Equity Incentives and Earnings Management: Evidence from the Adoption of an Anti-Hedging Policy

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ABSTRACT: Equity incentives can motivate managers to manage earnings, but the ability to hedge equity incentive portfolios through derivatives may alter this behavior. This study examines whether and how earnings management changes after firms adopt policies prohibiting such hedging. We find that anti-hedging policies increase income smoothing and raise the likelihood of meeting or just beating analysts' earnings forecasts, particularly in firms where managers previously engaged in derivative transactions. Consistent with this mechanism, we find a decline in derivative transactions following policy adoption, with the policy's effect on earnings management more pronounced in these firms. We also find that firms with smoother reported earnings following policy adoption experience a decline in investors' perceived risk. In contrast, we observe no significant changes in corporate risk-taking, CEO wealth exposure, or CEO equity sales following policy adoption. Overall, our findings suggest that anti-hedging policies may unintentionally encourage earnings management, thereby partially attenuating their intended effect of better aligning managerial and shareholder interests.

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1. Introduction

When direct monitoring mechanisms are imperfect, performance-based compensation—such as equity incentives—plays a central role in aligning the interests of managers and shareholders (Jensen and Meckling 1976). Equity incentives not only encourage managers to focus on shareholder value but also expose risk-averse managers with undiversified human capital and wealth to firm-specific risk. To protect the value of their equity portfolios, managers may be motivated to manage earnings. However, empirical evidence on the relation between equity incentives and earnings management remains mixed (e.g., Armstrong et al. 2013; Mayberry et al. 2021; Quinn 2018; Nienhaus 2022). One possible explanation for these conflicting findings is that prior studies have not explicitly accounted for how this relation might vary depending on managers' ability to hedge their equity portfolios through personal derivative transactions. This study examines whether and how earnings management changes when firms adopt anti-hedging policies that restrict such transactions.[1]

The adoption of anti-hedging policies offers a novel lens to examine the relation between equity incentives and earnings management. While direct monitoring mechanisms mitigate both incentive-misalignment and earnings management by reducing the information asymmetry between managers and shareholders (e.g., Klein 2002; Ayers et al. 2011), anti-hedging policies operate through a different governance channel—by preventing managers from transforming the payoff and risk of their equity portfolios at their discretion and with reduced transparency (Institutional Shareholder Services [ISS] 2012; U.S. Securities and Exchange Commission [SEC]

[1] Appendix A presents several examples of anti-hedging policies disclosed in proxy statements. These policies are generally defined as corporate rules that prohibit named executive officers, directors, or employees from engaging in transactions that are designed to hedge or offset any decline in the market value of the firm's equity securities. While the examples list specific prohibited transactions, the definitions of these policies generally provide flexibility to address emerging structures designed for downside price protection (SEC 2018).

2015). However, the policies may unintentionally encourage managers to manage earnings to mitigate the firm-specific risk of their unhedged equity portfolios.

We predict that anti-hedging policies will increase income smoothing and the likelihood of meeting or just beating earnings thresholds. Managers tend to smooth earnings on the belief that smoother earnings can affect investors' perceptions of the risk and persistence of firm performance, thereby inducing favorable valuation (Beidleman 1973; Barth et al. 1999; Graham et al. 2005; Francis et al. 2004; Myers et al. 2007; Benmelech et al. 2010; de Jong et al. 2014). Moreover, they tend to use income-increasing discretionary accruals to avoid stock price declines resulting from missing earnings benchmarks (e.g., Burgstahler and Dichev 1997; Skinner and Sloan 2002; Graham et al. 2005; Dichev et al. 2013). In response to anti-hedging policies, risk-averse managers with reduced ability to diversify their equity portfolios will likely consider the maintenance of a smooth earnings growth path by smoothing earnings or meeting or just beating consensus analyst forecasts as an alternative means of managing the risk of their equity portfolios.[2]

We identify a firm's initiation of an anti-hedging policy by searching for its proxy statements from fiscal years 2005 to 2015. Item 402(b) of Regulation S-K has required firms to disclose all material contents of executive compensation, including an anti-hedging policy, since 2006. For instance, if a firm initiated this policy during or before 2006, the firm must disclose it in the 2006 proxy statements to comply with the mandatory disclosure rule under Item 402(b). About 60% of Standard & Poor's 1500 firms (excluding financial and utility firms) had adopted this policy as of 2015, and therefore, extending our sample period beyond 2015 poses significant challenges in locating suitable control firms with common support (Rosenbaum and Rubin 1983).

[2] We also consider that these anticipated effects may not hold if a firm adopts the policy to appease its outside parties without intending to make real consequences, managers reduce their firm-specific risk exposures (Edmans et al. 2017) or risk-taking activities (Coles et al. 2006) or use firm-level hedging instruments (Barton 2001; Choi et al. 2015) without resorting to earnings management. We address these tensions in sections 4.2.2 and 4.4.2.

Our analysis begins by finding that firms are more inclined to adopt the policy under several conditions—when firms' options are more actively traded in the stock exchanges (i.e., lower hedging transaction cost), when they possess more effective monitoring mechanisms for shareholder-manager incentive alignment, when CEO equity ownership is higher, when CEO stock trades occur more frequently, and when a higher proportion of industry peers have already adopted the policy (indicating greater peer pressure to conform to common practices). To focus our analysis, we narrow our sample to firms that grant equity to their CEOs, as they stand to benefit the most from improving the efficacy and transparency of these incentives.

To mitigate selection bias related to the functional form of covariates, we construct a matched sample using both propensity-score matching and entropy balancing to ensure covariate balance between treated and control firms (Rosenbaum and Rubin 1983; Hainmueller 2012). This matched sample is central to our cross-sectional tests that divide both treated and control firms based on whether their managers engaged in derivative transactions before (pseudo) adoption years and whether such transactions declined afterward. To address potential selection bias associated with unobservable factors, we estimate Heckman (1976) two-stage least squares (2SLS) regressions over the matched sample, with the start-of-period percentage of adopters in the same industry as an instrumental variable (Heckman 1976). As a robustness check, we also run the Heckman 2SLS regressions over the entropy-balanced unmatched sample.

Our difference-in-differences analysis provides evidence that the adoption of anti-hedging policies influences earnings management. First, income smoothing and the likelihood of meeting or just beating consensus analyst forecasts generally increase after policy adoption.[3] These effects are robust to parallel trends and falsification tests over the matched sample, as well as to the

[3] In contrast, our untabulated results indicate that accounting irregularities—specifically earnings management beyond the limits of accounting standards—do not become more prevalent following the adoption of anti-hedging policies.

Heckman (1976) 2SLS regressions over the unmatched sample. Second, firms whose managers previously engaged in derivative transactions exhibit greater earnings management following policy adoption. For most of these firms, the frequency of such transactions declines after policy adoption, and the effect of anti-hedging policies on earnings management is more pronounced when this decline occurs. Third, we find a significant decline in investors' perceived risk for firms that increase income smoothing following policy adoption. Finally, we find no significant changes in corporate risk-taking, CEO firm-specific wealth, or CEO equity sales after policy adoption. Overall, our findings suggest that managers substitute toward income smoothing and meeting or just beating analyst earnings forecasts as an alternative means to manage the risk of their equity incentives, especially when anti-hedging policies are meaningfully enforced and not merely window-dressing.

Our study contributes to the related literature in several ways. First, it extends the literature on the relationship between equity incentives and earnings management. Previous studies have reported that CEOs with equity portfolio delta smooth income to signal their beliefs on future performance (Shu and Thomas 2019), while managers with equity portfolio vega smooth income to obfuscate earnings fluctuations resulting from excessive risk-taking induced by vega (Grant et al. 2009; Shu and Thomas 2019). While numerous studies have explored the connection between equity portfolio delta and discretionary accruals, their evidence remains inconclusive (e.g., Armstrong et al. 2013; Mayberry et al. 2021). Our research pioneers in demonstrating that income smoothing and meeting or just beating consensus analysts' forecasts increase after the adoption of anti-hedging policies. Our findings suggest that managers respond to these policies by relying on earnings management as an alternative risk management strategy for their equity portfolios.

Second, our findings further enrich the literature on managerial compensation and hedging. Prior studies have documented the characteristics of optimal compensation contracts or corporate social responsibility in the presence of hedging opportunities (Gao 2010; Dye and Sridhar 2016; Hung et al. 2019; Park et al. 2023). However, they do not provide direct evidence that hedging transactions are related to changes in earnings management (Bettis et al. 2015). Our study fills the gap in the literature by finding that, when firms attempt to enhance the efficacy and transparency of equity incentives through anti-hedging policies, managers engage in earnings management as a countervailing measure, especially in firms that experienced decreases in managerial derivative transactions after adopting these policies.

Overall, our findings inform corporate stakeholders and regulators of an unintended consequence of equity incentives with anti-hedging policies. While corporate boards intend to discourage managers from taking excessive risks by hedging the risk of equity incentives, they may prompt earnings management without a significant reduction in risk-taking. Thus, boards should monitor these unintended consequences and adopt complementary safeguards if necessary. Activist shareholders tend to regard anti-hedging policies as the best practice (ISS 2012), and the SEC has mandated that U.S.- listed companies disclose whether they permit or prohibit managerial compensation hedging (SEC 2018). Our findings suggest that they should also consider how the policy is enforced and how it affects earnings quality.

2. Background, Literature, and Hypothesis

2.1. Background and Literature

2.1.1. Equity incentives and earnings management

Shareholders and boards tend to use equity incentives to align the interests of managers and shareholders (e.g., Jensen and Meckling 1976). However, given the well-established link between earnings and stock prices (e.g., Ball and Brown 1968), equity incentives may incentivize managers to manipulate earnings to affect stock prices (Stein 1989; Cheng and Warfield 2005).

Prior studies have reported mixed evidence on the relationship between equity incentives and earnings management.[4] For example, Armstrong et al. (2013) document that financial misreporting may enhance investors' expectations of future cash flows but also increase the likelihood of future stock price crashes when the misreporting is later restated and subjected to SEC enforcement actions, resulting in the countervailing effects on equity portfolio values. Armstrong et al. find a positive association between equity portfolio vega and misreporting, suggesting that a higher vega offers managers increased compensation, even in situations where subsequent restatements lead to stock price crashes. In contrast, Mayberry et al. (2021) re-examine the results of Armstrong et al. (2013) to find that their results are not observed in the post-Dodd-Frank Act (SEC 2010) period and are also sensitive to omitted correlated variables. Jayaraman and Milbourn (2015) find a positive association between equity incentives and misreporting for firms with low auditor expertise. Quinn (2018) finds that meeting or just beating analysts' forecasts occurs less frequently following the adoption of stock ownership plans. Nienhaus (2022) finds evidence that a reduction in executive option pay after the enforcement of SFAS 123R reduces meeting or just beating analysts' earnings forecasts.

[4] One line of earlier studies reports a positive association between equity incentives and earnings management (e.g., Baker et al. 2003; Bartov and Mohanram 2004; Cheng and Warfield 2005; Bergstresser and Philippon 2006; Burns and Kedia 2006; Efendi et al. 2007; Cheng and Farber 2008; Cornett et al. 2008). Another line of earlier studies, such as Erickson et al. (2006) and O'Connor et al. (2006), do not find a significant association between equity incentives and earnings management. In addition, the results of earlier studies are generally not free from reverse causality that managers who successfully manage earnings might have higher equity incentives (e.g., Sloan 1993; Baber er al. 1998; Baker et al. 2003; Ashley et al. 2004). Armstrong et al. (2010) employ propensity-score matching to find a largely insignificant association between equity incentives and accounting irregularity.

