



# Insider trading before accounting scandals

Anup Agrawal\*, Tommy Cooper

University of Alabama, Culverhouse College of Business, Tuscaloosa, AL 35487-0224, United States

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## ABSTRACT

We examine insider trading in a sample of more than 500 firms involved in accounting scandals revealed by earnings-decreasing restatements, and in a control sample of non-restating firms. Managers who sell stock while earnings are misstated potentially commit two crimes, earnings manipulation and insider trading, and their selling increases investor scrutiny and the likelihood of the manipulation being revealed. We examine the purchases, sales and net sales of five groups of corporate insiders during the misstated period and a pre-misstated period, using a difference-in-differences approach. Using several measures of the level of insider trading, we estimate cross-sectional regressions that control for other determinants of the level of insider trading. For the full sample of restating firms, we find weak evidence that top managers of misstating firms sell more stock during the misstated period than during the pre-misstated period, relative to the control sample. But in a number of subsamples where insiders had greater incentives to sell before the revelation of accounting problems, we find strong evidence that top managers of restating firms sell substantially more stock during the misstated period. These findings suggest that managers' desire to sell their stockholdings at inflated prices is a motive for earnings manipulation. Our finding that insiders brazenly trade on a crime for which they are potentially culpable suggests that insider trading is more widespread in the market than has been found in the prior literature.

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## 1. Introduction

Accounting scandals during the early 2000s at prominent companies such as Enron, HealthSouth, Tyco and Worldcom shook investor confidence. The rapid succession of high-profile scandals was soon followed by numerous other companies also disclosing accounting problems. The resulting investor concern about accounting problems is an often cited cause of the stock market slump that ensued (see, e.g., [Browning and Weil, 2002](#)). Accounting scandals are often accompanied by large stock-price declines, SEC investigations, top-management turnover, and bankruptcy filings (see, e.g., [Agrawal and Chadha, 2005](#); [Agrawal and Cooper, 2013](#); [Desai et al., 2006a](#); [Palmrose et al., 2004](#)). Many misreporting companies and their top executives face lawsuits from regulators and investors (see, e.g., [Palmrose and Scholz, 2004](#)).

An issue in the lawsuits against these executives is whether they traded corporate securities before accounting problems were revealed.<sup>1</sup> For example, in the trial of former Enron CEO Jeffrey Skilling, the U.S. government contended that Skilling grossed nearly \$63 million from Enron stock sales in 2000 and 2001, while holding material, non-public information about the company's fraud (see [Emshwiller, 2006](#)). Skilling was not the only Enron executive whose selling seems well-timed. In all, 29 Enron executives and

\* Corresponding author. Tel: +1 205 348 8970.

E-mail address: [aagrwal@cba.ua.edu](mailto:aagrwal@cba.ua.edu) (A. Agrawal).

<sup>1</sup> Throughout the paper, we use the terms financial misstatement, accounting manipulation, accounting problem and accounting scandal interchangeably. As discussed in [Section 4.1](#), we examine only restatements prompted by misstatements, i.e., material violations of generally accepted accounting principles (GAAP). Given the wide latitude that companies have in reporting earnings under GAAP, GAAP-violations represent serious accounting problems.

directors sold \$1.1 billion in Enron stock from 1999 to mid-2001, while gross financial misreporting supported a soaring stock price (see Wayne, 2002). Similarly, during a period of massive accounting fraud at HealthSouth, then-CEO Richard Scrushy sold his stockholdings for several hundred million dollars (see Freudenheim and Romero, 2003). Another example is Qwest Communications, where then-CEO Joseph Nacchio sold over \$100 million in stock while the company overstated earnings (see Young, 2005).

Spectacular cases of abuse such as the Enron and HealthSouth scandals led to public and media outcry about insider trading before the revelation of accounting problems (see, e.g., Countryman, 2004). This paper provides systematic evidence on whether insider trading before accounting scandals is the norm or the exception. This issue is important for at least four reasons. First, stock market participants want to know if insider trading is widespread because it affects investors' willingness to trade, and consequently affects the liquidity of the stock. Second, gauging the extent of insider trading is of interest to policy makers and regulators concerned about the effectiveness of existing insider trading regulations. Third, insiders are less likely to trade before accounting scandals than before other major corporate events such as takeovers, bankruptcies, stock buybacks, or equity issues. Managers who sell stock while earnings are misstated potentially commit two crimes: earnings manipulation and trading on material, non-public information. Furthermore, by selling stock, they attract investor scrutiny which increases the risk of the manipulation being revealed. A finding that insiders openly trade on a crime for which they are likely to be blamed<sup>2</sup> would suggest that insider trading is more widespread in the market than has been found in the previous literature. Fourth, the prevalence of insider trading also has implications for the design of optimal incentive compensation schemes, which assume that greater managerial stockholdings align managers' interests with those of stockholders. Given that short sales by managers are prohibited, greater managerial stockholdings have the unintended consequence of increasing managers' ability to profit (i.e., to avoid losses) from advance knowledge of bad corporate news by selling stock.

