal discretionary expenses following the reduction in coverage is consistent with recent empirical evidence in He and Tian (2013), who argue that analysts impede innovative activity.

Overall, the key results presented here indicate that an exogenous reduction in analyst coverage causes greater use of accrual-based earnings management and less real activities manipulation, a substitution effect. These results indicate that managers substitute out of real activities manipulation and into accrual manipulation when they feel less scrutinized by financial analysts. This finding is consistent with managers rebalancing their mix of earnings management tools to reflect the lower likelihood of detection of accrual manipulation.

3.2. Cross-sectional analysis

Our findings so far indicate that managers trade-off the costs and benefits of earnings management tools (real- and accrual-based) and that financial analysts have a causal impact on this trade-off. To bolster confidence in the plausibility of our results and the validity of our quasi-experimental research design, we now investigate how the treatment effect varies cross-sectionally. In particular, we seek to understand whether the treatment effect is stronger among firms with a greater incentive to manage earnings. We consider firms in close proximity to the zero-earnings threshold (Section 3.2.1.), firms losing experienced analysts (Section 3.2.2.), and firms with lower costs of earnings management (Section 3.3.3.). These factors have been identified by the accounting and finance literature as key determinants of incentives to manage earnings.[26] As we will discuss in detail, the results from this section provide further support for a causal effect of analyst scrutiny on managers' mix of accrual-

[26]For overviews of the literature on earnings management incentives, see Dechow and Skinner (2000), Fields et al. (2001), and Healy and Palepu (2001).

and real-based earnings management.

3.2.1. Suspect firms at the zero earnings threshold

In this section, we examine the earnings management behavior of firms in close proximity to an important earnings benchmark. We demonstrate that the estimated treatment effect is stronger for this subset of "suspect" firms.

Conceptually, if analysts pressure managers to meet short run earnings benchmarks then this may incentivize managers to manipulate earnings. If this statement is true, it follows that managers should have a stronger incentive to manage earnings when earnings are expected to narrowly miss a short run earnings target. Put differently, the effect of analyst scrutiny on earnings management and managers' mix of accrual and real activities manipulation should be more evident when a firm is in close proximity to an earnings target.

This line of thought is corroborated by previous empirical research. First, empirical evidence clearly demonstrates that firms with reported earnings either meeting or closely beating important targets manage earnings more frequently. This occurs both through accrual-based earnings management (Bartov et al., 2002) and real activities manipulation (Bushee, 1998). Second, analyst coverage may exacerbate incentives to manipulate earnings near targets, since greater coverage is associated with a more severe market reaction to a firm missing an earnings target (Gleason and Lee, 2003; Hong et al., 2000).

In this section, we focus on the zero-earnings threshold. This benchmark has been shown to be particularly important in both research based on observational data and survey-based evidence. For instance, using a large sample of U.S. firms from 1984 until 1996, Degeorge et al. (1999) find that the positive earnings threshold is predominant in the sense that firms falling just short of this particular benchmark show a strong tendency to manage earnings upward (see also Burgstahler and Dichev, 1997). Moreover, in their survey of 401 U.S. financial executives, Graham et al. (2005) find that more than 65% agree or strongly agree that reporting a positive earnings per share is an important benchmark.

To identify firms near the zero-earnings threshold and conduct our empirical analysis we proceed as follows. We label earnings management suspects as firm-years with earnings meeting or closely beating the zero-earnings threshold in the previous year. In particular, we classify treated firm-years as meeting or closely beating this threshold whenever reported earnings before extraordinary items divided by lagged assets fall between 0 and 0.05% (Roychowdhury, 2006; Zang, 2012). We then define an indicator variable $SUSPECT$ that reflects this partitioning of the treatment sample. Finally, we repeat the heterogenous treatment effects analysis, that is, we re-estimate our baseline model allowing the treatment effect to differ between the suspect and non-suspect groups.

The results of this analysis are displayed in Table 5. The baseline treatment sample is partitioned into two groups depending on whether pre-merger earnings are in close proximity to zero threshold ($SUSPECT = 1$) or not ($SUSPECT = 0$). We then estimate the differential treatment effect between these two groups by interacting this indicator variable with the $POST \times TREATED$ variable. In columns [1] to [3], we see that each of the point estimates of the treatment effect is larger in magnitude for the suspect firms relative to the non-suspect firms. Moreover, for the suspect group, the estimated DiD coefficient in the accrual manipulation regression is positive and larger than the average treatment effect reported in Table 3. For the non-suspect group, the estimated treatment effect is not statistically significant. A similar pattern emerges for the estimates from the real earnings management equations, where we find the estimated treatment effect to be larger for the suspect treated firms as compared to the non-suspect firms.[27]

[27]We implement an F-test of the alternative hypothesis that the coefficient on the suspect firm triple-interaction is bigger than the non-suspect interaction (against the null hypothesis that they are equal). We implement three such tests, one for each of the earnings management regressions in columns [1], [2], and [3]. The respective p-values for these tests are 0.062, 0.037, and 0.127, indicating that the differences in coefficients are statistically significant (in columns [1] and [2]) or borderline statistically insignificant (in column [3]).

These results are consistent with our expectation that treated firms close to the important zero-earnings benchmark have a greater incentive to manage earnings. For these firms the exogenous loss of coverage induces a greater response, consistent with analyst coverage having a compounding effect on earnings management incentives.

3.2.2. Impact of analyst experience

In this section, we test cross-sectionally how the experience of the analysts following the firm influences earnings management behavior following the merger-related coverage loss.

An established literature has found that analyst experience (or skill) matters for capital markets outcomes and also has an important impact on earnings management outcomes. This research demonstrates that analysts with greater experience are more skillful in the sense that they provide more accurate forecasts that incorporate past information more quickly (Clement, 1999; Mikhail et al., 1997, 2003). These studies put forward two main explanations for the observed positive relationship between analyst experience and skill. First, senior analysts have survived longer than junior analysts, and it is therefore plausible that they are endowed with more skill to begin with. Second, senior analysts may acquire more skill with time, for example, as a result of industry specialization, training, or repeated interactions with management at a given company. On the relationship between analyst skill and managerial behavior, Yu (2008) finds that accrual manipulation is negatively correlated with the average level of experience of the analysts following the firm, conditional on the level of coverage. This final piece of evidence is consistent with the scrutiny of experienced analysts incrementally constraining accrual-based earnings management.

In our context, we hypothesize that treated firms followed by highly experienced analysts will have less of an incentive to adjust earnings management behavior following a reduction in coverage. Empirically, this will correspond to a small average treatment effect of the merger-related coverage loss on earnings management for treated firms followed by highly experienced analysts. Our basic intuition is that, from the perspective of a firm covered by highly experienced analysts, losing one analyst at random is likely to only have a small impact on managerial incentives. For these firms, the remaining pool of analysts covering the firm is skillful. Thus, the nature of remaining analysts' forecasts, the market reaction to earnings announcements, and hence managers' incentives will likely be unaffected by the coverage loss.

We investigate this hypothesis empirically by first suitably defining analyst experience at the firm level and then implementing a test in our heterogenous treatment effects framework.

We measure analysts' experience at the firm-level in the year prior to the merger. To this end, we first measure the level of experience of each individual analyst covering every firm in our treatment sample. We measure analyst-level experience in two complementary ways following Yu (2008). First, we consider "general experience," which is simply the number of years an analyst has worked as an analyst. We measure this as the number of years that an analyst identifier appears in the I/B/E/S database.