There is comparatively little research on the relationship between equity incentives and income smoothing. Income smoothing refers to the degree to which a stream of earnings has a smooth pattern. One line of research reports that managers smooth earnings to dampen the genuine volatility of earnings performance to preserve personal benefits (Trueman and Titman 1988; Leuz et al. 2003; Grant et al. 2009; Lang et al. 2012; Chen et al. 2017). Another line of research argues that managers smooth earnings to communicate their beliefs on future earnings to external investors to help these investors better predict future performance (Ronen and Sadan 1981; Suh 1990; Subramanyam 1996; DeFond and Park 1997; Sankar and Subramanyam 2001; Louis and Robinson 2005; Tucker and Zarowin 2006; Shu and Thomas 2019). Grant et al. (2009) find a positive association between risk-taking incentives and income smoothing, suggesting that managers use income smoothing to moderate earnings volatility arising from the risk-taking incentives. Shu and Thomas (2019) find a positive (negative) association between equity portfolio delta (vega) and price informativeness in the presence of income smoothing. They argue that equity portfolio delta enhances incentive alignment and thus induces informative earnings smoothing. In contrast, equity portfolio vega encourages managers to pursue more risky projects and, in turn, to smooth earnings to counterbalance the impact of such risk-taking on earnings volatility.

2.1.2. Hedging of equity portfolios

While general shareholders can presumably diversify their portfolios on average, managers tend to have firm-specific human capital and wealth that are difficult to diversify. The extensive use of equity incentives aggravates the concentration of managerial wealth in firm-specific assets, thus motivating managers to hedge their equity portfolios (Bettis et al. 2001; Jin 2002; Hall and Murphy 2002; Garvey and Milbourn 2003; Jagolinzer et al. 2007; Gao 2010; Dye and Sridhar 2016; Hung et al. 2019; Park et al. 2023). Hedging transactions based on derivative instruments

enable managers to maintain their equity ownership and protect their compensation from stock price declines even where the company fails to perform (SEC 2015). While managers can manage firm risk to protect their undiversified portfolios inside the firm, it is generally easier for them to hedge their shares outside the firm (Dunham and Washer 2012).

To strengthen the transparency and incentive-alignment effect of equity incentives, some companies adopt policies that prevent managers from hedging equity portfolios. Notably, the majority of anti-hedging policies in proxy statements prohibit executive officers from trading call options or warrants, as well as put options.[5] The disclosure on the corporate website also states that violations of the anti-hedging policy will be considered a severe disciplinary matter and may result in personnel actions, including termination of employment.

Sections 16(a) and 23(a) of the Securities Exchange Act of 1934 require insiders to report in Form 4 their derivative transactions within two business days. However, Form 4 filings provide incomplete information on managerial hedging transactions. Among officers' derivative transactions not classified into Rule 16b-3 transactions (i.e., equity incentive plans) and other Section 16(b) exempt transactions in the 2004–2015 sample period, 99.56% are option trades and equity-linked warrants. Only 0.44% fall within the common types of private contracts through investment banks (i.e., collars, prepaid variable forward contracts, equity swaps, and exchange funds) examined by prior research (Bettis et al. 2001; Jagolinzer et al. 2007; Gao 2010; Bettis et al. 2015). However, we cannot preclude the possibility that managers may be reluctant to reveal detailed information on their hedging transactions and be evasive in their reporting, or the

[5] For example, to illustrate the types of financial instruments typically prohibited and the associated enforcement mechanisms, Cisco's anti-hedging policy in the company's proxy statements and websites (exhibited in Appendix A) states that executive officers are prohibited from engaging in any speculative transactions in Cisco securities, including short sales, transactions involving put options, call options, or other derivative securities, or any other forms of private hedging transactions, including collars, and forward sale contracts, and they are discouraged from pledging Cisco securities in margin accounts or as collateral for loans.

enforcement of Form 4 filing rules may be relatively lax (Bisin et al. 2008; Dunham and Washer 2012).

Securities regulations mandate that firms must disclose their policies regarding the permission or prohibition of managerial hedging transactions. This transparency is intended to assist investors in assessing the effectiveness of a firm's equity incentives in aligning the interests of shareholders and managers. Item 402(b) of Regulation S-K requires the Compensation Disclosure & Analysis to explain all material information about a firm's compensation, including the firm's policy regarding whether to permit named executive officers to hedge any decrease in the market value of equity granted as compensation (SEC 2006). Section 955 of the DFA 2010 extends such disclosure to employees and non-executive directors (U.S. House of Representatives 2010). The SEC proposed rules to implement the DFA disclosure requirement in February 2015 (SEC 2015) and adopted the final rules in December 2018 (SEC 2018). Notably, the anti-hedging policy disclosures in proxy statements often refrain from specifying disciplinary actions that would be taken in the event of a violation. This omission may be due to the sensitivity of discussing such measures in public proxy statements. However, as shown in the Cisco case (see Appendix A), the disclosures on corporate websites discuss disciplinary actions or enforcement mechanisms.

Anti-hedging policies differ from insider trading restrictions. Insider trading restrictions typically prohibit short-selling of shares, forfeiture of trading profits from round-trip transactions (e.g., a buy followed by a sale) made within six months of each other, and equity trades based on material non-public information under Section 10(b) of the Securities Exchange Act of 1934 (Roulstone 2003). However, these restrictions do not directly prevent managers from hedging the economic risks of their equity incentives through derivative transactions as long as these transactions do not exploit material non-public information. In contrast, as illustrated by Cisco's

case in Appendix A, the policy is applicable, regardless of whether the employee has material, non-public information.

2.2. Hypothesis Development

The adoption of an anti-hedging policy provides an intriguing context for exploring the relationship between equity incentives and earnings management. While an anti-hedging policy is intended to enhance the incentive-alignment efficacy of equity incentives (ISS 2012), it may also present challenges for risk-averse managers with undiversified human capital and wealth to manage the risk of incentive equity portfolios through derivative transactions. We investigate whether the adoption of an anti-hedging policy will induce these managers to manage earnings in order to influence investors' perceptions of firm performance risk and persistence.

Our first hypothesis pertains to the effect of an anti-hedging policy on income smoothing. Prior studies suggest that managers tend to believe that smoother earnings can positively influence investors' perception of firm performance risk and persistence, leading to more favorable stock valuation (Beidleman 1973; Barth et al. 1999; Graham et al. 2005; Francis et al. 2004; Myers et al. 2007; Benmelech et al. 2010; Jong et al. 2014). When pre-managed earnings fall below a stable growth path, income smoothing requires income-increasing discretionary accruals to avoid reported earnings deviating from a desirable earnings path. Conversely, when pre-managed earnings exceed the desired growth path, income smoothing requires income-decreasing discretionary accruals to maintain a stable earnings growth path. Therefore, risk-averse managers with limited ability to diversify their wealth are likely to opt for maintaining a smooth earnings growth path through income smoothing to mitigate the increased risk in their stock-based compensation imposed by an anti-hedging policy.

Our second hypothesis pertains to the effect of an anti-hedging policy on the propensity of meeting or just beating earnings thresholds. Prior studies find evidence that failing to satisfy the capital market's expectation of firm performance results in significant declines in stock prices, and, hence, the use of discretionary accruals to meet or just beat consensus analyst earnings forecasts enables managers to avoid the negative valuation consequences from missing the earnings thresholds (e.g., Burgstahler and Dichev 1997; Skinner and Sloan 2002; Graham et al. 2005). Therefore, managers affected by anti-hedging restrictions are more likely to be motivated to engage in such a type of earnings management.

Both income smoothing and meeting or just beating analyst earnings forecasts fall within or beyond the boundary of legitimate discretion allowed by accounting standards. Thus, in the context of anti-hedging policy adoption, these forms of earnings management may emerge as accessible tools for risk-averse managers to protect the value of their equity portfolios. Taken together, we present our hypotheses in alternative forms:

H1: *Ceteris paribus, the adoption of an anti-hedging policy increases income smoothing.*

H2: *Ceteris paribus, the adoption of an anti-hedging policy increases the propensity of meeting or just beating consensus analyst earnings forecasts.*

Nevertheless, the hypothesized effects may be subject to certain contingencies, such as firms' intentions to adopt anti-hedging policies, the extent of their enforcement, and whether their managers pursue alternative countervailing actions in a substitutionary or complementary way. We empirically investigate these boundary conditions and alternative responses to anti-hedging policies in cross-sectional tests (Section 4.2.2) and additional analyses (Section 4.4.2).

3. Sample and Research Design

3.1. Identification of the Adoption of an Anti-Hedging Policy

Item 402(b) of Regulation S-K requires companies to disclose any material information on executive compensation, including a policy to permit or prohibit managerial hedging since 2006 (SEC 2006). Thus, for S&P 1500 firms covered by the Execucomp database, we search DEF 14A proxy statements to identify disclosures indicating that a firm prevents its named executive officers from engaging in all transactions that hedge equity incentives (see Appendix for examples). Specifically, we search for all sentences containing keywords, such as 'hedg-', 'derivative', and 'option', and carefully read these sentences to identify whether the firm adopted an anti-hedging policy for executive compensation in the fiscal year. While such policies may list examples of prohibited transactions, their scope typically extends beyond these examples. Permitting hedging transactions with board approval is not classified as anti-hedging.

Because of the mandatory nature of Item 402(b) disclosures, if a firm initiated an anti-hedging policy during or before 2006, the firm must have disclosed the policy in the 2006 proxy statements; otherwise, any negligence would violate the regulation. We determine each firm's adoption year by identifying the earliest DEF 14A filing that describes its anti-hedging policy. Although some firms may not adhere to this regulation, it remains reasonable to use Item 402(b) mandatory disclosure to identify the fiscal year in which a firm has initiated the policy. If the majority of sample firms were to adopt the policy long before starting such disclosure, our difference-in-differences analysis could possibly result in insignificant policy effects.

Table 1 presents the distribution of anti-hedging policy adoption. Panel A shows a steady year-on-year increase in adopters during our sample period, rising from 2 firms in 2004 to 1,153 in 2015. We focus on the years 2004–2015 for several reasons. In 2015, the SEC proposed rules

to implement DFA Section 955, which were finalized in 2018. By 2015, approximately 1,153 out of 1,941 non-financial and non-utility firms in the Execucomp database had adopted anti-hedging policies. Extending the sample period beyond 2015 would limit the availability of valid control firms with common support (Rosenbaum and Rubin 1983).[6] Moreover, the 2018 SEC rule cannot serve as a clean external shock to anti-hedging policy adoption, as most S&P 1500 firms had adopted policies before 2018, leaving post-2018 adopters subject to self-selection. Panel B reveals that most adopters operate in manufacturing, retail trade, services, transportation and communications, and mining. We exclude financial and utility firms due to their unique reporting practices and regulatory environments.

3.2. Sample Selection

We test whether changes in earnings management after anti-hedging policy adoption reflect genuine behavioral changes or window dressing. To do so, we identify whether the managers of a treated firm and its matched control firm did or did not engage in derivative transactions prior to their actual and pseudo adoption year, respectively, assuming that these firms have different ex-ante demand for regulating managerial hedging.

We employ a two-step matching procedure to ensure comparability between treated and control groups and minimize bias from observable covariates, thereby enhancing the validity of our causal inferences. First, we use propensity score matching (PSM) to pair each adopter with a non-adopter in the same year and industry that has a similar likelihood of adopting an anti-hedging policy (Rosenbaum and Rubin, 1983, 1985). PSM creates a balanced matched sample with regard to propensity scores, narrowing the control group to a comparable subset. However, because PSM does not guarantee covariate balance across all dimensions, we further apply entropy balancing to

[6] The common support condition is crucial in propensity score matching because there should be overlap in the distribution of propensity scores between the treated and control groups.

the propensity-score matched sample. This method reweights the control group so that the means and higher moments of all covariates exactly match those of the treated group (Hainmueller, 2012; McMullin and Schonberger, 2020).

