Analyst Coverage and Earnings Management: Quasi-Experimental Evidence*

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Abstract

Do securities analysts serve as effective external monitors, or do they pressure managers to focus on short-term performance? To explore this question, 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 stock-level coverage resulting from brokerage house mergers. We find that managers respond to a loss of coverage by decreasing real activities manipulation, while increasing their use of accrual-based earnings management. These effects are attributable to firms with low initial analyst coverage and also vary systematically with proxies for the costs of earnings management. Our causal evidence suggests that managers use real activities manipulation to enhance short-term performance and meet analyst forecasts, effects that are not uncovered when focusing solely on accrual-based earnings management.

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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, 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), as well as potential macroeconomic distortions—excessive hiring and investment—that could

[1]The manipulation of reported earnings is suitably defined in Healy and Wahlen (1999) p.6: "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."

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

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., Cohen et al., 2008; Roychowdhury, 2006; Zang, 2012).[3] 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.[4] Accordingly, the purpose of this paper is to provide the first observational empirical study into how securities analysts simultaneously affect 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) finds 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 from a shareholder perspective.[5] Thus, if analyst following pressures managers to

[3]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.

[4]Recent research finds that greater analyst coverage results in fewer discretionary accruals used in corporate financial reporting (Chen et al., 2013; 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.

[5]"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).

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) argues that managers prefer real activities manipulation because it may be harder to detect than accrual-based methods and thus entails lower expected private costs. To support this argument, Cohen et al. (2008) 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. Thus, if analysts monitor managers alongside regulators and other stakeholders, as previous research ascertains (e.g., 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 complicated 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 the 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)].[6] 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 may arise 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 occurring between 1994 and 2005 and accommodates all publicly traded U.S. firms. Associated 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

[6]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., 2012; 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, 2012), innovation (He and Tian, 2013), and the interaction of corporate disclosure and governance (Irani and Oesch, 2013) and stock liquidity (Balakrishnan et al., 2012).

real activities manipulation is coming primarily from a reduction in abnormal discretionary expenses, including R&D expenses. This suggests that analyst following pressures managers to utilize real activities manipulation in order to meet expectations, for instance, by disincentivizing innovative activity.[7] 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 non-linear and more pronounced for treated firms with low initial coverage. This validates our identification strategy by providing direct evidence that earnings management responds to large percentage drops in analyst coverage. 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. 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 exam-

[7]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).

ine the validity of our quasi-experiment—particularly, the parallel trends assumption—by implementing 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.[8] This is in contrast to the robust directional effects we uncover using our identification strategy. Moreover, these OLS 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 accrual-based 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 causal relationship and demonstrate that a reduction in analyst coverage causes an adjustment in earnings management. 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. 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. 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

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

studies have emerged examining this form of earnings management and whether there is any complementary or substitute interaction with accrual-based practices.[9] Zang (2012) assesses the tradeoffs between accrual manipulation and real earnings management and, by focusing on the timing and various 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, Cohen et al. (2008) studies the impact of the Sarbannes-Oxley Act (SOX) on the use of accrual-based versus real activities manipulation, finding that managers substitute towards real activities manipulation in the post-SOX era. 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

[9]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—in order to mimic highly productive firms, fool investors, and avoid detection. In a model of real and financial inter-temporal smoothing, Acharya and Lambrecht (2011) show that managers may underreport earnings and underinvest in order to manage outsiders' expectations. In these asymmetric information frameworks, under certain conditions, the two earnings management tools are complements.

a consequence of endogeneity (omitted variables bias, reverse causality, etc.).[10] 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 (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

[10]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) finds that firms increase the volume of public financial guidance in reaction to a loss of analyst coverage.

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.[11] 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 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 target—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 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. Prior to this merger, there were 180 (treated) stocks that were covered by both Morgan Stanley and Dean Witter Reynolds. After the merger, the merged entity had fewer analysts and, in particular, due to redundancy, fewer of the analysts with coverage overlap prior to the merger. More precisely, Morgan Stanley had 89 analysts prior to the merger, Dean Witter Reynolds had 39, and the combined entity kept a total of 84.