Second, we consider "house experience," which is the number of years that an analyst has worked at her current brokerage house. This is calculated by counting the number of years that an analyst and her current employer's identifier are matched in I/B/E/S. Firm-year level analyst experience is then calculated as the simple average of the experience of analysts covering the firm in a given year. Table 2 provides relevant summary statistics for firm-level analyst experience.[28]

Next, we define an indicator variable, EXP, and utilize our heterogenous treatment effects empirical framework. Our treatment sample is once again classified into two groups, this time depending on whether the pre-merger level of analyst experience is above ($EXP = 1$) or below ($EXP = 0$) the median level of firm-level experience in the sample. Our

[28]Note that for this analysis we lose a small number of control firms due to the fact that a unique analyst identifier is not reported in I/B/E/S.

baseline model (1) is then re-estimated allowing for a differential treatment effect across the two groups.[29]

The results of this analysis are shown in Table 6. Looking across columns [1] to [3] and comparing the point estimates between the two groups, we see that the coefficient on $POST \times TREATED$ is larger in magnitude for the low analyst experience group as compared to the high analyst experience group. This is true for each of the accrual manipulation and real activities manipulation equations. Moreover, this is also the case when analyst experience is measured using house experience (see columns [4] to [6]). However, while each of the point estimates are statistically significant at conventional levels, the differences are not in general.[30] Thus, we must interpret these results with caution given the weak evidence that that the coefficients are statistically distinct.

The results of these tests provide suggestive evidence that firms covered by highly experienced analysts do not adjust their earnings management behavior to the same extent as those followed by inexperienced analysts. This finding confirms our intuition that the random loss of an analyst from a highly experienced group covering a firm will not affect managerial incentives to engage in earnings management.

[29]Throughout Section 3.2, we choose to employ dichotomous variables and interact them with the $POST \times TREATED$ variable in our heterogenous treatment effects regression specification. While the analyst experience variables are continuous, we cannot cleanly incorporate them into a triple-differences specification. In the case of the analyst general and house experience variables, both variables are always ≥ 1 for treated firms (the pre-merger mean general experience is equal to 7.8 and minimum is equal to 1.5). This follows from treatment assignment requiring that a firm be covered by at least two analysts prior to the merger. This leads to a tricky interpretation of the triple-interaction coefficient in such a regression, as there is not a clear baseline level of experience. On the other hand, using dichotomous variables allows for a consistent interpretation of our estimated coefficients and also permits hypothesis testing.

[30]We implement an F-test of the alternative hypothesis that the coefficient on the inexperienced triple-interaction is bigger than the experienced interaction (against the null hypothesis that they are equal). We implement three tests, one for each of the earnings management regressions in columns [1] through [3] in Table 6. The respective p-values for these tests are 0.22, 0.23, and 0.09. We conduct equivalent F-tests for the "house experience" regressions, resulting in p-values of 0.10, 0.42, and 0.10 for columns [4], [5], and [6], respectively.

3.2.3. Impact of the costs of earnings management

Differences in firms' accounting and operational environments give rise to differences in the relative costs of real and accrual-based earnings management methods.[31] In this section, we investigate two important costs of accrual manipulation: auditor quality and accounting flexibility. We hypothesize and find that when these costs are relatively high, firms are unable to substitute away from real activities toward accrual manipulation following the loss of analyst coverage. Thus, we provide causal evidence that the impact of analyst scrutiny on accrual-based earnings management matters less in the presence of an effective auditor (or where managers have little accounting flexibility).

The literature has emphasized two factors limiting the use of accrual manipulation: first, scrutiny from external monitors, including auditors and regulators; and, second, the degree of accounting flexibility. We now briefly describe why these factors are important and then provide details on how we measure them for our empirical analysis.

We first focus on external scrutiny from auditors as a cost of accrual manipulation. The accounting literature has emphasized audit quality as an important constraint on accounting manipulation (e.g., Becker et al., 1998; DeFond and Jiambalvo, 1991; Myers et al., 2003; Stice, 1991). This literature has demonstrated that high quality auditors constrain extreme accounting choices made by management when presenting the firm's financial performance. On the other hand, when audit quality is low, auditors do not constrain such questionable choices, resulting in a failure to detect misreporting or material fraud.

In our tests, we follow this literature and use auditor tenure ($AUDITORTENURE$) as a proxy for auditor scrutiny, based on data obtained from Compustat. Empirical evidence supports the assertion that as auditor tenure increases so too does overall audit quality. Geiger and Raghunandan (2002) identify a lack of knowledge of client-specific risks as a

[31]See Zang (2012) and references therein for an in-depth analysis and discussion of the costs of real and accrual-based earnings management.

key reason why auditors are less effective early in their tenure and, as a consequence, audit failures occur more frequently. Along these lines, prior research demonstrates that a larger fraction of audit failures occur on newly acquired clients and that auditors' litigation risk is greater in the early years of an engagement (Palmrose, 1991). Moreover, Myers et al. (2003) find that longer auditor tenure is associated with higher earnings quality, using a broad cross-section of firms and several different measures of accrual manipulation as proxies for earnings quality.[32]

In addition to scrutiny from external monitors, accrual manipulation may also be constrained by the flexibility within the accounting systems and procedures of the firm. Barton and Simko (2002) argue that accrual manipulation occurring in previous periods should accumulate on the balance sheet. In particular, if managers have biased earnings up in previous periods then this will be reflected in an "overstatement" of net operating assets.[33] Indeed, the authors find a strong positive association between the current level of net operating assets (relative to sales) and reported cumulative levels of abnormal accruals over the past five years. Taking this logic a step further, the authors hypothesize that managers will be constrained in their ability to bias up earnings via accrual manipulation if net assets have already been overstated in the past. If managers wish to stay within the limits of GAAP, then liberal choices made in the past regarding loss recognition and measurement should limit their ability to make similarly generous assumptions going forward. Consistent with

[32]The other side of this argument—which has been the subject of debate among academics, regulators, and policymakers—has been that longer auditor tenure could compromise independence and lead to auditors' support for accounting choices that "push the boundaries" of GAAP. Those in favor of the mandatory rotation of auditors argue that capping auditor tenure limits concerns about auditor capture and deteriorating audit quality. See Myers et al. (2003) for a detailed discussion of these issues, as well as empirical evidence in support of our approach.

[33]"Overstatement" describes the extent to which reported net assets exceed some benchmark that would have been recorded under an unbiased application of GAAP. Following Barton and Simko (2002), we use current sales as this benchmark. While current sales may also be subject to manipulation, as acknowledged by these authors, such a reporting bias would only be present in the current period. The results presented in this section are also robust to using the alternative definition of scaled net operating assets found in Hirshleifer et al. (2004).

this hypothesis, the authors find that the likelihood of narrowly meeting or beating analysts' consensus earnings forecasts is decreasing in the extent to which net operating assets are overstated on the balance sheet.

Following Barton and Simko (2002), we capture accounting flexibility using the beginning-of-year net operating assets relative to sales (NOA_{t-1}), where net operating assets is calculated as shareholders' equity less cash and marketable securities plus total debt.

We investigate how scrutiny from auditors and the degree of accounting flexibility impact the use of different types of earnings management in response to the loss of coverage. If these factors do constrain accrual manipulation, this would be evidenced by a smaller increase in accrual-based earnings management following the loss of coverage. Moreover, if these costs are particularly onerous then managers might not rebalance their earnings management strategies towards accrual-based methods at all. In this case, we would reasonably expect to see no adjustment in earnings management behavior following the loss of coverage.