We analyze insider trading activity in a sample of 518 publicly traded U.S. companies that announced earnings-decreasing restatements during the period January 1997 – June 2002 to correct misstated financial statements, and in an industry-size matched sample of non-restating companies. We focus on restatements announced before the July 2002 adoption of the Sarbanes–Oxley Act (SOX) for three reasons. First, post-SOX, a large number of companies restated to 'clean house'; consequently, these cases tend to be less serious and have negligible average stock price reactions to their announcements (see, e.g., Agrawal and Cooper, 2013), reducing insiders' incentives to trade on them. Second, SOX section 304(a) requires a company's CEO and CFO to return any profits realized from the sale of company securities during the 12-month period following the first public issue or SEC filing of financial statements that are later restated due to a GAAP violation. This rule further reduces insiders' incentives to trade on financial misstatement post-SOX. Third, as Brochet (2010) argues, SOX and greater scrutiny from investors, media and regulators in the wake of the prominent accounting scandals that preceded SOX reduce the incentive of insiders to sell before disclosing bad news even more than their incentive to buy before disclosing good news. This argument is consistent with insider sales being more exposed to litigation and prosecution than insider purchases (see also Sale, 2002; Skinner, 1994).

Our sample includes restatements by prominent companies such as Adelphia, Best Buy, Enron, JDS Uniphase, K-Mart, Lucent Technologies, Rite-Aid, Worldcom, and Xerox. We focus on open-market stock transactions of five insider groups, all of whom are required to report all of their trades to the SEC on Forms 3, 4 and 5. These insider groups are top management, top financial officers, all corporate officers, board members, and blockholders. We examine their purchases, sales and net sales during the misstated period and a pre-misstated period using a difference-in-differences (DID) approach. We analyze five parametric and two non-parametric measures of the level of insider trading. Our analysis employs univariate tests and cross-sectional regressions that control for other potential determinants of the level of insider trading.

Prior studies find that stocks sold by insiders underperform stocks bought by them (see, e.g., Jaffe, 1974; Rozeff and Zaman, 1988; Seyhun, 1986). Seyhun (1988) finds that aggregate insider trading even predicts stock market movements. However, the extensive literature on insider trading before major corporate events presents somewhat mixed findings. Insiders appear to trade profitably before Chapter 11 bankruptcy filings, stock repurchases, seasoned equity offerings, earnings announcements, and dividend initiations (see, e.g., John and Lang, 1991; Karpoff and Lee, 1991; Lee et al., 1992; Penman, 1985; Seyhun and Bradley, 1997, respectively). But insiders appear to refrain from profitable active trading before other major events such as mergers (see Seyhun, 1990 for acquiring firms, and Agrawal and Jaffe, 1995; Agrawal and Nasser, 2012 for target firms).

Our study contributes to the literature on insider trading during earnings manipulation. Prior studies have examined insider trading during periods of earnings management and insider trading before SEC Accounting and Auditing Enforcement Releases (AAERs). As discussed in Section 3 below, the collective evidence from these studies points to abnormal insider selling during periods of earnings management, but the evidence on insider trading before SEC AAERs is mixed. To our knowledge, no prior empirical study provides a detailed examination of insider stock trades prior to the announcement of restatements to correct misstated earnings. This paper aims to fill this gap in the literature.

As discussed by Agrawal and Chadha (2005), earnings misstatements lie somewhere between earnings management and SEC enforcement actions in terms of the seriousness of the earnings manipulation. Earnings misstatements differ from typical earnings management in at least two respects. First, while earnings management appears to be practiced routinely at most public firms, a restatement is a rare event in the life of a company,<sup>3</sup> with serious consequences as discussed in the first paragraph in this section.

<sup>2</sup> This is akin to the possibility, investigated by the SEC, that terrorists traded stock or options before September 11, 2001 in industries directly affected by the attack such as insurance and airlines (see, e.g., Eisinger, 2001). Potesman (2006) finds some evidence of unusual trading before 9/11 in stock options on the two airlines targeted in the attacks.