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

We replicate this procedure for each of the remaining 12 mergers and identify a total of 1,938 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. 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, 2012; 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 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 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. In Section 3.3, we mitigate this concern by incorporating control variables into our linear regression framework.

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.[12]

2.2. Sample construction

In this section we detail how we construct our sample in order to implement the identification strategy described previously. First, we construct our sample by collecting 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.

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

[12]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.

our primary measures of earnings management (AM and RM, as defined below). This requirement results in a final sample of 61,822 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 61,138 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.[13] 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 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

[13] 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.

(item *ppegt*). These variables are all normalized by lagged total assets (item *at*).[14]

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.[15,16]

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

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

where $\Delta C.A$ 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,

[14] 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.

[15] In Section 3.3, we also consider the positive and negative components of abnormal discretionary accruals.

[16] 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).

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 AM.

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), which argues that managers have limited discretion over depreciation schedules in the short run.

The third non-regression-based measure follows Hribar and Collins (2002), which shows that using consecutive annual balance sheet variables can be problematic for the estimation of accruals for firms with merger and acquisitions activities, significant foreign currency accounts, or discontinued operations. 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 also identifies discrepancies between earnings and cash flows, but it is based on data from the income and cash flows statement, as opposed to the balance sheet.

Construction of a valid RM proxy uses the model introduced in Dechow et al. (1998), as implemented in Roychowdhury (2006) among others (e.g., Cohen et al., 2008; Cohen and Zarowin, 2010; 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.[17]

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:

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

Abnormal CFO (RM_{CFO}) is actual CFO minus the normal level of CFO calculated using the estimated coefficient 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

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

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 as computed following Zang (2012) and Cohen and Zarowin (2010)

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

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

Higher values of RM_1 and RM_2 imply that the firm is more likely to have used real activities manipulation.[18,19]

[18] RM_{PROD} is not multiplied by minus one as higher production costs suggest excess production in order to 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.

[19] 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.

2.4. Control variables

The empirical specification (1) enables us to include control variables in order to mitigate concerns that observable differences among treated and control firms drive any estimated average treatment effect.

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; 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 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 earnings ($EARN$) computed as earnings before interest and taxes. 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 61,822 to 61,138 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 costs of earnings management variables, 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. In robustness tests, we will demonstrate 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 investigate how this treatment effect varies with 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 roughly one analyst relative to non-treated firms in the year following merger. We examine whether this is the case by replacing EM with analyst coverage ($COVERAGE$) on the left-hand side in (1). The first column of Table 3 confirms that our quasi-experiment is relevant. The estimated coefficient is -0.648 with a t-value of -4.48. This is consistent in terms of size and significance with research using a similar experimental design (e.g., Derrien and Kecskes, 2012; 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 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 that could affect 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 (Chen et al., 2013; Irani and Oesch, 2013; Lindsey and Mola, 2013; Yu, 2008).

In columns 6 and 7, we examine the impact of the 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 -0.229 with a t-value of -4.41. 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 larger 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 the coverage drop. 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 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 (as in Yu, 2008), 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, simultaneously 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 those 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 low initial 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 depending on whether coverage in the year prior to the merger is above or below the median among treated firms. Mean coverage in the below(above)-median initial 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 initial 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 AM regression is positive and statistically significant, and negative and significant for the RM regressions. This is not the case for the high coverage subgroup. 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.[20] Panel A displays the results for RM_{PROD}, Panel B for RM_{CFO}, and Panel C for RM_{DISX}. 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. We do so in order to demonstrate the robustness of the point estimates to these potential sources of heterogeneity.

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

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

coverage drop is coming primarily from abnormal cash flow from operations and 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), which argues 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 are inconsistent with a pure monitoring role of analysts and raise the possibility that analysts pressure managers to meet earnings targets via real activities manipulation.