To test how the use of different types of earnings management is affected by each these costs, we split our treatment sample into two groups, "High" and "Low" costs, depending on whether the cost variable is above or below the median among treated firms, in the year prior to the merger. We then estimate our baseline model—both for accrual-based and real earnings management (i.e., AM and RM_1)—on each group separately and examine how the treatment effect varies between groups.[34]

The results of this analysis are presented in Table 7. Columns 1 to 4 and 5 to 8 show how auditor tenure and accounting flexibility, respectively, impact both real and accrual-based earnings management behavior. The results are consistent with the substitution effect being muted where the costs of accrual manipulation are high. We find that the cross-sectional effect is concentrated among firms in the low cost subsamples. For this group, the estimated DiD coefficient is positive and statistically significant for accrual manipulation (AM) and

[34] The results (omitted for brevity) are similar when we consider RM_2.

negative and statistically significant for real activities manipulation (RM_1). On the other hand, in the high cost subgroup, the estimated treatment effects are indistinguishable from zero. Thus, we only observe an adjustment in earnings management behavior—a substitution from real activities to accrual manipulation—among those firms where the costs of accrual manipulation are not prohibitive.

Overall, the results uncovered here indicate that the extent of substitution from real to accrual-based earnings management varies systematically with accounting-based costs of earnings management tools that have been emphasized in the literature (e.g., Zang, 2012).

When taken together with the other results of Section 3.2, we have shown that the mix of real- and accrual-based earnings management techniques depends on several factors known to influence managers' incentives to manage earnings. This analysis gives us greater confidence in our baseline estimates and also strengthens our argument for the causal effect of analyst scrutiny on the trade-off between real and accrual-based earnings management.

3.3. Robustness of average treatment effect

In this section we perform a series of tests to examine the validity of our quasi-experiment and robustness of our estimated average treatment effect in Section 3.1. We first show that our results hold when we control for ex ante differences between treated and control firms, using both control variables in a linear framework and a matching estimator (Section 3.3.1). Next, we investigate the validity of our research design by testing for pre- and post-trends in our earnings management variables (Section 3.3.2). Finally, we consider several alternative measures of the manipulation of accruals and real activities and show that our baseline estimates are unaffected in terms of magnitudes and statistical significance (Section 3.3.3).

3.3.1. Controlling for ex ante differences

The identifying assumption of our quasi-experiment states that the average change in the earnings management behavior of treated firms across the merger date is not due to any factor aside from the merger leading to a drop in analyst following. There is substantial evidence in the literature corroborating this assumption. Notably, Wu and Zang (2009) investigate merger-related departures of financial analysts and find that they occur primarily among target analysts, especially those with overlapping coverage with analysts in the merger counterparty. The evidence we present in Tables 1 and 3 is consistent with these findings (see our discussion in Sections 2.1 and 3.1). In addition, Hong and Kacperczyk (2010) demonstrate that the loss of coverage is unrelated to changes in firms' characteristics across the merger date. Thus, the loss of coverage is unrelated to both analyst quality and changes in firm characteristics that might drive the observed adjustment in earnings management behavior.

While this evidence supports our identification assumption, in this section we nevertheless rule out the possibility that our estimates merely capture ex ante differences in the characteristics of treated and control firms. To this end, we adopt two distinct approaches: we first incorporate control variables into our baseline linear regression model and then we use a matching estimator. We now discuss these two approaches in turn.

First, we reestimate our baseline specification (1) controlling for the sources of firm-level heterogeneity discussed in Section 2.4. These control variables include size and performance, both of which are known to vary predictably with earnings management behavior (e.g., Kothari et al., 2005). Our panel regression specification easily allows us to control for such potential sources of differences across firms—time-varying firm-level characteristics that correlate with earnings management behavior—in addition to the numerous fixed effects we have included thus far.

Table 8 shows these results, indicating that our baseline estimate of the effect of analyst following on accrual and real activities manipulation is robust to controlling for a large set of time-varying observables. Both the effect of the mergers on coverage (Column 1) and the magnitude and statistical significance of the estimated average treatment effect are largely unaffected.

Second, we implement a difference-in-differences matching estimator. This alternative approach will be beneficial if treatment and control samples differ along unobservable dimensions. In particular, including control variables in a linear framework might not control for unobservable heterogeneity, especially if there exist nonlinearities in the data (Roberts and Whited, 2012).

We match each treated firm to a set of control firms on the basis of observable characteristics that are measured at the firm level in the year prior to the merger. Initially, we match on firm size ($LNSIZE$), then operating performance (ROA), then both size and operating performance. We match on these two characteristics for two reasons. First, the mergers in our sample involve large brokerage houses that tend to cover big stocks (see Table 2). Second, size and performance vary in a predictable way with the use of accrual- and real-based earnings management (Kothari et al., 2005; Zang, 2012). In a final step, we match on all covariates.

We utilize a nearest-neighbor propensity score matching procedure, originally proposed by Rosenbaum and Rubin (1983) and previously used in our context (e.g., Balakrishnan et al., 2013; He and Tian, 2013; Irani and Oesch, 2013). To implement this matching scheme, we first run a logit regression of an indicator variable for whether a particular firm-year is classified as treated (indicator equal to one) or control (indicator equal to zero) on our matching variables.[35] The sample used to estimate this regression consists of 1,264 treatment and 29,117 candidate control pre-merger firm-years. This is the sample of treated and control firms with all control variables available. The estimated coefficients from the logit regression

[35] The results are very similar when we use a probit regression to predict propensity scores.

are used to estimate probabilities of treatment for each firm-year in the sample. These probabilities (propensity scores) are then used to perform a nearest-neighbor match. We match with replacement using a standard tolerance (0.005 caliper) and allowing for up to three unique matches per treated firm. We use multiple matches per treated firm to improve the accuracy of our estimated treatment effects, which is possible as the number of candidate control firms exceeds the number of treated firms.

Table 9 shows the results from the matching estimator. Panel A displays the summary statistics for the treatment and matched control samples. The number of successful matches drops slightly as we include more covariates into the matching scheme. Importantly, the summary statistics indicate that, at least for the covariates we match on, the differences between treated firms and matched control firms are small in terms of economic magnitudes. This is a clear indication that the matching scheme performs rather well.

Panel B displays the impact of the mergers on coverage and earnings management. Column 1 verifies that the merger event continues to have a meaningful impact on the coverage if treated firms relative to the matched control sample. The remaining columns indicate that the difference-in-differences matching estimator produces quantitatively similar estimates of the average treatment effect, both in terms of economic magnitudes and statistical significance. The results in Panels B persist across all sets of matching variables.

Overall, these results indicate that our main results (see Table 3) are not driven by heterogeneity between treatment and control groups. We have shown that differences in size and operating performance between treatment and control samples do not drive our results. Our estimates are very similar in magnitude and statistical significance when we match on all covariates, thus providing further evidence in support of our empirical design.

Taking the results of both approaches together, this section provides strong evidence that the coverage loss is exogenous and the resulting adjustment in earnings management behavior is not a consequence of some form of omitted variables bias.

3.3.2. Validity of quasi-experiment

The validity of our difference-in-differences identification strategy hinges on the parallel trends assumption. This assumption requires that treated and control firms exhibit similar growth rates of earnings management behavior in the run up to the merger.