<sup>3</sup> As we discuss in Section 4.1 below, there were a total of 919 restatements by U.S. public companies during a 5.5-year period beginning in January 1997. Based on a total of about 7000 public companies on Compustat per year, the annual probability of restatement by a public company works out to about 0.0239 (= 919 / (7000 \* 5.5)).

Second, the measurement of earnings management is largely an academic construct that provides no ‘smoking gun’, whereas earnings restatements are admissions by management that financial results were materially misstated. Most earnings misstatements also differ from cases where the SEC issues AAERs. Staff and resource constraints prevent the SEC from pursuing all cases of earnings manipulation. To have the greatest deterrent effect, the SEC’s Enforcement Division targets the most egregious violators and cases likely to generate greater media coverage (see, e.g., Agrawal and Chadha, 2005; Augustin et al., 2014; Dechow et al., 1996; Feroz et al., 1991). While less serious than cases warranting SEC AAERs, a study of earnings misstatements permits larger sample sizes and avoids issues of selection by the SEC.

For the full sample of restating firms, we find weak evidence that top managers of misstating firms sell more stock during the misstated period than during the pre-misstated period, relative to the control sample and after controlling for other determinants of the level of insider trading. But in a number of subsamples where insiders had greater incentives to liquidate their stockholdings before the revelation of accounting problems, we find strong evidence that top managers of restating firms sell substantially more stock during the misstated period. These subsamples include restatements that are more serious, lead to negative restated earnings, involve more than four misstated quarters, lead to larger declines in stock prices upon announcement, and result in greater dollar losses to insiders upon the announcement. The magnitude of abnormal selling by top managers of misstating firms is substantial. For example, in restatements whose announcements result in larger stock-price declines, there are, on average, about twice as many top managers of misstating firms selling in misstated periods than usual; their abnormal stock sales amount to about \$1 million in constant 2004 dollars in the average firm; and these abnormal sales represent about 24% of their stockholdings, a 125% increase relative to average stock sales by top managers over periods of equal length. Based on our finding of an average abnormal return of  $-12.5\%$  over days  $(-5, +1)$  around the restatement announcement, an abnormal stock sale of \$1 million avoids a loss of \$125,000. The results for top managers’ net sales ( $=$  sales  $-$  purchases) are generally similar; these results are not tabulated to save space.

Our analysis of insiders’ open-market stock transactions complements and extends in several ways studies of executive stock option exercises before restatement announcements (see, e.g., Burns and Kedia, 2008; Kedia and Philippon, 2009). First, executives exercising options are not really ‘cashing out’ unless they sell the resulting stock, a transaction not directly examined by studies of option exercises. Second, executives acquire stock not only through option exercises, but also via stock grants and stock bonuses received as compensation, and via open-market purchases. This study examines sales of stock acquired through all of these channels. Third, samples in studies of option exercises are limited to the top five executives of each sample firm. Our study covers a broader group of corporate insiders, namely all corporate officers (as defined in Section 4.3.2 below), board members, and blockholders. Fourth, while studies of option exercises focus on S&P 1500 firms, which are covered by the Execucomp database, our study covers all publicly traded U.S. companies using insider trading data from Thomson Financial. Finally, studies of option exercises likely miss company founders, who tend not to have stock options but have large stockholdings.<sup>4</sup> Our paper also complements recent studies of trading by other potentially informed investors, such as institutions, short sellers and financial intermediaries before restatement announcements.<sup>5</sup>

The remainder of this paper is organized as follows. Section 2 discusses the trade-offs an insider faces in deciding whether to sell stock while her firm is engaged in accounting manipulation. Section 3 briefly summarizes the existing empirical evidence on equity incentives and insider trading as motives for earnings manipulation. Section 4 describes our sample and data. Section 5 presents our results for the full sample. Section 6 presents the results for a number of subsamples where insiders have varying incentives to sell before the revelation of accounting problems. The final section concludes.

## 2. To sell or not to sell?

What is the trade-off an insider faces when deciding whether to sell stock while overstated financial results support an inflated stock price? Restatement announcements are typically followed by large declines in stock prices (see, e.g., Marciukaityte and Varma, 2008; Palmrose et al., 2004). So an insider’s benefit from selling equals the loss avoided by selling his stockholdings before the restatement announcement.