We begin by estimating the following probit regression model to estimate a propensity score for each firm-year:

$$Adoption_{PSM,i,t} = \alpha_0 + \alpha_i X_{i,t-1} + \alpha_j PeerAdopt_{i,t-1} + \varepsilon_{i,t} \quad (1),$$

where $Adoption_{PSM,i,t}$ is the indicator variable for the first year in which a firm has an anti-hedging policy in place. See Appendix B for definitions of all variables used in this study. For firms that adopted the policy in our sample period, $Adoption_{PSM,i,t}$ equals one for the first year of adoption, and the other years are excluded. For firms that never adopt it during the sample period, $Adoption_{PSM,i,t}$ equals zero for all years. An instrumental variable is the percentage of a company's peers that had adopted the policy by the end of year *t*-1 (*PeerAdopt*). Peer pressure, akin to the bandwagon effect, may influence adoption decisions, but it is unlikely to directly affect earnings management. We assume that the determinants of adoption, $X_{i,t-1}$, capture managers' incentives and opportunities for hedging. Similar to Gao (2010), we control market-to-book ratio, firm size, book leverage, the existence and trading volume of a firm's publicly traded stock options available for hedging, CEO net equity trades, and CEO ownership. Beyond them, we control for stock returns, stock return volatility, CEO characteristics, and corporate governance mechanisms (i.e., CEO tenure, CEO duality, institutional ownership, analyst coverage, and board independence). The propensity score for each firm-year observation is the predicted value of $Adoption_{PSM,t}$.

Table 2 presents the propensity-score matched sample selection procedures. Panel A summarizes the procedures. Panel B shows the results of estimating Equation 1 over 5,072

observations (843 adopters and 697 non-adopters). The negative coefficient on CEO equity ownership (*CEOOwn*) suggests that CEOs with stronger equity incentives are less willing to accept such policies. The positive coefficient on the trading volume of firm stock options (*SumVolume*) suggests that policy adoption occurs more frequently in firms with higher liquidity of stock options available for hedging. The positive coefficients on observable governance mechanisms (i.e., board independence, analyst coverage, and institutional ownership) imply that firms adopt anti-hedging policies to strengthen the effectiveness and transparency of equity incentives. The positive coefficient on the percentage of peer-firm adopters (*PeerAdopt*) highlights the role of peer pressure. Finally, anti-hedging policy adoption is positively related to firm size (*FirmSize*) and firm performance (*ROA* and *CAR*).

Next, we match each adopter to a non-adopter in the same year and industry with the closest score, within a cut-off of 0.25 × standard deviation of the propensity score without replacement (Rosenbaum and Rubin 1985).[7] After removing matched firms identified in the initial search, we repeat this matching procedure until no additional matched pairs can be formed within the cut-off. This process assigns a pseudo-adoption year to a non-adopter, even though it never adopts an anti-hedging policy during the sample period. We obtain 357 adopter-nonadopter pairs in total.

Panel C shows that the 357 adopter-non-adopter pairs have essentially indistinguishable means of propensity scores and covariates in the adoption years. Columns 1 through 3 present statistically significant differences of a few variables prior to entropy balancing, though their magnitudes appear economically insignificant. For example, the mean percentage of independent directors for adopters (69.4%) is 2.9 percentage points lower than that of non-adopters (72.3%). Given that both groups maintain high levels of board independence, this difference is unlikely to

[7] If multiple adopters are matched to a single adopter in each iteration, we prioritize selecting the earliest match with the lowest propensity score distance.

yield qualitatively different independence under the majority voting rules. A relatively large mean difference in firm size, however, highlights the need for entropy balancing to achieve precise covariate balance between the treated and control groups. To further improve covariate comparability, we reweight the matched control firms so that the mean and standard deviations of all covariates exactly match those of the treated firms. Columns 4 through 6 show that, after entropy balancing, the treated and control groups exhibit no statistically significant differences in the means of any covariates, indicating that the groups are well balanced.

We merge the 357 matched adopter-non-adopter pairs with variables for the second-stage regressions. We define the pre-adoption period as *T*-3 to *T*-1 and the post-adoption period as *T* to *T*+3, where *T* denotes a firm's adoption year. This window allows sufficient time for an anti-hedging policy to influence earnings management. We require all observations to have positive stock and option grants, ensuring that firms need to enhance the incentive-alignment effectiveness and transparency of equity incentives. To enable within-firm variations around adoption years, we require each firm to have at least one non-missing observation for both pre- and post-adoption periods. These requirements result in 2,516 firm-year observations, comprising 1,378 firm-years for 256 adopters and 1,138 firm-years for 228 non-adopters.

3.3. Regression Models for Testing Hypotheses

To test our hypotheses regarding the effects of an anti-hedging policy on earnings management, we estimate the following regression model:

$$EM_{i,t} = \beta_1 AHP_i + \beta_2 Post_{i,t} + \beta_3 AHPPost_{i,t} + \beta_j X_{i,t-1} + \beta_k IMR_{i,t} + Firm\ FE + Year\ FE + \varepsilon_{i,t} \quad (2),$$

where $EM_{i,t}$ is an earnings management measure, such as income smoothing and meeting or just beating consensus analyst forecasts. Our difference-in-differences analysis employs the following indicator variables: (1) AHP_i equals one for all observations of an adopter, (2) $Post_{i,t}$ equals one

for the post-adoption period for an adopter and the pseudo-post-adoption period for its matched control firm, and (3) $AHPPost_{i,t}$ equals one for the post-adoption period for an adopter and zero otherwise. The coefficient on AHP_i reflects the average difference in earnings management between adopters and non-adopters before the adoption year.[8] The coefficient on $Post_{i,t}$ captures the average time path of earnings management after pseudo-adoption years.[9] The coefficient on $AHPPost_{i,t}$ reflects the average effect of anti-hedging policy adoption on earnings management.

The principal component ($Smooth_{PCA,i,t}$) reflects the common information across the following three individual income-smoothing measures. The first measure is –1 × the correlation coefficient between changes in discretionary accruals and changes in earnings before discretionary accruals for the year ($Smooth_{TZ,\ i,t}$) (Tucker and Zarowin 2006). A more negative correlation coefficient reveals a higher level of earnings smoothing through shifting accruals from one period to the next to dampen fluctuations in earnings before discretionary accruals. However, this measure is subject to errors in estimating discretionary accruals. The second measure is –1 × the ratio of the standard deviation of operating income to the standard deviation of operating cash flows ($Smooth_{LNW,\ i,t}$) (Leuz et al. 2003). This measure does not distinguish between discretionary and non-discretionary income smoothing. The third measure is the standard deviation of pseudo-fiscal-year earnings minus the standard deviation of reported-fiscal-year earnings ($Smooth_{BPT,\ i,t}$) (Black et al. 2022). A pseudo-fiscal year consists of the first quarter of the current year and the last three quarters of the previous year. Reported-fiscal-year earnings will be less volatile than pseudo-fiscal-year earnings when managers use accruals to reduce the volatility of reported-fiscal-year earnings.

[8] Controlling firm-fixed effects in the Stata package automatically drops AHP_i. Thus, Table 3 does not present the coefficients on AHP_i to avoid perfect collinearity.

[9] Although we include year-fixed effects in our main regressions, $Post_{i,t}$ is not absorbed because each treated firm may have different adoption years (i.e., staggered adoption), and we assign a pseudo adoption year to its matched control firm when constructing the matched sample. As a result, $Post_{i,t}$ does not represent a common time dummy across all firms but rather indicates the post-treatment period relative to each firm's (actual or pseudo) adoption year. Therefore, $Post_{i,t}$ varies across firms and years in a way that is not collinear with the full set of year fixed effects.

The indicator variable $MorB_{i,t}$ captures firm-years in which reported annual earnings meet or just beat consensus analysts' annual earnings forecasts by two cents. We estimate a linear probability model that enables us to control for firm-fixed effects.[10]

Equation 2 includes covariates to mitigate the omitted correlated variable problem. It controls the determinants of anti-hedging policy adoption, $X_{i,t\text{-}1}$. We add the inverse Mills ratio (*IMR*) to control the self-selection bias (Heckman 1976).[11] The instrumental variable ($PeerAdopt_{i,t\text{-}1}$) is excluded from Equation 2 under the exclusion restriction assumption. We also include the natural logarithm of equity portfolio delta ($LogDelta_{i,t\text{-}1}$) and vega ($LogVega_{i,t\text{-}1}$) to control potential relations between these variables and subsequent earnings management, as found by prior studies. We control for firm- and year-fixed effects (*Firm FE* and *Year FE*).[12] *T*-statistics are based on standard errors clustered by firm. Finally, we winsorize continuous variables at the 1st and 99th percentiles to mitigate the influence of extreme outliers.

4. Results of Empirical Analysis

4.1. Descriptive Statistics

Panel D of Table 2 reports summary statistics for the regression variables. The mean of *MorB* is 19.7%, suggesting that discretionary accruals are frequently used to meet or just beat consensus analyst forecasts during the sample period. The median of each income smoothing measure is close to zero. The logarithm of equity portfolio delta (*LogDelta*) is positive for all firms, with a median of 5.195, corresponding to a median delta of $180.4 thousand. Given the median of cumulative market-adjusted daily stock return (*CAR*) of 6.6%, this implies an increase in incentive

[10] The drawback of a linear probability model is that the estimated coefficients may imply probabilities below zero or above one. The logit regression generates qualitatively similar results (untabulated).
[11] Our results remain robust even when excluding *IMR* from Equation (2).
[12] Substituting firm-fixed effects with industry-fixed effects yields results consistent with our main findings.

portfolio value of roughly $1.2 million (180.4×6.6≈1,190), suggesting that CEOs in our sample have strong incentives to hedge their equity portfolios against adverse stock price movements.

4.2. Baseline Results

4.2.1. Average effects of an anti-hedging policy on earnings management

Our hypotheses predict that when risk-averse managers with highly concentrated wealth and income are prevented from hedging their equity portfolios against adverse stock price fluctuations, they will rely more on earnings management. Such practices aim to shape investors' perception of earnings riskiness and persistence, mitigating firm-specific equity incentive risk.

Table 3 presents the average effects of anti-hedging policy adoption on earnings management over the matched sample. Columns 1 through 4 present the effects on income smoothing, while column 5 presents the effects on meeting or just beating consensus analyst forecasts by two cents.[13] The inconsistent statistical significance of the coefficients on $Post_{i,t}$ across columns indicates inconclusive changes in earnings management for non-adopters after pseudo-adoption years. In Columns 1 through 3, the coefficients on $AHPPost_{i,t}$ are significantly positive in two-tailed tests, lending support to Hypothesis H1 that managers rely more on income smoothing after anti-hedging policy adoption.[14] In Column 5, the coefficient on $AHPPost_{i,t}$ is significantly positive, indicating that reported earnings are more likely to meet or just beat consensus analyst earnings forecasts after policy adoption, supporting Hypothesis H2.

Overall, the results in Table 3 suggest that, as policy adoption makes direct hedging infeasible and thus increases managerial compensation risk, managers counteract this through income smoothing and by meeting or just beating consensus analyst forecasts as alternative means

[13] As mentioned in Section 3.3, the firm-fixed-effect regression of Stata drops the coefficient on *AHP* to avoid perfect collinearity.
[14] In column 4, the coefficient on *AHPPost* is statistically significant for one-tailed tests.

of managing the risk of equity portfolios. These results are consistent with the findings of prior studies that managers use earnings management tools to mitigate perceived performance risk (Francis et al., 2004; Myers et al., 2007; Benmelech et al., 2010).