To verify this assumption, we now conduct a falsification analysis. We rerun our baseline analysis from Table 3, but mechanically shift each merger event date by one year forward (Panel A) or backward (Panel B). To illustrate, for Merger 1, we move the event date one year forward to 12/31/1993 in Panel A of Table 10 and one year backward to 12/31/1995 in Panel B. If our finding that firms adjust their behavior in response to the exogenous loss of coverage holds (and this adjustment is not simply part of an ongoing trend), we would expect to observe insignificant estimated DiD coefficients for both of these exercises.

Table 10 shows these results. The estimates shown in Panel A and Panel B of Table 10 are consistent with the interpretation that the mergers cause an adjustment behavior, and this behavior is not part of an ongoing trend. Regardless of specification and regardless of whether we artificially shift the merger event dates by one year forward or backward, the estimated average treatment effects are not statistically significant. This demonstrates that the adjustment in earnings management behavior among the treated firms takes place only around the merger event dates and is not due to some trend either in the pre- or the post-event window. This provides evidence that the parallel trends assumption holds in our setup. Moreover, this also directly addresses the potential concern that our results might simply be due to reversion to the mean in earnings management behavior among treated firms, since it is unlikely that mean reversion would happen only in the year of the merger and not in the years before or after.

3.3.3. Alternative measures of earnings management

In this section, we show that our results are robust to several alternative measures of earnings management outlined previously in Section 2.3.

The outcomes of these tests are reported in Table 11. We recalculate each of the main measures of real and accrual-based earnings management using the two-digit SIC industry classification when calculating the normal level of accruals and real activities manipulation. In addition, following Sloan (1996), we consider three non-regression-based measures for accrual-based earnings management, which we broadly term as current accruals (CA). Each of these measures make use of accounting data, but none use a regression model to compute abnormal accruals. In each case, a higher value of the measure indicates more accruals used in the firm's reporting.

We estimate (1) for each of these alternative measures of earnings management. The estimated β_3 in Table 11 indicate that our main results are robust across these different measures. Following a loss of analyst coverage, for each of the current accruals measures, firms' total accruals increase, indicating a bigger wedge between a firm's cash flows and earnings, making it harder for an investor to discern true performance. These findings are consistent with our key findings for real and accrual-based earnings management following the exogenous coverage loss. Likewise, the estimated treatment effect is robust to employing a two-digit SIC industry classification.

Finally, we examine the negative and positive components of discretionary accruals. Positive discretionary accruals are consistent with income-increasing manipulations and vice versa for negative discretionary accruals. Managers may be incentivized to boost income by using positive discretionary accruals. However, managers may also use negative discretionary accruals in order (to smooth earnings) to make future earnings benchmarks easier to meet (as in Acharya and Lambrecht, 2014). Thus far, we have considered manipulations in both directions—since we have been interested in the impact of analyst coverage on earnings management *per se*—but now we consider the use of positive and negative discretionary accruals separately.

The results from re-estimating our baseline specification (including merger, firm, and industry fixed effects) indicate a reduction in the use of positive discretionary accruals in response to the coverage loss. The estimated difference-in-differences coefficient for positive discretionary accruals is 0.044 with a t-value of 2.35. The equivalent point estimate for the negative discretionary accruals regression is small in magnitude and not statistically significant. These results are consistent with analysts impacting the use of income-increasing discretionary accruals, as opposed to earnings smoothing behavior through managers' use of accrual manipulation.

3.4. Comparison with OLS results

We wrap up our empirical analysis by estimating a series of pooled OLS regressions of each of our measures of earnings management on analyst following and the collection of control variables detailed in Section 3.3.1. More precisely, we estimate

$$EM_{it} = \alpha_t + \alpha_j + \alpha_i + \beta COVERAGE_{it} + \gamma' X_{it} + \epsilon_{it}, \tag{13}$$

using our earnings management variables, AM, RM_1, and RM_2 as left-hand side variables, where, depending on the specification we use, we also include year fixed effects (α_t), Fama-French industry fixed effects (α_j), firm fixed effects (α_i), and the same set of time-varying firm-level control variables used in the analysis thus far. To be comparable with the results from our natural experiment, we restrict our sample to the time period from 1994 until 2005.

The OLS regression estimates are shown in Table 12.[36] We present the results without

[36]Notice that the estimation sample used in Table 12 is smaller than the sample of the regressions estimated in Table 8. For the sample used in Table 12, every firm-year appears once, whereas, in Table 8,

any fixed effects, and then gradually introduce year, industry, and firm fixed effects. Overall, the coefficients on $COVERAGE$ are very small and approximately an order of magnitude lower than the estimates from our experiment. Moreover, these estimates depend on the fixed effects specification we use and are generally unstable and imprecisely estimated.

As we have mentioned throughout this study, these OLS estimates are tricky to interpret due to the endogenous relationship between analyst following and earnings management. This identification problem potentially explains mixed evidence on the use of real activities manipulation to meet analyst forecasts (e.g., Roychowdhury, 2006), as this methodology treats both exogenous (e.g., due to brokerage house mergers) and endogenous changes in analyst coverage equally. In contrast, the quasi-experimental design we employ identifies a specific—although pervasive in both the cross-section and time-series—collection of exogenous reductions in coverage. We use these events to isolate an economically meaningful and statistically significant effect, which is stable over many specifications and robustness tests.

4. Conclusion

We examine the causal effects of financial analyst coverage on earnings management. We use brokerage house mergers as a quasi-experiment to isolate reductions in analyst coverage that are exogenous to firm characteristics (Hong and Kacperczyk, 2010; Wu and Zang, 2009). Using a difference-in-differences methodology, we find that firms that lose analyst coverage reduce real activities manipulation and increase their use of accrual-based earnings management. An important implication of these results is that while analyst coverage may be associated with lower accrual-based earnings management (e.g., Irani and Oesch, 2013; Lindsey and Mola, 2013; Yu, 2008), pressure to meet analysts' expectations may nevertheless lead managers to resort to real activities manipulation. Given real activities manipulation

each firm-year can enter the sample multiple times. For example, a firm-year acts as a control firm-year for multiple mergers occurring within a short time-frame.

may entail costly deviations from normal business practices (Graham et al., 2005), this points to a potentially detrimental real effect of securities analyst coverage. Thus, our findings shed further light on how financial analysts affect firm value by providing a more complete picture of their on influence managers' overall earnings management strategy.

Finally, since analyst coverage and termination decisions correlate with firm characteristics for numerous reasons, the estimates found in existing studies tend to be biased because of endogeneity. The quasi-experiment we use addresses this identification problem by focusing on a large set of reductions in coverage—present throughout the time-series and cross-section of firms—that are orthogonal to the characteristics of the firm. This approach potentially has many other useful applications in the accounting and finance literature for studying the impact of analyst coverage on incentives and market outcomes. We look forward to future work along these lines.

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Table 1
Descriptive statistics for mergers

This table reports details of the merger events considered in this paper. The details were compiled from I/B/E/S following Hong and Kacperczyk (2010), as described in the text. The names and dates of the merging brokerage houses are included. For each merger, the brokerage house in the top row is the acquiring house and the brokerage house in the bottom row is the target. The table describes analyst career outcomes in the wake of the merger. The table also breaks out the number of stocks that were covered by both the merging brokerage houses and the overlap in coverage prior to the merger. These stocks make up our treatment sample and are the focus of this paper. The number of overlapping stocks retained is also included. This refers to the number of overlapping stocks from before the merger that continue to be covered by analysts retained at the new entity (that were previously employed at the bidder and by the target, respectively) in the year following the merger.