3.2. Impact of the costs of accrual manipulation

Differences in the relative costs of real and accrual-based earnings management methods—determined by firms' accounting and operational environments—should influence managers' optimal mix of the two strategies. In this section, we show that the extent of substitution from real to accrual-based earnings management, following the coverage shock, varies in the cross-section of firms. We focus on the costs of accrual manipulation and show that firms with high pre-shock costs of accrual manipulation do not substitute away from real activities manipulation to the same extent as firms with low costs.

The literature has emphasized two factors limiting the use of accrual manipulation: first, scrutiny from external monitors; and, second, the degree of accounting flexibility. A high-quality auditor may not permit overly-aggressive accounting estimates relative to low-quality auditors (e.g., Becker et al., 1998; DeFond and Jiambalvo, 1991). This may be a consequence of skill, career concerns, or auditor capture, among other reasons. In addition, accrual manipulation may be more likely to be detected when industry regulators increase their scrutiny of firms' accounting practices (Dyck et al., 2010).

As well as by scrutiny from external monitors, accrual manipulation is constrained by

the flexibility within the accounting systems and procedures of the firm. For instance, there may be a higher likelihood of detecting accrual manipulation among firms that have made aggressive accounting assumptions in the past. These firms will be more likely to violate accounting standards with further use of accrual-based management.

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 adjust 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 post-shock.

To proxy auditor scrutiny, we use auditor tenure ($AUDITORTENURE$), which we obtain from Compustat. As has been argued in the literature, as audit tenure increases, so too does the likelihood of detecting accounting errors as well as overall audit quality (Stice, 1991). Accordingly, there is a negative correlation between tenure and measures of accrual-based earnings management due to familiar auditors placing greater constraints on managerial discretion (Myers et al., 2003).

We proxy accounting flexibility using the Barton and Simko (2002) balance sheet measure of prior accounting decisions: beginning-of-year net operating assets, NOA_{t-1}, where NOA is calculated as shareholders' equity less cash and marketable securities and plus total debt.[21] This measure captures the extent of accrual manipulation in prior years, which places a constraint on the ability of managers to manage contemporaneous accruals due to limited flexibility within accounting standards and procedures. The justification for using lagged net operating assets is as follows. Due to concordances between the balance sheet and

[21]The results presented in this section are also robust to using the alternative definition of net operating assets found in Hirshleifer et al. (2004).

income statement, abnormal accruals reflected in prior earnings must also be reflected in net assets. Thus, net assets are overstated when firms engage in accrual manipulation in previous periods.

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 AM and RM_1—on each group separately and examine how the treatment effect varies between groups.[22]

The results of this analysis are presented in Table 5. 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 AM and negative and statistically significant for RM. 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 cross-sectional results uncovered here indicate that the extent of substitution from real to accrual-based earnings management varies systematically with costs of earnings management tools that have been emphasized in the literature (e.g., Zang, 2012).

3.3. Robustness of average treatment effect

This section performs several tests to examine the validity of our quasi-experiment and robustness of our estimated average treatment effect in Section 3.1.

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

3.3.1. Controlling for ex ante differences

Estimates from the regression model (see Table 3) are unbiased if the average change in earnings management (accrual-based and real) of treated firms across the merger date is not due to any factor aside from the merger leading to a drop in analyst following. This is a statement of the exogeneity assumption of our experiment. We believe that the drop in analyst following we examine is plausibly exogenous, as the merger-related departures of analysts is likely due to redundancy or culture clash (Wu and Zang, 2009).

That being said, it is still possible that our estimated partial effect may be capturing differences in the characteristics of treated and control firms. To address this issue, we incorporate control variables into our baseline regression model. These include size and performance which are known to vary predictably with earnings management behavior. 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—that are not controlled for by the numerous fixed effects we include. In this spirit, we estimate (1) including the sources of heterogeneity discussed in Section 2.4.

Table 6 shows these results, indicating that our baseline estimate of the effect of analyst following on AM and RM 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.