An insider’s cost of selling stock before the restatement announcement consists of three components. First, he stands to lose his job or directorship with the company. Second, he risks damaging his reputation and faces a reduction in future career prospects. Third, he faces possible civil and criminal penalties under the insider trading law. Section 10(b) of the Securities Exchange Act of 1934 and SEC rule 10(b)-5 prohibit corporate insiders from trading on the basis of material, non-public information.<sup>6</sup> Subsequent court rulings, such as the U.S. Supreme Court decisions in the *SEC vs. Texas Gulf Sulfur* (1969) and *Chiarella* (1980) cases, have buttressed this rule. The increased penalties under the Insider Trading Sanctions Act of 1984 and the Insider Trading and Securities Fraud Enforcement Act of 1988 have given the regulations more teeth.

<sup>4</sup> While we do not have data on the prevalence of founders among the group of insiders, it is likely significant given that the typical firm in our sample is relatively young and small (see Sections 4.1 and 4.3.1 below).

<sup>5</sup> See, e.g., Bodnaruk et al. (2009), Hribar et al. (2009), Desai et al. (2006b), Griffin et al. (2012), and Bernile et al. (2015).

<sup>6</sup> In addition, Section 16(c) of the Securities Exchange Act of 1934 prohibits registered corporate insiders (officers, directors, and beneficial owners of 10% or more of the company’s outstanding equity) from short-selling company stock. Insiders are allowed to sell stock they own as long as the sale does not result in a round-trip trade within a six-month period (under section 16(b) of the Act) and the insider does not possess material, non-public information at the time of the sale (under SEC rule 10b-5. See, e.g., Agrawal and Jaffe, 1995; Bainbridge, 1999). While the law does not prohibit insiders from buying put options, company rules usually ban such purchases, except for hedging purposes.

Thus, insiders face both costs and benefits from trading on accounting manipulation. As discussed in the introduction, the insiders we analyze are required to report their trades to the SEC. So the revelation of *any* profitable trading during the period of accounting manipulation is likely to impose substantial costs on them. On the other hand, their benefit from trading will vary depending on the seriousness of the manipulation, the stock price reaction to its revelation and the potential loss they can avoid by selling before the revelation. We expect that in cases where the manipulation is less serious, and the stock price decline and insider's potential loss upon the revelation are small, their benefit from selling is likely to be below its potential cost to them. Consequently, we expect them to refrain from selling in such cases. On the contrary, when the benefit from selling is large (e.g., in cases of more serious manipulation, greater stock price decline, and greater potential loss to insiders upon the revelation), the benefit likely exceeds the potential cost and we expect insiders to sell.<sup>7</sup>

Therefore, in addition to the full sample of restatements, we examine a number of subsamples where insiders' benefit from selling before the disclosure of accounting problems may be smaller (larger). These subsamples include restatements that are less (more) serious, result in positive (negative) restated earnings, result in smaller (larger) changes in reported earnings, involve less (more) restated quarters, result in smaller (larger) declines in stock prices upon announcement, or lead to smaller (greater) dollar losses to insiders upon announcement. We present detailed analyses of these subsamples in Section 6. Our findings shed light on insiders' expected net benefit from selling before the announcement of different types of restatements.

In addition, we analyze whether the identity of the initiator matters. In restatements initiated by the company, insiders are likely to be better informed about the upcoming announcement and its timing. They are more likely to sell in this case because they can time their stock sale well before the announcement. On the contrary, in restatements initiated by outside parties (e.g., auditors or the SEC), insiders may be surprised. They may not sell in this case because they are unable to time their sale sufficiently before the restatement announcement.

We examine trades by several groups of insiders. These include top management, top financial officers, other corporate officers, directors, and blockholders. Are all of these groups likely to be equally informed about the earnings manipulation and the forthcoming restatement? Probably not. One would expect the first two groups to have greater knowledge of the misstatement. But the other groups also are sufficiently close to the firm that they may become aware of it.<sup>8</sup>

### 3. Prior studies on earnings manipulation, equity incentives and insider trading

This section briefly summarizes the existing empirical evidence on equity incentives and insider trading as motives for earnings manipulation. Prior studies identify earnings manipulation in one of three ways: earnings management, SEC enforcement actions, and earnings restatements. We briefly discuss these strands of the literature in Sections 3.1 through 3.3.