4.2.2. Cross-sectional variations in the policy effects on earnings management

While our baseline tests suggest that anti-hedging policies increase earnings management, several factors could attenuate these effects. For example, some firms might adopt such policies to appease their outside parties by demonstrating compliance with the best practices in executive compensation, even though it has no urgent need or intention to regulate managerial hedging. In this case, policies may exist on paper but be weakly enforced, limiting their effectiveness. To address this counterargument, we conduct cross-sectional analyses based on managers' derivative transaction patterns before and after policy adoption.

The presence of managerial derivative transactions prior to policy adoption may signal an urgent need to regulate such transactions that may transform the risk-return profiles of equity portfolios. In contrast, firms without managerial derivative transactions before policy adoption may adopt the policy to signal compliance with the best executive compensation practice, despite the absence of a pressing need to regulate hedging. Therefore, the effect of an anti-hedging policy on earnings management is expected to be more prominent in the former case than in the latter. Furthermore, if firms enforce the policy more strictly, managers would engage less frequently in derivative transactions after policy adoption. In this case, its effect on earnings management would be amplified for firms with a decline in managerial derivative transactions.

We obtain managerial derivative transaction data from the Thomson Reuters Insider Trading Database (SEC Form 4 filings). These transactions consist of options (OPTNS, PUT, CALL), equity-linked warrants (WT), zero-cost collars (CLLR), prepaid variable forwards

(FWD), exchange funds (EXFND), and equity swaps (EQSWP). We exclude Rule 16b-3 transactions and other section 16(b) exempt transactions, such as employee stock option plans and other specific purposes (i.e., tender shares, bona fide gifts, small acquisition under Rule 16a-6, transactions by will or descent and distribution, deposit to or withdrawal from the voting trust) because they are generally unrelated to hedging.[15] We keep only cleansed data (excluding observations with the cleanse codes of 'A' and 'S').

We classify a firm as having managerial derivative transactions in the pre-adoption period if its officers engage in at least one derivative transaction in the pre-adoption period (*DerivaPre* = 1). We decompose this subsample into firms whose officers engage less frequently in derivative transactions in the post-adoption period (*DecreaseDeriva* = 1) and those whose officers do not reduce the frequency of derivative transactions (*DecreaseDeriva* = 0).[16]

Table 4 presents the results of our cross-sectional tests. Panel A presents the distribution of subsamples defined above. For 188 out of 484 sample firms, managers engage in derivative transactions in the pre-adoption period (*DerivaPre* = 1). Within the subgroup, 76 adopters experienced decreases in managerial derivative transactions following policy adoption (*DecreaseDeriva* = 1), while 25 adopters did not (*DecreaseDeriva* = 0). In contrast, 54 non-adopters experienced decreases in managerial derivative transactions after policy adoption (*DecreaseDeriva* = 1), while 33 non-adopters did not (*DecreaseDeriva* = 0). These patterns suggest that the anti-hedging policy is particularly pertinent and more strictly enforced by adopters whose managers curtailed derivative transactions after policy adoption (*DecreaseDeriva* = 1).

[15] For example, Arrow Electronics' anti-hedging policy employs a similar definition of derivative transactions to be banned. For details, please see https://s22.q4cdn.com/708983889/files/doc_downloads/2020/Anti-Hedging-and-Anti-Pledging-Policy.pdf.

[16] *DecreaseDeriva* equals one if the mean of the frequency of managerial derivative transactions in the post-adoption period is lower than that in the pre-adoption period, and zero otherwise. By definition, *DecreaseDeriva* equals zero for all firms with *DerivaPre* = 0.

Nevertheless, some adopters failed to reduce managerial derivative transactions, implying that potential difficulties in detecting and regulating such transactions may weaken the enforcement of anti-hedging policies.

Panel B presents subsample regression results. Columns 1 and 2 report estimates for the composite income-smoothing measure ($Smooth_{PCA}$), while Columns 3 and 4 present the likelihood of meeting or just beating consensus analysts' annual earnings forecasts by two cents (*MorB*). The coefficients on *AHPPost* are significantly positive for firms with pre-adoption managerial derivative transactions (*DerivaPre* = 1 in Columns 1 and 3), but are insignificantly different from zero for firms without pre-adoption managerial derivative transactions (*DerivaPre* = 0 in Columns 2 and 4). The differences between the coefficients on *AHPPost* in Columns 1 versus 2 and in Columns 3 versus 4 are 0.243 and 0.086, respectively, and statistically significant. These results suggest that the anti-hedging policy motivates managers to rely on income smoothing to a greater extent when firms face stronger demands to regulate managerial hedging, as opposed to adopting the policy for symbolic or "window-dressing" purposes.

Panel C presents corroborating evidence. We decompose firms with *DerivaPre* = 1 into two subsamples with *DecreaseDeriva* = 1 (i.e., firms with decreases in managerial derivative transactions following policy adoption) and *DecreaseDeriva* = 0 (i.e., firms without those transactions before policy adoption). The coefficients on *AHPPost* are significantly positive in Columns 1 and 4 (*DecreaseDeriva* = 1), but are insignificantly different from zero elsewhere. Moreover, Columns 1 and 4 show that the coefficients on *AHPPost* are significantly larger in the regression for firms with *DecreaseDeriva* = 1 than in the regression for firms with *DerivaPre* = 0.

Taken together, the results in Table 4 confirm H1 and H2, suggesting that the effects of anti-hedging policies on earnings management are more prominent for firms that actively

discourage managerial derivative transactions through enforcement, relative to firms that adopt such policies for window dressing without real improvements.

4.3. Robustness Tests

4.3.1. The parallel-trend assumption tests

Whether treated and control firms exhibit no differential year-over-year changes in the outcome variables before treatments (Bertrand et al. 2004) is essential for ensuring that anti-hedging policies influence earnings management. Following Angrist and Pischke (2009) and Lechner (2011), we augment Equation (2) with *Pre*(*n)*, *AHPPre*(-*n*), *Post*(*0*), *AHPPost*(*0*), *Post*(*n*), and *AHPPost(n). Pre*(-*n*) equals one if a firm's fiscal year *t* coincides with the *n* years (*n* = 1, 2) before the year in which the firm initiated an anti-hedging policy and zero otherwise.[17] *Post*(*0*) is an indicator variable for the (pseudo) adoption year. *Post*(*n*) equals one if a firm's fiscal year *t* coincides with the *n* years (*n* = 1, 2, 3) after the adoption year and zero otherwise.

Table 5 presents the results of tests for validating the parallel trend assumption. In columns 1 and 2, the coefficients on *AHPPre*(-*n*) for *n* = 1 and 2 are not significantly different from zero, suggesting that earnings management was not significantly different between adopters and their matched non-adopters. The coefficients on *Post(0)* and *Post(n)* for *n* = 1, 2, 3 are insignificantly different from zero, suggesting that, conditional on covariates in the regressions, adopters, and non-adopters show parallel movements in earnings management in the absence of the treatment shock. In contrast, the significantly positive coefficients on *AHPPost*(*n*) for *n* = 1, 2, 3 indicate that the policy adoption generates significantly positive incremental effects on earnings management immediately after the adoption year. Taken together, the results in Table 5 largely support the parallel trend assumption.

[17] To avoid perfect multi-collinearity, we do not include *Pre*(-3) and *AHP*Pre*(-3) in the augmented regression models.

4.3.2. The falsification tests

Another way to test the assumption of parallel trends is to perform the difference-in-differences analysis using false adoption events (Roberts and Whited, 2013). The falsification test may also address the counterargument that firms might have started to regulate managerial hedging transactions long before they started to disclose such policies and thus our hypothesized effects might not necessarily occur after the year in which these firms started to offer such disclosures.

We reset the adoption year to three years before the actual adoption year *T* and then redo the propensity score matching procedure to reselect the treated and matched control firms. *FalsePost*(*-3*) indicates the post-adoption period based on the redefined adoption year. As shown in Table 6, when we shift the adoption year to *T*-3, the significance of the results testing our hypotheses H1 and H2 diminishes. This finding supports the causal effects of anti-hedging policy adoption on earnings management as previously demonstrated in Table 3. The result also dismisses the possibility that firms had already adopted the policies long before disclosing them.

4.3.3. The unmatched sample regression

Our cross-sectional tests require the assignment of a pseudo-adoption year to each control firm based on propensity score matching. While propensity score matching is useful for removing bias associated with a functional form of covariates, it drops a number of observations and thus makes the test results sensitive to design choices (Shipman et al. 2017).

To cross-check the validity of our main results, we use Heckman's (1976) 2SLS regressions for the unmatched entropy-balanced sample. The first-stage probit regression yields the inverse Mills ratio that aggregates information on omitted covariates that influence the adoption of an anti-hedging policy. We add the inverse Mills ratio to the second-stage regression models. The instrumental variable used in 2SLS is the percentage of a firm's peer firms with anti-

hedging policies in place at the end of the previous fiscal year (*PeerAdopt*). To satisfy the exclusion restriction, we do not include the instrumental variable in the second-stage regressions.

Table 7 presents the results of 2SLS for the unmatched sample. The unmatched sample includes 9,149 observations (see Panel A for the sample selection procedure). Panel B presents the first-stage probit regression and exhibits that similar observable factors as in Panel A of Table 2 are associated with the presence of an anti-hedging policy in a fiscal year. In Panel C, we confirm that earnings management is positively related to the presence of an anti-hedging policy in a given year. In column 1, the coefficient on *AHPExist* for *SmoothPCA* is significantly positive for two-tailed tests, supporting H1. In column 2, the coefficient on *AHPExist* for *MorB* is significantly positive for one-tailed tests (t = 1.58), providing additional support for H2, albeit with weaker statistical power compared to the matched sample results. Overall, the results in Table 7 are qualitatively consistent with the propensity-score matched sample results.

4.3.4. Cross-sectional tests based on the availability of publicly traded options

Prior studies suggest that the availability of publicly traded options provides managers with an ex-ante opportunity for managers to hedge their equity holdings at relatively low cost (Gao 2010; Hung et al. 2019; Park et al. 2023). Accordingly, we predict that the adoption of anti-hedging policies will have stronger effects for firms with such low-cost hedging opportunities. To examine this channel, we re-estimate our cross-sectional tests using the presence of firms' publicly traded stock options as the partitioning variable. The results are consistent with this prediction. For brevity, we do not tabulate these results.

4.3.5. Confounding effects of CEO turnover

CEO turnover may influence a successor's financial reporting decisions (e.g., big baths). In addition, the appointment of an external CEO could lead to changes in corporate policy

regarding whether to disallow managers' hedging transactions.

In this section, we test whether our main results are robust to CEO turnover.[18] First, we find that the results remain qualitatively unchanged when firm-year observations with CEO turnover are excluded from the samples. Second, we augment Equation 2 by interacting the indicator variables *AHP*, *Post*, and *AHPPost* with the CEO turnover indicator (*Turnover*). The augmented regression results show that the coefficients on *AHPPost* remain significant, while the coefficients on *AHPPost* × *Turnover* are insignificantly different from zero. Furthermore, when we replace *Turnover* with the indicator for external CEO appointments (*External*), the coefficients on *AHPPost* × *External* are also insignificant. Taken together, these results suggest that CEO turnover—whether internal or external—does not materially alter the effect of anti-hedging policies on earnings management. For brevity, we do not tabulate the results.

4.4. Additional Analysis

4.4.1. Effects of anti-hedging policy adoption on investors' perceived risk[19]

Our hypotheses rest on the premise that managers believe smoother earnings reduce investors' perceived risk (e.g., Graham et al. 2005; Kim et al. 2021). To confirm this premise, we conduct an additional test.