This is 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 identification strategy depends on the parallel trends assumption. This means that treated and control firms must have similar growth rates of earnings manage-

ment behavior before the merger. To verify this assumption, we now conduct a falsification analysis.

Table 7 shows these results. 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 7 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.

The estimates shown in the panels of Table 7 confirm this interpretation. 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. Note also that this directly addresses the potential concern that our results might simply be due to reversion to the mean in the 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

We now show that our results are robust to several alternative measures of earnings management. The outcomes of these tests are reported in Table 8. 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 8 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, 2011). 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 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 9.[23] We present the results without any fixed effects, and then gradually introduce year, industry, and firm fixed effects. Overall, the coefficients on $COVERAGE$ is very small and approximately an order of magnitude lower than the estimates from our experiment. However, 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

[23]Notice that the estimation sample used in Table 9 is smaller than the sample of the regressions estimated in Table 6. For the sample used in Table 9, every firm-year appears once, whereas, in Table 6, 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.

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., Chen et al., 2013; 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 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 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. This quasi-experiment 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 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 percentage of overlapping coverage retained is also included. This refers to the percentage of overlapping stocks that continue (i.e., in the year following the merger) to be covered by analysts at the bidder and by (retained) analysts at the target.

Brokerage house	IBES identifier	Merger date	Stock coverage		
			#	Overlap	%Overlap retained
Paine Webber	189	12/31/1994	816	171	63.3
Kidder Peabody	150		722		20.2
Morgan Stanley	192	5/31/1997	1,081	180	83.1
Dean Witter Reynolds	232		553		12.4
Smith Barney (Travelers)	254	11/28/1997	1367	254	64.3
Salomon Brothers	242		936		41.4
EVEREN Capital	829	1/9/1998	249	8	71.4
Principal Financial Securities	495		212		0.0
DA Davidson & Co	79	2/17/1998	108	12	52.9
Jensen Securities	932		73		93.8
Dain Rauscher	76	4/6/1998	459	39	29.2
Wessels Arnold & Henderson	280		201		45.3
First Union	282	10/1/1999	417	24	57.5
EVEREN Capital	829		277		17.9
Paine Webber	189	6/12/2000	758	17	62.9
JC Bradford	34		229		0.0
Credit Suisse First Boston	100	10/15/2000	1,359	299	51.5
Donaldson Lufkin and Jenrette	86		1,021		21.5
UBS Warburg Dillon Read	85	12/10/2000	936	165	49.1
Paine Webber	189		730		49.0
JP Morgan	873	12/31/2000	721	80	51.8
Chase Manhattan	125		598		41.9
Fahnestock	98	9/18/2001	161	7	87.5
Josephthal Lyon & Ross	933		121		0.0
Janney Montgomery Scott	142	3/22/2005	165	8	90.0
Parker/Hunter	860		64		10.0

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 financial reporting quality 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	29,656	0.243	0.034	0.086	0.216	0.472
RM_1	1,266	0.114	-0.154	0.102	0.312	0.894	29,656	0.061	-0.239	0.089	0.394	1.195
RM_2	1,266	0.098	-0.129	0.034	0.198	1.021	29,656	0.108	-0.166	0.049	0.260	1.366
RM_{CFO}	1,266	-0.100	-0.209	-0.095	-0.014	0.391	29,656	-0.038	-0.205	-0.074	0.034	0.533
RM_{PROD}	1,266	-0.083	-0.208	-0.045	0.054	0.265	29,656	-0.076	-0.205	-0.050	0.077	0.305
RM_{DISX}	1,266	0.197	0.007	0.130	0.302	0.801	29,656	0.145	-0.078	0.110	0.351	1.056
Panel B: Control variables												
$COVERAGE$	1,264	22.868	14	22	31	11.333	29,316	7.269	2	5	10	7.181
$LNSIZE$	1,264	8.365	7.134	8.327	9.488	1.733	29,316	5.700	4.412	5.608	6.822	1.808
ROA	1,264	0.106	0.067	0.108	0.163	0.133	29,316	0.042	0.021	0.085	0.138	0.219
MTB	1,264	5.036	2.048	3.127	5.697	7.024	29,316	3.391	1.342	2.262	3.941	5.151
$EARN$	1,264	0.778	0.084	0.308	1.012	1.082	29,316	0.119	0.001	0.017	0.072	0.400
$SALESGROWTH$	1,264	0.155	0.044	0.119	0.238	0.228	29,316	0.133	0.025	0.126	0.272	0.321
Panel C: Costs of earnings management variables												
$AUDITORTENURE$	1,264	5.454	3	5	8	2.212	29,632	5.257	3	5	7	2.498
NOA	1,256	0.702	0.530	0.669	0.806	0.402	29,532	0.725	0.494	0.690	0.872	0.496
$ZSCORE$	1,219	5.130	2.354	3.695	6.136	4.581	28,908	4.886	2.286	3.751	6.260	4.816