Brokerage House	IBES Identifier	Merger Date	Analyst employment			Stock coverage		
			Before	After	%Separation post-merger	#	Overlap	Overlap retained
Paine Webber	189	12/31/1994	52	43	17.3	816	171	121
Kidder Peabody	150		57	10	82.6	722		40
Morgan Stanley	192	5/31/1997	89	78	12.4	1,081	180	160
Dean Witter Reynolds	232		39	6	84.6	553		11
Smith Barney (Travelers)	254	11/28/1997	108	81	25.0	1367	256	164
Salomon Brothers	242		91	47	48.4	936		122
EVEREN Capital	829	1/9/1998	32	23	28.1	249	8	7
Principal Financial Securities	495		19	2	89.5	212		0
DA Davidson & Co	79	2/17/1998	7	5	29.6	108	12	4
Jensen Securities	932		5	5	0.0	73		11
Dain Rauscher	76	4/6/1998	50	31	38.0	459	39	11
Wessels Arnold & Henderson	280		17	11	35.3	201		24
First Union	282	10/1/1999	39	30	23.1	417	24	15
EVEREN Capital	829		37	13	64.9	277		1
Paine Webber	189	6/12/2000	62	49	21.0	758	17	11
JC Bradford	34		23	0	100.0	229		0
Credit Suisse First Boston	100	10/15/2000	141	113	19.9	1,359	299	178
Donaldson Lufkin and Jenrette	86		98	25	74.5	1,021		76
UBS Warburg Dillon Read	85	12/10/2000	121	91	24.8	936	165	107
Paine Webber	189		67	39	41.8	730		98
JP Morgan	873	12/31/2000	92	67	31.4	721	80	47
Chase Manhattan	125		50	35	30.0	598		34
Fahnestock	98	9/18/2001	19	12	36.8	161	7	7
Josephthal Lyon & Ross	933		14	0	100.0	121		0
Janney Montgomery Scott	142	3/22/2005	14	13	7.1	165	8	8
Parker/Hunter	860		5	3	40.0	64		1

Table 2
Summary statistics for the treatment and control samples

This table reports summary statistics for our treatment and control samples in the year prior to merger. The treatment sample consists of all stocks covered by two merging brokerage houses around the one-year merger window. The control sample is the remainder of the Compustat universe with the required data. Panel A reports summary statistics for the earnings management variables. Panel B reports summary statistics for the control variables. All variables are defined in Appendix A.

Variable	Treated firms						Control firms					
	N	Mean	Q1	Median	Q3	Std. dev.	N	Mean	Q1	Median	Q3	Std. dev.
Panel A: Earnings management variables												
AM	1,266	0.180	0.021	0.062	0.175	0.376	29,455	0.243	0.034	0.086	0.216	0.472
RM_1	1,266	0.114	-0.154	0.102	0.312	0.894	29,455	0.061	-0.239	0.089	0.394	1.195
RM_2	1,266	0.098	-0.129	0.034	0.198	1.021	29,455	0.108	-0.166	0.049	0.260	1.366
RM_{CFO}	1,266	-0.100	-0.209	-0.095	-0.014	0.391	29,455	-0.038	-0.205	-0.074	0.034	0.533
RM_{PROD}	1,266	-0.083	-0.208	-0.045	0.054	0.265	29,455	-0.076	-0.205	-0.050	0.077	0.305
RM_{DISX}	1,266	0.197	0.007	0.130	0.302	0.801	29,455	0.145	-0.078	0.110	0.351	1.056
Panel B: Control variables												
$COVERAGE$	1,264	22.868	14	22	31	11.333	29,115	7.269	2	5	10	7.181
$LNSIZE$	1,264	8.365	7.134	8.327	9.488	1.733	29,115	5.700	4.412	5.608	6.822	1.808
ROA	1,264	0.106	0.067	0.108	0.163	0.133	29,115	0.042	0.021	0.085	0.138	0.219
MTB	1,264	5.036	2.048	3.127	5.697	7.024	29,115	3.391	1.342	2.262	3.941	5.151
$SALESGR$	1,264	0.044	0.119	0.239	0.155	0.228	29,115	0.025	0.126	0.271	0.132	0.321
Panel C: Cross-sectional analysis variables												
$EXPERIENCE$	1,266	7.796	6.581	8	8.956	1.778	29,362	3.800	6.429	8.622	6.184	3.820
$HOUSEEXP$	1,266	4.411	5.500	6.411	5.436	1.508	29,362	2.500	4.062	5.857	4.209	2.815
$AUDITORTENURE$	1,266	5.454	3	5	8	2.212	29,455	5.257	3	5	7	2.498
NOA	1,266	0.702	0.530	0.669	0.806	0.402	29,455	0.725	0.494	0.690	0.872	0.496

Table 3

Baseline effects on coverage, accrual-based, and real earnings management

This table reports results from the estimation of (1). $POST$ is a variable that is equal to one for the post-merger period and zero for the pre-merger period. For each merger, we construct an indicator variable ($TREATED$) which is equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. $COVERAGE$ is the number of analysts covering a firm in the year prior to the merger. AM denotes our measure of accrual-based earnings management. RM_1 and RM_2 denote our measures for real earnings management. In Panel B, we classify treated firm-years into two groups depending on whether a firm's pre-merger level of $COVERAGE$ is above ($LOWCOVERAGE = 0$) or below ($LOWCOVERAGE = 1$) the median and allow the treatment effect to vary between these two groups. If indicated, the regressions include industry fixed-effects, merger fixed effects, or firm fixed effects. t-Values (in parentheses) are robust to clustering at the firm-level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

Panel A: Baseline effects

	$COVERAGE$	AM	AM	AM	AM	RM_1	RM_2
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
$POST$	-0.007	-0.022***	-0.022***	-0.024***	-0.024***	0.064***	0.049***
	(-0.264)	(-3.463)	(-3.468)	(-3.746)	(-3.637)	(8.591)	(7.021)
$TREATED$	15.624***	-0.112***	-0.068***	-0.040***	-0.021	0.067***	0.060***
	(30.508)	(-7.199)	(4.501)	(-2.847)	(-1.501)	(3.820)	(3.718)
$POST \times TREATED$	-0.633***	0.043**	0.043**	0.043**	0.043**	-0.095***	-0.089***
	(-4.442)	(2.165)	(2.172)	(2.166)	(2.101)	(-3.457)	(-3.319)
Merger fixed effects	No	No	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	No	No	Yes	Yes	Yes	Yes
Firm fixed effects	No	No	No	No	Yes	Yes	Yes
Number of observations	61,442	61,442	61,442	61,442	61,442	61,442	61,442
R-Squared	0.1389	0.001	0.058	0.156	0.354	0.332	0.297

Panel B: Conditional on pre-merger coverage level			
	AM	RM_1	RM_2
	[1]	[2]	[3]
$POST$	-0.024***	0.064***	0.048***
	(-3.64)	(8.59)	(7.02)
$TREATED$	-0.022	0.068***	0.060***
	(-1.52)	(3.88)	(3.75)
$POST \times TREATED \times 1\{LOWCOVERAGE = 1\}$	0.055**	-0.122***	-0.099***
	(2.01)	(-3.98)	(-3.42)
$POST \times TREATED \times 1\{LOWCOVERAGE = 0\}$	0.029	-0.064	-0.076*
	(1.22)	(-1.56)	(-1.94)
Merger fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Number of observations	61,442	61,442	61,442
R-Squared	0.354	0.386	0.297