Following Kim et al. (2021), we use the average option-implied volatility over a fiscal year (*ImpliedVolatility*) as a proxy for investors' perceived risk, measured as the mean of daily option-implied volatility available from OptionMetrics. We partition the matched sample using the indicator variable *IncreaseSmooth,* which equals one for firms with smoother earnings after policy

[18] We identify CEO turnover events from the ExecuComp database and hand-collect detailed information about CEO turnover events from the Factiva database. There are only 16 firm-year observations where *External* equals one.
[19] We appreciate the anonymous reviewer's suggestion regarding this test.

adoption than before, and zero otherwise.[20] We then test whether the effect of anti-hedging policies on option-implied volatility differs between these two groups.

Table 8 reports the results based on 2,368 observations (after excluding 148 observations with missing *ImpliedVolitilty*). The coefficient on *AHPPost* is significantly negative for the full matched sample (Column 1) and for firms with *IncreaseSmooth* = 1 (Column 2), but is insignificantly different from zero for firms with *IncreaseSmooth* = 0 (Column 3). The difference in coefficients between Columns 2 and 3 is statistically significant. Overall, these results suggest that investors' perceived risk declines after policy adoption, particularly for firms that engage in greater income smoothing after policy adoption, thereby lending support to our premise.

4.4.2. Exploring managerial alternative responses to anti-hedging policies

In addition to differences in policy enforcement, managers may respond to hedging restrictions through channels other than accrual-based income smoothing or meeting or just beating earnings thresholds. For instance, they may reduce firm-specific wealth exposure by selling equity, undertake less risky investment or financing, or engage in alternative forms of earnings management, such as real activities manipulation. These countervailing mechanisms could either substitute for or complement accrual-based earnings management. To assess these possibilities, we conduct a series of additional analyses presented below.

4.4.2.1. CEO firm-specific wealth and net equity sales

Because anti-hedging policies restrict managers' ability to mitigate equity-portfolio risk through derivative transactions, CEOs may instead choose to reduce their equity holdings rather

[20] Specifically, *IncreaseSmooth* equals one if, for a given firm, the median level of income smoothing in the post-adoption period exceeds that in the pre-adoption period, and zero otherwise. We conduct this test using the matched sample, as the unmatched sample does not assign pseudo-adoption years to non-adopters.

than rely on income smoothing.[21] To examine this possibility, we analyze the effect of anti-hedging policies on CEO firm-specific wealth following the methodology of Coles et al. (2013). We also test whether CEO net equity sales increase after policy adoption. Table 9 reports that neither CEO firm-specific wealth nor CEO net equity sales change significantly following adoption, in both the matched and unmatched samples.

4.4.2.2. Corporate risk-taking activities

To mitigate the impact of anti-hedging policies on their equity portfolios, managers may reduce risk-taking activities. However, taking fewer risks has ambiguous implications for firm value: foregoing profitable but risky investments can harm long-term performance, while curbing excessive risk-taking may enhance value and lessen the need for earnings management to obfuscate performance risk. Managers' responses are therefore likely shaped by weighing these conflicting effects on their equity portfolios.

We examine whether anti-hedging policies reduce risk-taking activities. Following Coles et al. (2006), we measure risk-taking as capital investment (total capital investment, R&D, acquisition, and capital expenditures) and financial leverage. Table 10 shows that neither capital investment nor leverage declines significantly after policy adoption. These results contradict theoretical predictions from Coles et al. (2006) that risk-averse managers adjust real business decisions in response to compensation-related frictions. Instead, the lack of significant changes supports the view that earnings management remains the primary channel through which managers react to hedging constraints.

4.4.2.3. Other forms of earnings management

[21] In reality, many firms adopt target ownership plans requiring managers to maintain a minimum level of stock ownership (Core and Larcker 2002; Brisley et al. 2021). These constraints limit managers' ability to reduce equity holdings, thereby increasing demand for hedging transactions that avoid direct equity sales.

In this section, we explore the effects of anti-hedging policies on other forms of earnings management. First, real activity manipulation can boost short-term stock prices but is generally viewed as detrimental to long-term firm value (Ewert and Wagenhofer 2005). Following Roychowdhury (2006), we measure it as abnormal production minus abnormal cash flows from operations and abnormal discretionary expenses. Table 11 shows that anti-hedging policies significantly increase real activity manipulation in the matched sample, but have no significant effect in the unmatched sample. The relatively weaker effects compared to income smoothing or meeting earnings thresholds suggest that managers weigh the costs and benefits of real activity manipulation. Second, financial misreporting may inflate short-term stock prices but also raises the likelihood of future stock price crashes when restatements or SEC investigations occur (Armstrong et al. 2013). Untabulated results show no increase in financial misreporting—measured by restatements due to non-clerical errors and SEC Accounting and Auditing Enforcement Releases—after policy adoption. This result suggests that managers do not resort to misreporting beyond the boundaries of accounting standards in response to anti-hedging policies.

5. Conclusion

This study examines how anti-hedging policies affect earnings management. We find that the adoption of an anti-hedging policy increases income smoothing and the tendency to meet or just beat analyst earnings forecasts, shaping investor perceptions of the risk and persistence of firm performance. Notably, firms whose managers engaged in derivative transactions in the pre-adoption periods witnessed a sharp reduction in such transactions in the post-adoption periods, and the policy effect on earnings management is strongest for these firms. However, managerial risk-taking activities and firm-specific wealth remain largely unchanged.

Our findings contribute to two strands of literature. First, we add new evidence to the literature on the relationship between equity incentives and earnings management. Specifically, we find that managers smooth income to a greater extent and become more inclined to meet or just beat consensus analysts' forecasts after the adoption of an anti-hedging policy, especially when policy adoption is intended to regulate managerial hedging rather than serve window-dressing purposes. Second, we extend research on compensation and hedging by providing evidence on the economic consequences of anti-hedging policies. While these policies effectively reduce managerial derivative transactions, they inadvertently motivate managers to resort to earnings management to mitigate the increased compensation risk. Thus, the policies aiming to align the interests of shareholders and managers may trigger collateral effects, particularly a heightened tendency towards earnings management.

Our findings have important implications for boards of directors, regulators, investors, and other market participants. First, although these policies are intended to discourage managers from taking excessive risks by hedging the downside risk of equity incentives, our evidence suggests that they may instead prompt managers to manipulate earnings performance without significantly reducing risk-taking activities. Accordingly, boards of directors and regulators should be cautious in assuming that anti-hedging policies are universally beneficial. They should monitor these unintended consequences and adopt complementary safeguards that encompass compensation design, internal controls, and oversight mechanisms to mitigate unintended consequences resulting from imposing greater compensation-related risk exposure on managers.

Second, investors and other market participants can use the presence of an anti-hedging policy to form expectations about how a firm's equity incentive plans align managerial and shareholder interests (ISS 2012). However, the policy may inadvertently encourage earnings

management. Therefore, they should consider not only whether the policy exists, but also how it is enforced and how it influences earnings quality.

In this study, we have taken rigorous measures to ensure the robustness of our results. We have minimized the impact of confounding covariates and have achieved consistent outcomes across various methods, including propensity-score matching and Heckman 2SLS. Nevertheless, we acknowledge that these remedial measures cannot completely eradicate the endogeneity issue inherent in the voluntary adoption of an anti-hedging policy. Therefore, researchers must exercise caution in interpreting our empirical findings and consider the limitations associated with voluntary policy adoption.

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Appendix A. Examples of corporate policies on managerial equity incentive hedging

We manually searched the proxy statements of all non-financial and utility firms of the Execucomp database between 2004 and 2015 and identified the fiscal year of the proxy statement that first mentions the adoption of an anti-hedging policy by a firm. We determined each firm's adoption year by identifying the earliest DEF 14A filing that describes such policy. We found all sentences containing keywords, such as 'hedg-', 'derivative', and 'option', and carefully read these sentences to identify whether the firm adopts an anti-hedging policy for executive compensation in the year. Allowing hedging transactions with the board's approval was not counted as an anti-hedging policy.

Proxy statements disclosures

MONDELEZ INTERNATIONAL, INC. (proxy disclosure on 3/13/2007)

Prohibits officers (and any member of the officer's family sharing the household) from transactions in puts, calls or other derivative Company securities on an exchange or in any other organized market, as well as any other derivative or hedging transactions on Company securities.

TENNANT CO (proxy disclosure on 3/19/2010)

Anti-Hedging and Pledge Prohibition

In February 2010, the Board amended our insider trading policy with respect to access persons (persons with certain financial information as defined by the policy) to prohibit speculative trading or hedging of positions in Tennant securities, including writing or trading in options, warrants, or any other derivatives of Tennant securities, or entering into any transactions designed specifically to protect or hedge against a decrease in value of Tennant securities. It also prohibited pledges of any Tennant securities (e.g., pledge to a bank or financial institution as collateral for a loan, or pledge to a broker in connection with a market transaction, such as a margin loan or prepaid forward sale contract).

CARRIZO OIL & GAS (proxy disclosure on 4/30/2013)

In 2013, the Board adopted a policy applicable to all the named executive officers and directors of the Company, prohibiting hedging of Carrizo Oil & Gas, Inc. securities, including publicly traded options, puts, calls and short sales.

CISCO (proxy disclosure on 9/30/2015)

Cisco's compensation philosophy and related governance features are complemented by specific elements designed to align Cisco's executive compensation with long-term shareholder interests and effective compensation-related risk management, including the following:

- Executive officers are prohibited from engaging in any speculative transactions in Cisco securities, including engaging in short sales, engaging in transactions involving put options, call options, or other derivative securities, or engaging in any other forms of hedging transactions, such as collars or forward sale contracts; and
- Executive officers are strongly discouraged from pledging Cisco securities in margin accounts or as collateral for loans, and no executive officer currently engages in such practices.

Based in part on this philosophy and these governance features, the Compensation Committee does not believe that the compensation program creates risks that are reasonably likely to have a material adverse effect on Cisco.

Corporate governance policy or codes of business conducts

CISCO (https://investor.cisco.com/corporate-governance)

A violation, including failure to report potential violations, of the Financial Officer Code of Ethics will be viewed as a severe disciplinary matter and may result in personnel action, including termination of employment. If you believe that a violation has occurred, please contact Cisco Legal, the Ethics Office, or the Audit Committee of the Board of

Directors. As with the COBC, it is against Cisco policy to retaliate against an employee for good-faith reporting of any potential or actual Code violations.

Derivatives and hedging transactions are not permitted.

Cisco employees are also prohibited from trading in any Cisco derivative securities, such as put and call options, regardless of whether the employee has material, non-public information. Cisco's policy also prohibits short selling or engaging in any other forms of hedging transactions in Cisco securities, such as collars or forward sale contracts, because of the divergence it could create between objectives of employees and other shareholders.