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 *COVERAGE* is above or below the median and allow the treatment effect to vary between these two groups. All continuous variables are winsorized at the 1% and 99% level. 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	-0.104	-0.022***	-0.022***	-0.024***	-0.024***	0.065***	0.074***
	(-0.392)	(-3.493)	(-3.487)	(-3.741)	(-3.622)	(4.877)	(4.508)
TREATED	15.615***	-0.113***	-0.068***	-0.021***	-0.021	0.134***	0.148***
	(30.628)	(-7.258)	(4.519)	(-2.847)	(-1.501)	(4.548)	(4.008)
$POST \times TREATED$	-0.648***	0.043**	0.043**	0.043**	0.043**	-0.229***	-0.271***
	(-4.488)	(2.184)	(2.179)	(2.166)	(2.101)	(-4.412)	(-4.314)
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	61,822	61,822	61,822	61,822	61,822	61,822	61,822
R-Squared	0.1384	0.001	0.058	0.156	0.541	0.242	0.193

Panel B: Conditional on initial coverage

	AM	RM_1	RM_2
	[1]	[2]	[3]
$POST$	-0.024***	0.063***	0.048***
	(-3.622)	(8.504)	(6.914)
$TREATED$	-0.022	0.067***	0.060***
	(-1.532)	(3.838)	(3.760)
$POST \times TREATED \times 1\{COVERAGE < \text{med.}\}$	0.056**	-0.124***	-0.101***
	(2.048)	(-4.088)	(-3.499)
$POST \times TREATED \times 1\{COVERAGE > \text{med.}\}$	0.028	-0.065	-0.0782**
	(1.176)	(-1.60)	(-1.981)
Merger fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Number of observations	61,822	61,822	61,822
R-Squared	0.545	0.333	0.240

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_{PROD}, $-RM_{CFO}$, and $-RM_{DISX}$ denote measures of real earnings management based on abnormal production costs, cash flows from operations, and discretionary expenses, respectively. All continuous variables are winsorized at the 1% and 99% level. 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 production costs

RM_{PROD}	[1]	[2]	[3]	[4]
$POST$	0.018***	0.018***	0.019***	0.019***
	(7.418)	(7.423)	(7.696)	(7.308)
$TREATED$	-0.0075	-0.012	-0.017	0.005
	(-0.059)	(-1.471)	(-1.538)	(0.947)
$POST \times TREATED$	-0.004	-0.004	-0.005	-0.005
	(-0.667)	(-0.658)	(-0.701)	(-0.711)
Merger fixed effects	No	Yes	Yes	Yes
Industry fixed effects	No	No	Yes	Yes
Firm fixed effects	No	No	No	Yes
Number of observations	61,822	61,822	61,822	61,822
R-Squared	0.001	0.007	0.282	0.194