Table 4
Channels of real activities manipulation

This table reports results from the estimation of (1). $POST$ is a variable that is equal to one for the post-merger period and zero for the pre-merger period. For each merger, we construct an indicator variable ($TREATED$) which is equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. $COVERAGE$ is the number of analysts covering a firm in the year prior to the merger. $-RM_{DISX}$, $-RM_{CFO}$, and RM_{PROD} denote measures of real earnings management based on abnormal production costs, cash flows from operations, and discretionary expenses, respectively. If indicated, the regressions include industry fixed-effects, merger fixed effects, or firm fixed effects. t-Values (in parentheses) are robust to clustering at the firm-level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

Panel A: Abnormal discretionary expenses

$-RM_{DISX}$	[1]	[2]	[3]	[4]
$POST$	0.049***	0.049***	0.048***	0.048***
	(7.654)	(7.639)	(7.536)	(7.180)
$TREATED$	0.0032**	0.061***	0.085***	0.056***
	(1.986)	(3.659)	(5.343)	(3.714)
$POST \times TREATED$	-0.077***	-0.077***	-0.077***	-0.077***
	(-3.211)	(-3.214)	(-3.201)	(-3.071)
Merger fixed effects	No	Yes	Yes	Yes
Industry fixed effects	No	No	Yes	Yes
Firm fixed effects	No	No	No	Yes
Number of observations	61,442	61,442	61,442	61,442
R-Squared	0.001	0.017	0.082	0.345

Panel B: Abnormal cash flows from operations

$-RM_{CFO}$	[1]	[2]	[3]	[4]
$POST$	0.011***	0.011***	0.010***	0.011***
	(3.439)	(3.450)	(3.300)	(3.312)
$TREATED$	-0.048***	-0.051***	-0.048***	0.012*
	(-5.586)	(-5.921)	(-5.643)	(1.758)
$POST \times TREATED$	-0.017	-0.017	-0.018*	-0.018
	(-1.594)	(-1.592)	(-1.658)	(-1.624)
Merger fixed effects	No	Yes	Yes	Yes
Industry fixed effects	No	No	Yes	Yes
Firm fixed effects	No	No	No	Yes
Number of observations	61,442	61,442	61,442	61,442
R-Squared	0.002	0.027	0.067	0.348

Panel C: Abnormal production costs				
RM_{PROD}	[1]	[2]	[3]	[4]
$POST$	0.010***	0.010***	0.011***	0.011***
	(5.741)	(5.742)	(6.081)	(5.834)
$TREATED$	-0.013	-0.021**	-0.021**	0.001
	(-1.358)	(-2.022)	(-2.301)	(0.191)
$POST \times TREATED$	-0.001	-0.001	-0.001	-0.001
	(-0.087)	(-0.094)	(-0.134)	(-0.178)
Merger fixed effects	No	Yes	Yes	Yes
Industry fixed effects	No	No	Yes	Yes
Firm fixed effects	No	No	No	Yes
Number of observations	61,442	61,442	61,442	61,442
R-Squared	0.000	0.005	0.093	0.665

Table 5
Suspect firms at the zero earnings threshold

This table reports results from estimating (1) with a heterogenous treatment effect that accounts for whether a firm is suspect or not. Treated firms that just meet or beat the zero earnings benchmark in the pre-merger period are assigned "suspect" status. Accordingly, an indicator variable $SUSPECT$ is set equal to one for treated firms with earnings before extraordinary items over lagged assets between 0 and 0.5%. We use our measures of accrual-based earnings management (AM) and real activities manipulation (RM_1 and RM_2) as dependent variables. $POST$ is a variable that is equal to one for the post-merger period and zero for the pre-merger period. For each merger, we construct an indicator variable ($TREATED$) which is equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. If indicated, the regressions include industry fixed-effects, merger fixed effects, or firm fixed effects. t-Values (in parentheses) are robust to clustering at the firm level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

	AM	RM_1	RM_2
	[1]	[2]	[3]
$POST$	-0.024***	0.064***	0.048***
	(-3.68)	(8.58)	(7.01)
$TREATED$	-0.021	0.067***	0.060***
	(-1.49)	(3.81)	(3.71)
$POST \times TREATED \times 1\{SUSPECT = 1\}$	0.108**	-0.206***	-0.157**
	(2.18)	(-3.03)	(-2.32)
$POST \times TREATED \times 1\{SUSPECT = 0\}$	0.033	-0.076***	-0.077***
	(1.56)	(-2.61)	(-2.77)
Merger fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Number of observations	61,442	61,442	61,442
R-Squared	0.354	0.388	0.297

Table 6
Impact of analyst experience

This table reports results from estimating (1) with a heterogenous treatment effect that accounts for whether treated firms are covered by experienced analysts or not. Treated firms are classified as having high and low analyst experience status using two schemes. First, if the average number of years that analysts following the firm have worked as an analyst. Second, the average number of years that analysts following the firm have worked as an analyst at their current brokerage house. An indicator variable EXP is set equal to one for treated firms with above median firm-level analyst experience in the pre-merger period and zero otherwise. We use our measures of accrual-based earnings management (AM) and real activities manipulation (RM_1 and RM_2) as dependent variables. $POST$ is a variable that is equal to one for the post-merger period and zero for the pre-merger period. For each merger, we construct an indicator variable ($TREATED$) which is equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. If indicated, the regressions include industry fixed-effects, merger fixed effects, or firm fixed effects. t-Values (in parentheses) are robust to clustering at the firm level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

	General experience			House experience		
	AM	RM_1	RM_2	AM	RM_1	RM_2
	[1]	[2]	[3]	[4]	[5]	[6]
$POST$	-0.023***	0.064***	0.049***	-0.024***	0.064***	0.049***
	(-3.68)	(8.58)	(7.01)	(-3.68)	(8.58)	(7.01)
$TREATED$	-0.030**	0.072***	0.069***	-0.030**	0.072***	0.069***
	(-2.11)	(3.93)	(4.18)	(-2.12)	(3.92)	(4.19)
$POST \times TREATED \times 1\{EXP = 0\}$	0.094*	-0.154***	-0.180***	0.111**	-0.128**	-0.176***
	(1.66)	(-2.59)	(-3.11)	(2.18)	(-2.09)	(-3.06)
$POST \times TREATED \times 1\{EXP = 1\}$	0.053***	-0.111***	-0.102***	0.049**	-0.116***	-0.101***
	(2.79)	(-3.73)	(-3.59)	(2.51)	(-3.89)	(-3.56)
Merger fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	61,256	61,256	61,256	61,256	61,256	61,256
R-Squared	0.353	0.388	0.300	0.353	0.387	0.297

Table 7
Impact of costs of accruals management

This table reports results from the estimation of (1), with the sample split based on the costs of accruals management. Treated firms are classified according to the cost of accrual manipulation. We cut the sample first according to the median of auditor tenure ($AUDITORTENURE$) of treated firms and then based on the median of accounting flexibility measured by net operating assets (NOA). Firms with above-median auditor tenure and below-median NOA are assigned to each of the "High" cost subsamples. We use our measures of accrual-based earnings management (AM) and real activities manipulation (RM_1) as dependent variables. $POST$ is a variable that is equal to one for the post-merger period and zero for the pre-merger period. For each merger, we construct an indicator variable ($TREATED$) which is equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. Regressions include industry fixed-effects, merger fixed effects, and firm fixed effects. t-values (in parentheses) are robust to clustering at the firm level. ***, **, * denotes 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