Appendix B. Variable definitions

Variable	Description
Variables used in the probit regression for propensity score matching	
$Adoption_{PSM}$	An indicator variable that equals one for the year in which a firm initiates an anti-hedging policy and zero for all years for non-adopters.
FirmSize	The natural logarithm of a firm's net sales in a year.
RetVol	The standard deviation of a firm's raw daily returns during a year.
BLeverage	The ratio of a firm's long-term liability minus deferred taxes to its total assets at the end of a year.
MTB	The ratio of a firm's market value of equity to its book value of equity at the end of a year.
ROA	A firm's earnings before extraordinary items deflated by its total assets at the beginning of a year.
CAR	A firm's cumulative abnormal stock returns during a year (i.e., daily stock returns less CRSP value-weighted returns cumulated during a year).
Analysts	The natural logarithm of the number of analysts who issue earnings forecasts for a firm in a year, and zero if missing.
Instown	The ratio of a firm's outstanding shares owned by institutional investors to the firm's total shares outstanding at the end of a year, and zero if missing.
Independence	The percentage of independent directors on the board of directors of a firm at the end of a year.
CEOOwn	The ratio of shares owned by CEOs to total shares outstanding at the end of a year.
Tenure	The number of years that an executive officer has held the position of CEO as of the end of a year.
Dual	An indicator variable that equals one if the CEO is the board chairman at the end of a year, and zero otherwise.
OptionExist	An indicator variable that equals one if the firm's options are publicly traded in a year, and zero otherwise.
SumVolume	The natural logarithm of the total number of a firm's options traded by investors during a year.
SumValue	The sum value of net share purchases by a firm's CEO deflated by the market value of equity at the end of a year.
PeerAdopt	The percentage of a firm's peer firms (based on a two-digit SIC code) that have an anti-hedging policy in place at the start of a year. It is used as an instrumental variable in this study.
Variables used in the analysis of the effects of anti-hedging policy adoption	
AHP_i	An indicator variable that equals one for an anti-hedging policy adopter and zero for a propensity-score-matched non-adopter. For example, if a firm adopts the policy in 2010 (T = 2010), *AHP* equals one for the firm for the T-3 to T+3 period and zero for its matched non-adopter during the same period.
$Post_{i,t}$	An indicator variable that equals one for a firm-year pair for which an anti-hedging policy is in place, and zero otherwise. The same pseudo-adoption year as an adopter is assigned to its matched non-adopter.
$AHPPost_{i,t}$	An indicator variable that equals one if a fiscal year of an anti-hedging policy adopter lies in its post-adoption period and zero otherwise.
$AHPExist_{i,t}$	An indicator variable that equals one if a firm has an anti-hedging policy in place in a given fiscal year and zero otherwise. The variable is used for the unmatched sample regressions.
$Smooth_{PCA}$	The income smoothing composite score measured as the first component from the principal component analysis of $Smooth_{TZ}$, $Smooth_{LNW,}$ and $Smooth_{BPT}$.

$Smooth_{TZ}$	An income smoothing measure that equals -1 x the correlation coefficient between the change in accruals and the change in pre-discretionary income based on observations from year *t*-4 to year *t* (Tucker and Zarowin 2006).
$Smooth_{LNW}$	An income smoothing measure that equals the negative value of the ratio of the standard deviation of operating income to the standard deviation of cash flows from operating activities from year *t*-4 to year *t* (Leuz et al. 2003)
$Smooth_{BPT}$	An income smoothing measure that equals the standard deviation of unmanaged earnings less the standard deviation of reported earnings (Black et al. 2022). The standard deviation of reported earnings is calculated over the five most recent years of the annual earnings scaled by average total assets. The standard deviation of unmanaged earnings is calculated as the standard deviation over the five most recent pseudo years of earnings scaled by average total assets. The pseudo year is defined as the first quarter of the current year and the last three quarters of the prior year.
MorB	Meeting or just beating consensus analyst forecasts, which equals one if the difference between reported earnings per share and consensus analyst forecast of earnings per share lies between 0 and 2 cents, and zero otherwise.
LogDelta	The natural logarithm of CEO equity portfolio delta in a fiscal year. Delta is a dollar change in the CEO incentive portfolio for a one percent change in stock price.
LogVega	The natural logarithm of CEO equity portfolio vega in a fiscal year. Vega is the dollar change in the CEO incentive portfolio for a one percent change in the standard deviation of stock return.
DerivaPre	An indicator variable for managerial engagement in derivative transactions prior to the adoption of an anti-hedging policy. We count call, put or unspecified options, prepaid variable forward contracts, equity swaps, exchange funds, collar and warrants, that do not fall into Rule 16b-3 transactions, other section 16(b) exempt transactions and small acquisitions, disposition under a tender of shares in a change of control transaction, value-added transactions, or inactive transactions.
DecreaseDeriva	An indicator variable for firms whose managers engage less frequently in derivative transactions following anti-hedging policy adoption than before.
Turnover	An indicator variable that is set to one if the firm experiences CEO turnover in a year, and zero otherwise.
External	An indicator variable that is set to one if the firm has external CEO appointments in a year, and zero otherwise.
ImpliedVolatility	Investors' perceived risk, measured as the mean of daily implied volatility (available from OptionMetrics database) over a fiscal year.
IncreaseSmooth	An indicator variable that equals one if a firm's within-firm median level of income smoothing in the post-adoption (or post-pseudo-adoption) period exceeds that in the pre-adoption (or pre-pseudo-adoption) period, and zero otherwise.
FirmRelatedWealth	The natural logarithm of CEO firm-related wealth in a fiscal year. CEO firm-related wealth is the value of the CEO's stock and option portfolio.
NetSales	CEO's net sales of the shares in a year, deflated by the number of outstanding shares at the beginning of a fiscal year.
Investment	Total investment deflated by the total assets at the beginning of a fiscal year.
RD	Research & development expenditures deflated by total assets at the beginning of a fiscal year.
Acquisition	Acquisitions deflated by the total assets at the beginning of a fiscal year.
Capex	Capital expenditure deflated by the total assets at the beginning of a fiscal year.
BLeverage	The ratio of a firm's long-term liability minus deferred taxes to its total assets at the end of a year.

REM	The portion of earnings inflated by real activities manipulation is measured as abnormal cash flows from operating activities plus abnormal production costs less abnormal discretionary expenditures (Roychowdhury 2006).

Table 1. Anti-hedging policy adoption by year and industry

This table presents the number of firms adopting an anti-hedging policy by year and by industry. Financial firms and utility firms are not counted in this table. The final sample for hypothesis tests excludes 6 adopters before 2006.

Panel A. Number of anti-hedging policy adopters by year

Year	Number of firms with an anti-hedging policy in place	Number of firms without an anti-hedging policy in place	Number of new adopters
2004	2	1,939	2
2005	4	1,937	2
2006	75	1,866	71
2007	106	1,835	31
2008	125	1,816	19
2009	161	1,780	36
2010	334	1,607	173
2011	499	1,442	165
2012	771	1,170	272
2013	958	983	187
2014	1071	870	113
2015	1,153	788	82

Panel B. Number of anti-hedging policy adopters by industry

Industry (SIC two-digit codes)	Frequency	Percentage
Agriculture, forestry, and fishing (01-09)	4	0.35
Mining (10-14)	81	7.03
Construction (15-17)	28	2.43
Manufacturing (20-39)	597	51.78
Transportation and communications (40-48)	74	6.42
Wholesale trade (50-51)	45	3.90
Retail trade (52-59)	114	9.89
Services (70-88)	208	18.04
Non-classifiable (99)	2	0.17
Total	1,153	100

Table 2. The sample selection procedure and descriptive statistics

This table presents the procedures for selecting a propensity-score-matched sample. All variables are defined in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels for two-sided *t*-tests, respectively.

Panel A. The sample selection procedure

All Execucomp constituents (excluding financial and utility firms) adopting anti-hedging policies for 2004-2015	1,153 firms
Subset matched with control firms using the propensity score matching	714 firms
Merged firm-years with all determinants for CEO total compensation available for 2004-2015	
Firm-years omitted beyond *T*-3 to *T*+3	3,922
Firm-years omitted for firms with missing values for the variables for analysis	(1,111)
Requiring at least one non-missing observation in both the pre-adoption period and the post-adoption period	(295)
Final firm-year sample	2,516

Panel B. Results of estimating a probit regression to estimate propensity scores

VARIABLES	*Coefficient*	(t-value)
$PeerAdopt_{i,t-1}$	1.482***	(7.010)
$RetVol_{i,t-1}$	0.125	(0.975)
$FirmSize_{i,t-1}$	0.199***	(9.221)
$ROA_{i,t-1}$	0.569***	(2.694)
$MTB_{i,t-1}$	-0.010	(-1.297)
$CAR_{i,t-1}$	0.052	(0.811)
$BLeverage_{i,t-1}$	-0.219**	(-2.053)
$Analysts_{i,t-1}$	0.104***	(3.608)
$Instown_{i,t-1}$	0.259**	(2.511)
$CEOOwn_{i,t-1}$	-1.509***	(-2.630)
$Independence_{i,t-1}$	2.299***	(13.259)
$Tenure_{i,t-1}$	-0.006	(-1.548)
$Dual_{i,t-1}$	-0.007	(-0.131)
$OptionExist_{i,t-1}$	-0.100	(-0.622)
$SumVolume_{i,t-1}$	0.057***	(4.257)
$SumValue_{i,t-1}$	9.997**	(2.119)
Constant	-4.909***	(-19.234)
Pseudo R-squared	0.179	
Observations	5,072	

Panel C. *T*-tests for the mean differences in propensity score and covariates in the adoption years

	Before Entropy Balancing			After Entropy Balancing		
Variables	Adopters (357 firms) (1)	Non-adopters (357 firms) (2)	Mean difference (3)=(1)–(2)	Adopters (357 firms) (4)	Non-adopters (357 firms) (5)	Mean difference (6)=(4)–(5)
Propensity Score	0.206	0.205	0.001	0.205	0.207	-0.002
PeerAdopt	0.096	0.105	-0.009	0.095	0.101	-0.006
RetVol	0.434	0.468	-0.034**	0.432	0.432	0.000
FirmSize	7.078	6.737	0.341***	7.073	7.061	0.012
BLeverage	0.207	0.184	0.023	0.202	0.200	0.002
MTB	2.916	2.829	0.087	2.882	2.826	0.056
ROA	0.057	0.061	-0.004	0.056	0.055	0.001
CAR	0.092	0.087	0.005	0.094	0.089	0.005
Analysts	2.174	2.091	0.083	2.171	2.201	-0.030
Instown	0.760	0.748	0.012	0.759	0.759	0.000
Independence	0.694	0.723	-0.029**	0.693	0.692	0.001
CEOOwn	0.018	0.021	-0.003	0.017	0.017	0.000
Tenure	9.020	9.524	-0.504	8.930	9.006	-0.076
Dual	0.502	0.454	0.048	0.501	0.506	-0.005
OptionExist	0.919	0.927	-0.008	0.919	0.916	0.003
SumVolume	10.544	10.327	0.217	10.542	10.452	0.090
SumValue	-0.002	-0.002	0.000	-0.002	-0.002	0.000

Panel D. Descriptive statistics

VARIABLES	N	Mean	*StdDev*	p25	p50	p75
AHP_i	2,516	0.548	0.498	0.000	1.000	1.000
$Post_{i,t}$	2,516	0.541	0.498	0.000	1.000	1.000
$AHPPost_{i,t}$	2,516	0.303	0.460	0.000	0.000	1.000
$Smooth_{PCA,\ i,t}$	2,516	-0.002	0.997	-0.637	0.067	0.749
$Smooth_{TZ,\ i,t}$	2,516	-0.052	0.521	-0.485	-0.067	0.365
$Smooth_{LNW,\ i,t}$	2,516	-1.038	0.623	-1.312	-0.921	-0.599
$Smooth_{BPT,\ i,t}$	2,516	0.002	0.026	-0.006	0.000	0.007
$MorB_{i,t}$	2,516	0.197	0.398	0.000	0.000	0.000
$IMR_{i,t}$	2,516	0.728	0.160	0.605	0.714	0.832
$PeerAdopt_{i,t-1}$	2,516	0.092	0.099	0.017	0.054	0.141
$RetVol_{i,t-1}$	2,516	0.433	0.197	0.294	0.393	0.525
$FirmSize_{i,t-1}$	2,516	7.067	1.418	6.083	7.010	8.097
$ROA_{i,t-1}$	2,516	0.059	0.091	0.023	0.060	0.102
$MTB_{i,t-1}$	2,516	2.642	2.089	1.336	2.066	3.175
$BLeverage_{i,t-1}$	2,516	0.197	0.192	0.007	0.174	0.305
$CAR_{i,t-1}$	2,516	0.087	0.345	-0.113	0.066	0.266
$Analysts_{i,t-1}$	2,516	2.183	0.967	1.792	2.303	2.890
$Instown_{i,t-1}$	2,516	0.772	0.202	0.697	0.821	0.914
$CEOOwn_{i,t-1}$	2,516	0.016	0.036	0.001	0.004	0.012
$Independence_{i,t-1}$	2,516	0.680	0.160	0.538	0.634	0.857
$Tenure_{i,t-1}$	2,516	9.176	7.186	4.000	7.000	12.000
$Dual_{i,t-1}$	2,516	0.496	0.500	0.000	0.000	1.000
$OptionExist_{i,t-1}$	2,516	0.919	0.274	1.000	1.000	1.000
$SumValue_{i,t-1}$	2,516	-0.002	0.004	-0.001	0.000	0.000
$SumVolume_{i,t-1}$	2,516	10.551	3.866	9.427	11.275	13.023
$LogVega_{i,t-1}$	2,516	3.929	1.508	3.029	4.077	4.978
$LogDelta_{i,t-1}$	2,516	5.213	1.377	4.350	5.195	6.112

Table 3. Effects of an anti-hedging policy on earnings management

This table presents the results of testing the effects of an anti-hedging policy on earnings management for the matched sample after entropy balancing. We use two measures of earnings management: (1) income smoothing and (2) meeting or just beating consensus analysts' earnings forecasts. T-statistics in parentheses are based on standard errors clustered by firms. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels for two-tailed tests, respectively. † is statistical significance at the 10% level for one-tailed tests. See Appendix B for definitions of variables.