Panel B: Abnormal cash flows from operations

$-RM_{CFO}$	[1]	[2]	[3]	[4]
$POST$	0.039***	0.039***	0.038***	0.039***
	(6.592)	(6.589)	(6.457)	(6.248)
$TREATED$	-0.063***	-0.063***	-0.047***	0.029**
	(-4.738)	(-4.647)	(-3.387)	(2.174)
$POST \times TREATED$	-0.049***	-0.049***	-0.050***	-0.051**
	(-2.561)	(-2.588)	(2.631)	(-2.538)
Merger fixed effects	No	Yes	Yes	Yes
Industry fixed effects	No	No	Yes	Yes
Firm fixed effects	No	No	No	Yes
Number of observations	61,822	61,822	61,822	61,822
R-Squared	0.002	0.026	0.092	0.245

Panel C: Abnormal discretionary expenses

$-RM_{DISX}$	[1]	[2]	[3]	[4]
$POST$	0.03***	0.035***	0.034***	0.033***
	(2.968)	(2.974)	(2.881)	(2.646)
$TREATED$	0.053**	0.091***	0.139***	0.118***
	(2.061)	(3.452)	(5.172)	(4.291)
$POST \times TREATED$	-0.209***	-0.209***	-0.211***	-0.209***
	(-4.387)	(-4.388)	(-4.418)	(-4.213)
Merger fixed effects	No	Yes	Yes	Yes
Industry fixed effects	No	No	Yes	Yes
Firm fixed effects	No	No	No	Yes
Number of observations	61,822	61,822	61,822	61,822
R-Squared	0.005	0.040	0.211	0.452

Table 5
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. 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). Treated firms are classified according to the cost of accrual manipulation: 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. All continuous variables are winsorized at the 1% and 99% level. 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***	-0.029	0.202***	0.011	-0.040***	-0.001	0.108***
	(-2.029)	(-4.768)	(-1.523)	(7.598)	(1.203)	(4.703)	(-0.038)	(4.018)
$TREATED$	-0.037*	-0.043**	0.031	0.154***	-0.015	-0.072**	0.110**	0.180***
	(-1.694)	(-2.511)	(0.729)	(3.001)	(-0.649)	(-4.838)	(2.238)	(3.486)
$POST \times TREATED$	0.027	0.053*	-0.124	-0.445***	0.002	0.088***	-0.133	-0.345***
	(0.981)	(1.944)	(-1.599)	(-4.804)	(0.064)	(3.663)	(-1.576)	(-3.707)
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	32,000	29,822	32,000	29,822	30,323	31,499	30,323	31,499
R-Squared	0.168	0.162	0.084	0.108	0.185	0.152	0.102	0.122

Table 6
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. All continuous variables are winsorized at the 1% and 99% level. 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]
$POST$	0.033	-0.0155**	0.068***	0.082***
	(1.23)	(-2.37)	(5.10)	(4.90)
$TREATED$	1.999***	-0.0165	0.136***	0.155***
	(11.87)	(-1.20)	(4.57)	(4.17)
$POST \times TREATED$	-0.775***	0.0374*	-0.226***	-0.268***
	(-5.17)	(1.80)	(-4.45)	(-4.35)
$LNSIZE$	1.368***	0.035***	0.038***	0.054***
	(20.56)	(4.78)	(2.51)	(2.70)
ROA	-1.247***	0.054	-0.043	-0.077
	(-5.24)	(1.35)	(-0.48)	(-0.63)
MTB	-0.081***	-0.001	0.003	0.009***
	(-8.35)	(-1.30)	(1.34)	(2.61)
$EARN$	0.001***	-0.000*	0.000***	0.000***
	(4.30)	(1.85)	(4.36)	(4.28)
$COVERAGE$		-0.002*	0.004	0.002
		(-1.717)	(1.53)	(0.76)
Merger fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Number of observations	61,138	61,138	61,138	61,138
R-Squared	0.884	0.358	0.306	0.264

Table 7
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. All continuous variables are winsorized at the 1% and 99% level. If indicated, the regressions include two-digit SIC 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]
$POST$	0.079***	0.135***	0.192***
	(9.138)	(8.857)	(11.218)
$TREATED$	0.023	0.023	-0.027
	(1.293)	(0.637)	(-0.630)
$POST \times TREATED$	-0.031	-0.056	-0.010
	(-1.148)	(-0.976)	(-0.151)
Merger/Industry/Firm FE	Yes	Yes	Yes
Number of observations	60,604	60,604	60,604
R-Squared	0.353	0.267	0.213