	Auditor tenure				Accounting flexibility			
	AM		RM_1		AM		RM_1	
	High	Low	High	Low	High	Low	High	Low
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
$POST$	-0.017**	-0.046***	0.021**	0.125***	0.012	-0.037***	0.017*	0.071***
	(-2.012)	(-4.718)	(2.023)	(9.531)	(1.263)	(-3.963)	(1.833)	(6.548)
$TREATED$	-0.037*	-0.044**	0.030	0.077***	-0.015	-0.048***	0.008	0.077***
	(-1.694)	(-2.501)	(1.102)	(2.931)	(-0.659)	(-2.613)	(0.358)	(3.544)
$POST \times TREATED$	0.028	0.053*	-0.027	-0.146***	0.001	0.089***	-0.056	-0.122***
	(0.998)	(1.944)	(-0.661)	(-3.309)	(0.044)	(3.473)	(-1.386)	(-3.340)
Merger fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	31,848	29,594	31,848	29,594	30,085	31,357	30,085	31,357
R-Squared	0.168	0.162	0.476	0.473	0.413	0.402	0.354	0.367

Table 8
Robustness: Average treatment effect with control variables

This table reports results from the estimation of (1) with additional control variables. For each merger, we consider a one-year window prior to the merger (pre-merger window) and a one-year window after the merger (post-merger window). The dependent variables are $COVERAGE$ in the first column, AM in the second column, RM_1 in the third column and RM_2 in the fourth column. $POST$ is a variable that is equal to one for the post-merger period and zero for the pre-merger period. For each merger, we construct an indicator variable ($TREATED$) which is equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. If indicated, the regressions include industry fixed-effects, merger fixed effects, or firm fixed effects. t-Values (in parentheses) are robust to clustering at the firm level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

	$COVERAGE$	AM	RM_1	RM_2
	[1]	[2]	[3]	[4]
$POST$	0.054**	-0.008	0.051***	0.023***
	(1.978)	(-1.223)	(6.660)	(3.312)
$TREATED$	1.928***	-0.0150	0.055***	0.049***
	(11.417)	(-1.081)	(3.204)	(3.131)
$POST \times TREATED$	-0.758***	0.040*	-0.094***	-0.087***
	(-5.104)	(1.954)	(-3.387)	(-3.261)
$LNSIZE$	1.402***	0.027***	0.033***	0.015*
	(21.145)	(3.572)	(3.608)	(1.869)
ROA	-1.188***	0.009	0.007	0.020
	(-5.004)	(0.0201)	(0.138)	(0.436)
MTB	-0.081***	-0.002	0.002	0.002
	(-8.368)	(-1.357)	(1.504)	(1.591)
$SALESGR$	0.600***	-0.089*	0.141***	0.213***
	(7.458)	(-5.362)	(6.911)	(10.702)
$COVERAGE$		-0.002*	0.005***	0.005***
		(-1.811)	(3.293)	(3.384)
Merger fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Number of observations	60,758	60,758	60,758	60,758
R-Squared	0.884	0.301	0.337	0.305

Table 9
Robustness: Difference-in-differences matching estimator

This table reports summary statistics and results of using a difference-in-differences matching estimator. The treatment sample consists of all stocks covered by two merging brokerage houses around the one-year merger window with valid matching variables. The control sample is the remainder of the Compustat universe with valid matching variables. Treated firms are matched using a nearest-neighbor logit propensity score match using a 0.005 caliper and matching up to three control firms. Panel A shows summary statistics for the treatment and matched control samples in the year prior to merger. N and M represent total the number of treatment and matched control firms, respectively. Panel B estimates the effect of the brokerage house mergers on *COVERAGE* and *EM* variables. *t*-values (in parentheses) are robust to clustering at the firm-level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

Panel A: Summary statistics for matched samples

Variable	Treated firms			Matched control firms			Difference	
	Mean	Std.	Med.	Mean	Std.	Med.	in means	*t*-Stat.
i) Matching on LNSIZE (N = 1,255; M = 3,749)								
LNSIZE	8.355	1.723	8.327	8.339	1.707	8.315	0.016	(0.292)
ROA	0.107	0.136	0.108	0.114	0.127	0.120	-0.007*	(-1.650)
COVERAGE	22.839	11.335	22	19.266	11.169	18	3.573***	(9.702)
MTB	5.869	6.811	3.109	7.960	6.244	3.728	-2.091	(-0.961)
SALESGR	0.156	0.228	0.119	0.161	0.228	0.125	-0.005	(0.637)
ii) Matching on ROA (N = 1,258; M = 3,753)								
LNSIZE	8.366	1.735	8.327	8.501	1.760	8.528	-0.154**	(-2.474)
ROA	0.107	0.136	0.108	0.116	0.131	0.117	-0.009*	(-1.800)
COVERAGE	22.728	11.168	22	21.994	11.086	21	0.734*	(1.836)
MTB	5.854	6.803	3.123	8.293	8.401	3.827	-2.438	(-0.902)
SALESGR	0.154	0.226	0.120	0.162	0.226	0.125	-0.008	(1.001)
iii) Matching on LNSIZE/ROA (N = 1,255; M = 3,751)								
LNSIZE	8.344	1.709	8.320	8.622	1.708	8.677	-0.277***	(-4.633)
ROA	0.107	0.136	0.108	0.116	0.134	0.123	-0.010**	(-2.034)
COVERAGE	22.759	11.230	22	22.121	11.091	21	0.638	(1.628)
MTB	5.841	6.819	3.091	9.500	7.823	3.845	-3.658	(-1.449)
SALESGR	0.155	0.228	0.119	0.167	0.227	0.125	-0.011	(-1.329)
iv) Matching on all covariates (N = 1,252; M = 3,748)								
LNSIZE	8.344	1.709	8.320	8.638	1.715	8.717	-0.293***	(-4.895)
ROA	0.107	0.136	0.108	0.116	0.131	0.120	-0.009*	(-1.956)
COVERAGE	22.759	11.230	22	22.160	11.161	21	0.599	(1.524)
MTB	5.841	6.819	3.091	9.411	7.896	3.803	-3.569	(-1.411)
SALESGR	0.154	0.226	0.119	0.162	0.329	0.125	-0.008	(-0.952)

Panel B: Average treatment effects for matched samples				
	$COVERAGE$	AM	RM_1	RM_2
	[1]	[2]	[3]	[4]
$LNSIZE$-matched	-0.794***	0.085***	-0.084***	-0.098***
(Standard error)	(0.141)	(0.019)	(0.026)	(0.024)
ROA-matched	-0.644***	0.040**	-0.106***	-0.109***
(Standard error)	(0.128)	(0.018)	(0.024)	(0.022)
$LNSIZE/ROA$-matched	-0.860***	0.093***	-0.094***	-0.098***
(Standard error)	(0.141)	(0.021)	(0.025)	(0.023)
$LNSIZE/.../SALESGR$-matched	-0.835***	0.081***	-0.103***	-0.117***
(Standard error)	(0.139)	(0.021)	(0.024)	(0.022)

Table 10
Validity of the quasi-experiment: Placebo regressions

This table reports results from estimating (1). In Panel A, we shift the one-year window prior/after the merger window by one year into the future. In Panel B, we shift the one-year window prior/after the merger window by one year into the past. We use our measures of accrual-based earnings management (AM) and real activities manipulation (RM_1 and RM_2) as dependent variables. $POST$ is a variable that is equal to one for the (shifted) post-merger period and zero for the (shifted) pre-merger period. For each merger, we construct an indicator variable ($TREATED$) which is equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. If indicated, the regressions include industry fixed-effects, merger fixed effects, or firm fixed effects. t-Values (in parentheses) are robust to clustering at the firm level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