	Dependent variable =				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	$Smooth_{PCA,\ i,t}$	$Smooth_{TZ,\ i,t}$	$Smooth_{LNW,\ i,t}$	$Smooth_{BPT,\ i,t}$	$MorB_{i,t}$
$Post_{i,t}$	-0.165**	-0.050	-0.094**	-0.002	-0.078**
	(-2.51)	(-1.47)	(-2.18)	(-0.98)	(-2.14)
AHPPost$_{i,t}$	**0.308*** **	**0.115** **	**0.152*** **	**0.003†**	**0.083** **
	(3.35)	**(2.46)**	**(2.79)**	**(1.38)**	**(2.37)**
Control variables					
$IMR_{i,t}$	1.609	0.803	0.892	-0.099**	-0.661
	(1.17)	(1.22)	(1.00)	(-1.97)	(-0.99)
$LogDelta_{i,t-1}$	-0.038	0.024	-0.072**	0.002	0.011
	(-0.69)	(0.94)	(-2.02)	(0.95)	(0.51)
$LogVega_{i,t-1}$	-0.004	-0.004	0.008	-0.003**	-0.004
	(-0.08)	(-0.20)	(0.27)	(-1.97)	(-0.28)
$RetVol_{i,t-1}$	-0.758**	-0.224	-0.517**	0.026**	0.224
	(-2.08)	(-1.27)	(-2.16)	(2.10)	(1.22)
$FirmSize_{i,t-1}$	0.122	0.083	0.060	-0.017***	-0.144***
	(0.96)	(1.19)	(0.92)	(-3.50)	(-2.94)
$ROA_{i,t-1}$	-0.242	-0.143	-0.121	0.020	0.288**
	(-0.79)	(-0.96)	(-0.60)	(1.49)	(2.17)
$MTB_{i,t-1}$	0.018	0.010	0.006	-0.001	-0.012
	(0.78)	(0.80)	(0.44)	(-0.83)	(-1.44)
$BLeverage_{i,t-1}$	-0.367	-0.069	-0.274*	0.002	0.098
	(-1.47)	(-0.60)	(-1.75)	(0.38)	(1.00)
$CAR_{i,t-1}$	-0.075	-0.046	-0.020	0.003	0.001
	(-1.13)	(-1.26)	(-0.47)	(1.00)	(0.04)
$Analysts_{i,t-1}$	-0.070	-0.031	-0.006	-0.002	-0.002
	(-0.62)	(-0.55)	(-0.08)	(-0.54)	(-0.05)
$Instown_{i,t-1}$	0.135	0.198	-0.038	-0.017	-0.149
	(0.42)	(1.27)	(-0.20)	(-1.64)	(-1.20)
$CEOOwn_{i,t-1}$	0.068	-0.544	0.704	0.023	0.006
	(0.03)	(-0.53)	(0.54)	(0.37)	(0.00)
$Independence_{i,t-1}$	0.397	0.155	0.224	-0.012*	-0.042
	(1.44)	(1.12)	(1.28)	(-1.75)	(-0.38)
$Tenure_{i,t-1}$	-0.017	-0.010*	-0.006	0.001**	0.002
	(-1.38)	(-1.81)	(-0.82)	(2.00)	(0.46)
$Dual_{i,t-1}$	0.175	0.078	0.085	-0.003	-0.047
	(1.35)	(1.25)	(1.10)	(-0.76)	(-0.87)
$OptionExist_{i,t-1}$	0.322	0.095	0.176	0.014	0.021
	(0.98)	(0.57)	(0.90)	(1.38)	(0.15)
$SumVolume_{i,t-1}$	-0.026	-0.009	-0.011	-0.002*	-0.005
	(-0.63)	(-0.43)	(-0.45)	(-1.68)	(-0.28)

$SumValue_{i,t-1}$	6.845	4.545	1.418	-0.373	-2.853
	(0.94)	(1.34)	(0.31)	(-1.40)	(-0.76)
Constant	-1.602	-1.353	-1.639	0.213***	1.760**
	(-0.92)	(-1.53)	(-1.54)	(2.94)	(2.21)
Observations	2,516	2,516	2,516	2,516	2,516
Adjusted R-squared	0.486	0.488	0.470	0.178	0.136
Firm-fixed Effects	YES	YES	YES	YES	YES
Year-fixed Effects	YES	YES	YES	YES	YES
SE clustered by firm	YES	YES	YES	YES	YES

Table 4. Results of cross-sectional tests

This table presents the results of cross-sectional tests based on the subsamples of firms with and without managerial derivative transactions in the pre-adoption period. Derivative transactions consist of call, put or unspecified options, prepaid variable forward contracts, equity swaps, exchange funds, and warrants, that do not fall into Rule 16b-3 Transactions, other section 16(b) exempt transactions and small acquisitions, disposition under a tender of shares in a change of control transaction, value-added transactions, or inactive transactions. *DerivaPre* is equal to one if a firm's officers engaged in derivative transactions in the pre-adoption periods and zero otherwise. *DecreaseDeriva* is equal to one if a firm's officers engage less frequently in derivative transactions after the policy adoption than before and zero otherwise. ***, **, and * denote statistical significance levels at 1%, 5%, and 10% in two-tailed tests. † denotes a statistical significance level at 10% in one-tailed tests. T-statistics in parentheses are based on standard errors clustered by firms.

Panel A. Number of sample firms with and without managerial derivative transactions

	(1) Adopters	(2) Non-adopters	(3) Difference (1)-(2)
DerivaPre = 1 (188 firms)			
(1) *DecreaseDeriv* = 1 (130 firms)	76	54	22
(2) *DecreaseDeriv* = 0 (58 firms)	25	33	-8
DerivaPre = 0 (296 firms)			
(1) *DecreaseDeriv* = 1 (0 firms)	0	0	0
(2) *DecreaseDeriv* = 0 (296 firms)	155	141	14

Panel B. Results for subsamples with and without pre-adoption managerial derivative transactions

	Dependent variable =			
	$Smooth_{PCA,\ i,t}$		$MorB_{i,t}$	
	(1) *DerivaPre* = 1	(2) *DerivaPre* = 0	(3) *DerivaPre* = 1	(4) *DerivaPre* = 0
$Post_{i,t}$	-0.246**	-0.111	-0.106*	-0.050
	(-2.40)	(-1.35)	(-1.88)	(-1.05)
***AHPPost*$_{i,t}$**	**0.435*** **	**0.192**	**0.134** **	**0.048**
	(3.36)	**(1.59)**	**(2.36)**	**(1.08)**
Control variables	Included	Included	Included	Included
Subgroup coefficient difference in	**(1) - (2)**		**(3) - (4)**	
***AHPPost*$_t$**	**0.243****		**0.086***	
(p-value)	**(0.02)**		**(0.08)**	
Observations	993	1,523	993	1,523
Adjusted R-squared	0.555	0.440	0.158	0.124
Firm-fixed Effects	YES	YES	YES	YES
Year-fixed Effects	YES	YES	YES	YES
SE clustered by firm	YES	YES	YES	YES

Panel C. Effects of an anti-hedging policy on earnings management via the frequency of managerial derivative transactions

	Dependent variable =					
	$Smooth_{pca,\ i,t}$			$MorB_{i,t}$		
	DerivaPre = 1		*DerivaPre* = 0	*DerivaPre* = 1		*DerivaPre* = 0
	(1)	(2)	(3)	(4)	(5)	(6)
	DecreaseDeriva = 1	*DecreaseDeriva*= 0		*DecreaseDeriva* = 1	*DecreaseDeriva*= 0	
$Post_{i,t}$	-0.401***	0.017	-0.111	-0.058	-0.213*	-0.050
	(-3.27)	(0.09)	(-1.35)	(-0.84)	(-2.00)	(-1.05)
***AHPPost*$_{i,t}$**	**0.602*** **	**0.121**	**0.192**	**0.137***	**0.111**	**0.048**
	(3.88)	**(0.51)**	**(1.59)**	**(1.94)**	**(1.02)**	**(1.08)**
Control variables	Included	Included	Included	Included	Included	Included
Subgroup coefficient difference in	**(1) vs. (3)**			**(4) vs. (6)**		
***AHPPost*$_t$**	**0.410** **			**0.089***		
(p-value)	**(0.02)**			**(0.08)**		
Observations	704	289	1,523	704	289	1,523
Adjusted R-squared	0.568	0.581	0.440	0.142	0.162	0.124
Firm-fixed Effects	YES	YES	YES	YES	YES	YES
Year-fixed Effects	YES	YES	YES	YES	YES	YES
SE clustered by firm	YES	YES	YES	YES	YES	YES

Table 5. Results of parallel trend assumption test

This table presents the results of validating the parallel trend assumption. We test whether earnings management differs between the policy adopters and their matched non-adopters before a (pseudo) adoption year and increases in or after the adoption year. *Pre(-n)* is equal to one for a fiscal year *n* years before the year in which a firm first initiates an anti-hedging policy, and zero otherwise. *Post(n)* is equal to one for a fiscal year *n* years after the adoption year and zero otherwise. To avoid perfect multicollinearity, we drop *Pre(-3)* and *AHPPre(-3)*. For brevity, only focal variables are tabulated. T-statistics in parentheses are based on standard errors clustered by firms. *, **, and *** represent significance at the 10%, 5%, and 1% level in two-tailed tests, respectively. † indicates significance at the 10% level in one-tailed tests.