Panel B: Event date shifted one year backward

	AM	RM_1	RM_2
	[1]	[2]	[3]
$POST$	0.057***	0.006	-0.059***
	(12.106)	(0.524)	(-4.451)
$TREATED$	0.002	-0.063*	-0.051
	(0.288)	(-1.739)	(-1.139)
$POST \times TREATED$	0.004	0.043	0.023
	(0.278)	(0.887)	(0.396)
Merger/Industry/Firm FE	Yes	Yes	Yes
Number of observations	54,522	54,522	54,522
R-Squared	0.348	0.252	0.159

Table 8
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 two-digit SIC 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*	0.042	0.009	-0.021***	-0.018***	-0.020***
	(1.947)	(1.486)	(0.279)	(-3.418)	(-3.271)	(-3.324)
$TREATED$	-0.039**	0.1416***	0.1286***	-0.018	-0.021*	-0.017
	(-2.073)	(3.762)	(3.039)	(-1.621)	(-1.714)	(-1.598)
$POST \times TREATED$	0.057**	-0.230***	-0.266***	0.010***	0.011***	0.011***
	(2.014)	(-3.448)	(-3.331)	(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	61,466	61,466	61,466	61,822	61,822	61,822
R-Squared	0.200	0.181	0.156	0.194	0.171	0.110

Table 9
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., all of which are winterized at the 1 and 99% levels. If indicated, the regressions include two-digit SIC 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]
$COVERAGE$	-0.002	-0.004**	-0.008***	0.004***	-0.003	-0.006***	0.003**	-0.002	-0.005***	0.002	0.006	0.002
	(-1.403)	(-2.291)	(-4.215)	(3.391)	(-1.616)	(-3.434)	(2.434)	(-0.880)	(-2.802)	(0.938)	(1.482)	(0.621)
$LNSIZE$	0.057***	0.013	0.007	-0.006	0.004	0.001	-0.012**	0.006	0.001	0.025**	-0.011	-0.010
	(8.907)	(1.478)	(0.893)	(-1.122)	(0.397)	(0.111)	(-2.131)	(0.637)	(0.139)	(2.038)	(-0.570)	(-0.512)
ROA	-0.571***	0.255***	-0.125**	-0.361***	0.251***	-0.179***	-0.136***	0.277***	-0.081	0.352***	-0.045	-0.149
	(-14.804)	(3.771)	(-2.062)	(-9.812)	(3.721)	(-3.001)	(-3.742)	(4.089)	(-1.348)	(4.240)	(-0.371)	(-1.143)
MTB	0.010***	-0.015***	0.001	0.014***	-0.019***	-0.005*	0.006***	-0.023***	-0.012***	-0.003	-0.003	-0.001
	(4.371)	(-5.176)	(0.269)	(6.947)	(-6.396)	(-1.652)	(3.021)	(-7.673)	(-4.053)	(-1.268)	(-0.832)	(-0.274)
$EARN$	-0.000***	0.000	0.000***	-0.000***	0.000	0.000***	-0.000***	0.000*	0.000***	-0.000*	0.000***	0.000***
	(-5.327)	(0.973)	(2.779)	(-4.281)	(1.182)	(2.965)	(-2.925)	(1.725)	(4.004)	(-1.669)	(3.370)	(3.437)
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	0.021	0.005	0.003	0.184	0.051	0.076	0.247	0.077	0.108	0.501	0.375	0.312

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
$EARN$	Earnings before extraordinary items
$SALESGROWTH$	One year growth in company's sales

Panel C: Costs of earnings management variables	
Variable	Definition
$AUDITORTENURE$	Number of years a company has retained the same auditor
NOA	Net operating assets computed from cash flow statement following Barton and Simko (2002)
$ZSCORE$	Measure of financial health computed following Altman (1968)