Panel A: Event date shifted one year forward

	AM	RM_1	RM_2
	[1]	[2]	[3]
$POST$	0.080***	0.130***	0.109***
	(9.153)	(17.301)	(13.732)
$TREATED$	0.025	0.004	0.044*
	(1.353)	(0.233)	(1.953)
$POST \times TREATED$	-0.030	-0.000	-0.026
	(-1.143)	(-0.036)	(-0.918)
Merger/Industry/Firm FE	Yes	Yes	Yes
Number of observations	60,292	60,292	60,292
R-Squared	0.353	0.292	0.237

Panel B: Event date shifted one year backward

	AM	RM_1	RM_2
	[1]	[2]	[3]
$POST$	0.049***	0.109***	0.025***
	(10.401)	(9.928)	(4.441)
$TREATED$	-0.018**	-0.012	-0.044**
	(-2.079)	(-0.548)	(-2.559)
$POST \times TREATED$	0.012	-0.017	-0.009
	(0.847)	(-0.851)	(-0.436)
Merger/Industry/Firm FE	Yes	Yes	Yes
Number of observations	53,196	53,196	53,196
R-Squared	0.342	0.293	0.284

Table 11
Robustness: Alternative measures of earnings management

This table reports results from the estimation of (1). For brevity, we only report the estimated coefficient on the $POST \times TREATED$ interaction. The dependent variables are listed in the first column and are alternative measures of EM. For each merger, we consider a one-year window prior to merger (pre-merger window) and a one-year window after the merger (post-merger window). $POST$ is a variable that is equal to one for the post-merger period and zero for the pre-merger period. For each merger, we construct an indicator variable ($TREATED$) which is equal to one for each stock covered by both merging brokerage houses (treatment sample) and zero otherwise. All regressions include industry fixed-effects, merger fixed effects, or firm fixed effects. t-Values (in parentheses) are robust to clustering at the firm level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

	AM (SIC 2)	RM_1 (SIC 2)	RM_2 (SIC 2)	CA	CA (Cash Flow)	CA (exc. Depr.)
	[1]	[2]	[3]	[4]	[5]	[6]
$POST$	0.019*	0.045***	0.020**	-0.021***	-0.018***	-0.020***
	(1.894)	(5.388)	(2.539)	(-3.418)	(-3.271)	(-3.324)
$TREATED$	-0.040**	0.048**	0.033*	-0.018	-0.021*	-0.017
	(-2.113)	(2.552)	(1.933)	(-1.621)	(-1.714)	(-1.598)
$POST \times TREATED$	0.059*	-0.074**	-0.057*	0.010***	0.011***	0.011***
	(1.884)	(-2.524)	(-1.939)	(2.958)	(2.753)	(2.809)
Merger fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	60,718	60,718	60,718	61,822	61,822	61,822
R-Squared	0.264	0.372	0.286	0.194	0.171	0.110

Table 12
Analyst coverage and earnings management: OLS estimation

This table reports results from panel regressions of earnings management measures on analyst coverage and control variables that do not account for the endogeneity of analyst coverage. We use our measures of accrual-based earnings management (AM) and real activities manipulation (RM_1 and RM_2) as dependent variables. If indicated, the regressions include industry fixed-effects, year fixed effects, or firm fixed effects. t-Values (in parentheses) are robust to clustering at the industry level. ***, **, * Denote 1%, 5%, and 10% statistical significance. All variables are defined in Appendix A.

	AM	RM_1	RM_2	AM	RM_1	RM_2	AM	RM_1	RM_2	AM	RM_1	RM_2
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
COVERAGE	-0.003**	-0.002	-0.001	0.003***	-0.001	-0.001	0.002**	0.000	0.000	0.002	0.005**	0.003*
	(-2.526)	(-1.475)	(-1.366)	(2.666)	(-0.546)	(-0.683)	(2.187)	(0.123)	(0.496)	(0.973)	(2.301)	(1.901)
LNSIZE	0.042***	0.020***	0.009**	-0.019***	0.010*	0.005	-0.019***	0.012**	0.003	0.016	0.019*	0.000
	(7.031)	(3.748)	(2.234)	(-3.535)	(1.896)	(1.387)	(-3.688)	(2.280)	(0.717)	(1.297)	(1.809)	(0.007)
ROA	-0.622***	0.109***	-0.107***	-0.400***	0.124***	-0.137***	-0.162***	0.161***	-0.100***	0.314***	-0.025	-0.054
	(-15.455)	(2.777)	(-3.734)	(-10.523)	(3.153)	(-4.784)	(-4.302)	(4.132)	(-3.490)	(3.523)	(-0.391)	(-1.001)
MTB	0.007***	-0.016***	-0.009***	0.011***	-0.017***	-0.011***	0.004**	-0.019***	-0.012***	-0.003	-0.004**	-0.005***
	(3.027)	(-9.054)	(-7.078)	(5.693)	(-9.728)	(-8.188)	(1.995)	(-10.823)	(-9.210)	(-1.244)	(-2.098)	(-2.963)
SALESGR	-0.277***	0.185***	0.231***	-0.273***	0.167***	0.234***	-0.199***	0.197***	0.260***	-0.056*	0.234***	0.308***
	(-11.232)	(8.587)	(12.903)	(-11.772)	(7.789)	(13.415)	(-8.524)	(9.197)	(14.879)	(-1.704)	(8.436)	(12.620)
Year fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Number of observations	25,125	25,125	25,125	25,125	25,125	25,125	25,125	25,125	25,125	25,125	25,125	25,125
R-Squared	0.023	0.013	0.019	0.184	0.060	0.080	0.247	0.090	0.105	0.501	0.464	0.391

Appendix A: Variable definitions

This appendix presents the definitions for the variables used throughout the paper.

Panel A: Earnings management variables

Variable	Definition
AM	Absolute abnormal accruals computed as the difference between a company's total accruals and its nondiscretionary accruals.
RM_{CFO}	Abnormal cash flows from operations calculated following Roychowdhury (2006)
RM_{PROD}	Abnormal production costs calculated following Roychowdhury (2006)
RM_{DISX}	Abnormal discretionary expenses calculated following Roychowdhury (2006)
RM_1	Combined real earnings management measure computed as the sum of RM_{DISX} and RM_{PROD}
RM_2	Combined real earnings management measure computed as the sum of RM_{DISX} and RM_{CFO}
CA	Non-regression current accruals measure as in Sloan (1996)
CA (Cash Flow)	Current accruals measure as in Hribar and Collins (2002)
CA (exc. Depr.)	Current accruals measure excluding depreciation as in Barton and Simko (2002)

Panel B: Control variables

Variable	Definition
$COVERAGE$	Number of analysts in I/B/E/S covering stock in current year
$LNSIZE$	Natural logarithm of market capitalization (price times shares outstanding)
ROA	Return on assets calculated as net income divided by total assets
MTB	Natural logarithm of a firm's book value divided by its market capitalization
$SALESGR$	One year growth in a firm's sales

Panel C: Cross-sectional analysis variables

Variable	Definition
$SUSPECT$	Earnings before extraordinary items divided by assets between 0 and 0.05%
$EXPERIENCE$	Average number of years in I/B/E/S database for analysts covering firm
$HOUSEEXP$	Average number of years at current brokerage house for analysts covering firm
$AUDITORTENURE$	Number of years a company has retained the same auditor
NOA	Net operating assets computed from cash flow statement following Barton and Simko (2002)