	Dependent variable =	
	(1)	(2)
	$Smooth_{PCA,\ i,t}$	$MorB_{i,t}$
$Pre(-2)_{i,t}$	0.055	0.042
	(0.74)	(1.09)
$Pre(-1)_{i,t}$	0.141*	0.035
	(1.74)	(0.88)
$Post(0)_{i,t}$	0.132	0.019
	(1.52)	(0.48)
$Post(1)_{i,t}$	0.104	-0.060
	(1.15)	(-1.44)
$Post(2)_{i,t}$	-0.074	0.014
	(-0.79)	(0.29)
$AHPPre(-2)_{i,t}$	**0.076**	**0.011**
	(0.73)	**(0.20)**
$AHPPre(-1)_{i,t}$	**0.071**	**0.035**
	(0.59)	**(0.64)**
$AHPPost(0)_{i,t}$	**0.166**	**0.040**
	(1.30)	**(0.70)**
$AHPPost(1)_{i,t}$	**0.316****	**0.181*****
	(2.17)	**(3.16)**
$AHPPost(2)_{i,t}$	**0.571*****	**0.093†**
	(3.27)	**(1.44)**
$AHPPost(3)_{i,t}$	**0.667*****	**0.124***
	(3.64)	**(1.86)**
Control variables	Included	Included
Observations	2,516	2,516
Adjusted R-squared	0.491	0.136
Firm-fixed Effects	YES	YES
Year-fixed Effects	YES	YES
SE clustered by firm	YES	YES

Table 6. Results of the falsification tests

This table presents the results of falsification tests by resetting the adoption year to the actual adoption year minus three years. *FalsePost(-3)* equals one for the false post-adoption period when we reset a firm's adoption year to be three years before its actual adoption year. For brevity, only focal variables are tabulated. T-statistics in parentheses are based on standard errors clustered by firms. *, **, and *** represent significance at the 10%, 5%, and 1% level in two-tailed tests, respectively.

	Dependent variable =	
	(1)	(2)
	$Smooth_{PCA,\ i,t}$	$MorB_{i,t}$
$FalsePost(-3)_{i,t}$	-0.039	-0.014
	(-0.60)	(-0.43)
$\boldsymbol{AHPFalsePost(-3)_{i,t}}$	**0.023**	**0.030**
	(0.29)	**(1.01)**
Control variables	Included	Included
Observations	3,075	3,075
Adjusted R-squared	0.435	0.142
Firm-fixed Effects	YES	YES
Year-fixed Effects	YES	YES
SE clustered by firm	YES	YES

Table 7. Heckman's 2SLS for the unmatched sample

This table presents the results of testing our hypothesis using Heckman's (1976) two-stage least squares regressions for the unmatched full sample between 2004 and 2015. The first-stage probit regression includes an instrumental variable (*PeerAdopt*), while the second-stage regression excludes the instrumental variable. We entropy-balance the treated and control groups with regard to the second moment of each variable. For brevity, only focal variables are tabulated. *AHPExist* equals one for the years in which a firm has an anti-hedging policy in place and zero otherwise. In the unmatched sample, no pseudo-adoption year is assigned to a non-adopter. T-statistics in parentheses are based on standard errors clustered by firms. *, **, and *** represent significance at the 10%, 5%, and 1% level in two-tailed tests, respectively. † indicates significance at the 10% level in one-tailed tests.

Panel A. The sample selection procedure for the unmatched sample

All Execucomp constituents (excluding financial and utilities firms) adopting anti-hedging policies between 2004 and 2015 inclusive	1,153 firms
The subset merged with the variables used to test the hypothesis between 2004 and 2015 inclusive, including both adopt firms and non-adopt firms	42,971
Omitting all missing values of the variables for analysis	(33,822)
Final firm-year unmatched sample	9,149
* 6,464 observations for 857 adopters and 2,685 observations for 483 non-adopters.	

Panel B. Results of testing a probit regression to estimate the inverse Mills ratio

	Dependent variable = $AHPExist_{i,t}$	
	Coefficient	(*t*-value)
***PeerAdopt*$_{i,t-1}$**	**6.092*** **	**(34.83)**
RetVol$_{i,t-1}$	0.027	(0.26)
FirmSize$_{i,t-1}$	0.123***	(8.06)
ROA$_{i,t-1}$	-0.171	(-0.99)
MTB$_{i,t-1}$	0.005	(1.43)
BLeverage$_{i,t-1}$	-0.028	(-0.35)
CAR$_{i,t-1}$	-0.073	(-1.42)
Analysts$_{i,t-1}$	0.106***	(5.32)
Instown$_{i,t-1}$	0.326***	(3.99)
CEOOwn$_{i,t-1}$	-2.502***	(-4.04)
Independence$_{i,t-1}$	1.889***	(14.77)
Tenure$_{i,t-1}$	-0.001	(-0.19)
Dual$_{i,t-1}$	-0.011	(-0.32)
OptionExist$_{i,t-1}$	-0.116	(-0.89)
SumVolume$_{i,t-1}$	0.056***	(5.74)
SumValue$_{i,t-1}$	6.386	(1.58)
Constant	-4.389***	(-22.39)
Pseudo R-squared	0.310	
Observations	9,149	

Panel C. Results of the second-stage regressions for the unmatched sample

	Dependent variable =	
	(1)	(2)
	$Smooth_{PCA,\,i,t}$	$MorB_{i,t}$
$AHPExist_{i,t}$	**0.148*** **	**0.026†**
	(2.98)	**(1.58)**
Inverse Mills Ratio$_{i,t}$	-0.236**	0.004
	(-2.23)	(0.13)
The other control variables	Included	Included
Observations	9,149	9,149
Adjusted R-squared	0.351	0.127
Firm-fixed Effects	YES	YES
Year-fixed Effects	YES	YES
SE clustered by firm	YES	YES

Table 8. Effects of an anti-hedging policy on investors' perception of risk

This table presents the results of testing the effects of an anti-hedging policy on investors' perceived risk. The dependent variable, *ImpliedVolatility*, is the mean of daily option-implied volatility over a fiscal year. The partitioning variable, *IncreaseSmooth*, is equal to one for firms with income smoothing increasing following the adoption of anti-hedging policies and zero otherwise. T-statistics in parentheses are based on standard errors clustered by firms. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels for two-tailed tests, respectively. See Appendix B for definitions of variables.

	Dependent Variable = *ImpliedVolatility*		
	(1)	(2)	(3)
VARIABLES		*IncreaseSmooth* = 1	*IncreaseSmooth* = 0
$Post_{i,t}$	0.012	0.022	0.003
	(1.33)	(1.55)	(0.26)
***AHPPost*$_{i,t}$**	**-0.028*** **	**-0.038** **	**-0.020**
	(-2.82)	**(-2.43)**	**(-1.50)**
The other control variables	Included	Included	Included
Subgroup coefficient difference in *AHPPost*$_{i,t}$ *(p-value)*		**(2)-(3)** **-0.018*** **(0.09)**	
Observations	2,368	1,150	1,218
Adjusted R-squared	0.833	0.841	0.827
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 9. Effects of an anti-hedging policy on CEO firm-specific wealth

This table presents the results of testing the effects of an anti-hedging policy on CEO firm-specific wealth for the matched and unmatched samples, respectively. We use two measures of CEO firm-specific wealth: (1) the CEO's firm-related wealth and (2) the CEO's net equity sales. T-statistics in parentheses are based on standard errors clustered by firms. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels for two-tailed tests, respectively. See Appendix B for definitions of variables.

Panel A. Effects of an anti-hedging policy on CEO firm-specific wealth for the propensity score-matched sample

	Dependent variable =	
	(1)	(2)
	$Firm\_Related\_Wealth_{i,t}$	$NetSales_{i,t}$
$Post_{i,t}$	0.017	0.000
	(0.38)	(1.33)
$\boldsymbol{AHPPost_{i,t}}$	**0.007**	**-0.000**
	(0.13)	**(-0.58)**
The other control variables	Included	Included
Observations	2,516	1,830
Adjusted R-squared	0.867	0.285
Firm-fixed Effects	YES	YES
Year-fixed Effects	YES	YES
SE clustered by firm	YES	YES

Panel B. Effects of an anti-hedging policy on CEO firm-specific wealth for the unmatched sample

	Dependent variable =	
	(1)	(2)
	$Firm\_Related\_Wealth_{i,t}$	$NetSales_{i,t}$
$\boldsymbol{AHPExist_{i,t}}$	**0.007**	**0.000**
	(0.41)	**(0.98)**
The other control variables	Included	Included
Observations	9,003	6,555
Adjusted R-squared	0.920	0.306
Firm-fixed Effects	YES	YES
Year-fixed Effects	YES	YES
SE clustered by firm	YES	YES

Table 10. Effects of an anti-hedging policy on risk-taking activities

This table presents the results of testing the effects of an anti-hedging policy on risk-taking activities for the matched and unmatched samples, respectively. We use five measures of risk-taking activities: (1) total investment, (2) research and development expenses, (3) acquisition, (4) capital expenditure and (5) leverage. T-statistics in parentheses are based on standard errors clustered by firms. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels for two-tailed tests, respectively. † indicates significance at the 10% level in one-tailed tests. See Appendix B for definitions of variables.

Panel A. Effects of an anti-hedging policy on risk-taking activities for the propensity score-matched sample

	Dependent variable =				
	(1)	(2)	(3)	(4)	(5)
	$Investment_{i,t}$	$RD_{i,t}$	$Acquisition_{i,t}$	$Capex_{i,t}$	$BLeverage_{i,t}$
$Post_{i,t}$	-0.121	0.125	-0.007	0.007	-0.016*
	(-0.12)	(0.93)	(-0.76)	(0.03)	(-1.84)
$\boldsymbol{AHPPost_{i,t}}$	**-0.876**	**-0.225**	**-0.006**	**0.038**	**0.001**
	(-1.06)	**(-1.24)**	**(-0.81)**	**(0.17)**	**(0.15)**
The other control variables	Included	Included	Included	Included	Included
Observations	2,516	2,516	2,516	2,516	2,510
Adjusted R-squared	0.377	0.928	0.114	0.768	0.739
Firm-fixed Effects	YES	YES	YES	YES	YES
Year-fixed Effects	YES	YES	YES	YES	YES
SE clustered by firm	YES	YES	YES	YES	YES

Panel B. Effects of an anti-hedging policy on risk-taking activities for the unmatched sample

	Dependent variable =				
	(1)	(2)	(3)	(4)	(5)
	$Investment_{i,t}$	$RD_{i,t}$	$Acquisition_{i,t}$	$Capex_{i,t}$	$BLeverage_{i,t}$
$\boldsymbol{AHPExist_{i,t}}$	**0.174**	**-0.167†**	**0.003**	**0.001**	**0.004**
	(0.37)	**(-1.57)**	**(0.78)**	**(0.01)**	**(0.75)**
The other control variables	Included	Included	Included	Included	Included
Observations	9,149	9,149	9,149	9,149	9,126
Adjusted R-squared	0.353	0.907	0.146	0.769	0.684
Firm-fixed Effects	YES	YES	YES	YES	YES
Year-fixed Effects	YES	YES	YES	YES	YES
SE clustered by firm	YES	YES	YES	YES	YES

Table 11. Effects of an anti-hedging policy on real earnings manipulation

This table presents the results of testing the effects of an anti-hedging policy on real earnings manipulation for the matched and unmatched samples, respectively. T-statistics in parentheses are based on standard errors clustered by firms. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels for two-tailed tests, respectively. See Appendix B for definitions of variables.

Panel A. Effects of an anti-hedging policy on real earnings manipulation for the propensity score-matched sample

	Dependent variable =
	$REM_{i,t}$
$Post_{i,t}$	0.018
	(0.18)
$\boldsymbol{AHPPost_{i,t}}$	**0.194****
	(2.40)
The other control variables	Included
Observations	2,475
Adjusted R-squared	0.162
Firm-fixed Effects	YES
Year-fixed Effects	YES
SE clustered by firm	YES

Panel B. Effects of an anti-hedging policy on real earnings manipulation for the unmatched sample

	Dependent variable =
	$REM_{i,t}$
$\boldsymbol{AHPExist_{i,t}}$	**0.071**
	(1.02)
Inverse Mills Ratio$_{i,t}$	1.190***
	(6.95)
The other control variables	Included
Observations	8,858
Adjusted R-squared	0.219
Firm-fixed Effects	YES
Year-fixed Effects	YES
SE clustered by firm	YES