#### 3.1. Earnings management

The earnings management literature suggests that firms with CEOs who have greater equity incentives are more likely to manage earnings, and that managers sell more shares while inflating earnings. Beneish and Vargus (2002) find that income-increasing accruals are significantly less persistent in firms with high insider selling. Periods of high accruals and large insider sales tend to be followed by periods with lower earnings and stock returns. At least two studies find that firms whose CEOs have greater equity incentives are more likely to manage earnings (as measured by abnormal accruals) or to be targeted by securities fraud allegations (see Bergstresser and Philippon, 2006; Denis et al., 2006). Feng et al. (2011) find that CEOs, but not CFOs, of manipulating firms have higher equity incentives than control firms. Their results suggest that CFOs mainly yield to pressure from CEOs motivated by equity incentives to engage in accounting manipulation.

In addition, during periods of high accruals, top executives sell more shares, and CEOs exercise more stock options than usual. Bartov and Mohanram (2004) find that managers appear to inflate earnings via accruals management before stock option exercises. Discretionary accruals and earnings changes are abnormally positive pre-exercise and reverse post-exercise. Using different measures of earnings management, Cheng and Warfield (2005) find that managers with greater equity incentives: (1) sell more stock in subsequent periods, (2) are more likely to report earnings that meet or barely exceed analysts' forecasts, and (3) are less likely to report positive earnings surprises in the current period, in an apparent attempt to avoid future earnings disappointments. Kraft et al. (2014) find that earnings management is more likely to occur when senior officers sell stock after a firm meets their voluntary earnings forecasts.

Bergstresser et al. (2006) investigate yet another avenue for managing earnings. They find that firms with defined benefit pension plans make more optimistic assumptions about these plans' future investment returns during periods when their managers are exercising stock options. Darrough and Rangan (2005) provide analogous evidence for initial public offerings (IPOs). They find that managers selling more of their shareholdings via the offering boost IPO-year earnings by reducing R&D spending and increasing discretionary accruals.

<sup>7</sup> This argument is supported by Aitken et al. (2015), who find that when exchange trading rules are more extensive and surveillance is greater, the insider trading that occurs tends to be more profitable.

<sup>8</sup> Even if an insider does not commit the accounting manipulation himself but is aware of it, he may be held responsible if he has direct authority or oversight over the individuals committing the manipulation. For example, if top management and board members know that the CFO is misreporting financial results, all of them may be held accountable.

### 3.2. SEC enforcement actions

A second strand of the literature identifies earnings manipulation via SEC enforcement actions against firms or their officers for committing egregious GAAP violations that overstated earnings. The SEC publishes details of these enforcement actions in AAERs. Three papers that examine insider trading during such earnings manipulation reach different conclusions. In a wide-ranging study on the causes and consequences of earnings manipulation, [Dechow et al. \(1996\)](#) present univariate results on insider sales for a sample of 70 firms subject to SEC enforcement actions during 1982–1992. They find no difference between the insider stock sales of manipulating firms and those of a control sample during the earnings-manipulation period. They examine the dollar value of stock sales by the CEO and by all officers and directors, scaled by the market value of equity at the beginning of the manipulation period. Using two different measures of insider selling, [Beneish \(1999\)](#) finds that insiders of earnings-manipulating firms are more likely to sell their stockholdings than control-firm insiders; managers of manipulating firms are also more likely to exercise stock options than managers of control firms. These conclusions are based on univariate tests on a sample of 54 firms subject to SEC enforcement actions during 1987–1993. Echoing Beneish's findings, [Johnson et al. \(2009\)](#) find that top executives at earnings-manipulating firms sell more stock and exercise more options during the fraud period than executives at control firms. Their conclusions are based on univariate tests on a sample of 53 firms subject to SEC enforcement actions for accounting frauds committed during 1992–2001.

[Thevenot \(2012\)](#) examines insider trading at firms that had accounting irregularities during 1997–2006. She finds that, on average, a 10 percentage point increase in the probability that a firm's accounting irregularities will lead to a class action lawsuit (SEC enforcement action) results in a \$24 (\$19) million reduction in insiders' net sales of the firm's stock during periods of intentional financial misstatements.

### 3.3. Earnings misstatements

The third strand of the literature identifies earnings manipulation via earnings misstatements. A number of studies examine whether high-powered CEO equity incentives make firms more or less likely to misreport earnings and provide mixed findings (see, e.g., [Armstrong et al., 2010](#); [Burns and Kedia, 2006](#)).<sup>9</sup> [Burns and Kedia \(2008\)](#) examine stock option exercises by top executives in a sample of 224 large (i.e., S&P 1500) firms that restated their financial statements during the period 1997–2002. In several subsamples of more serious misstatements (although not in the full sample), they find that executives of restating firms exercise significantly more options than executives of non-restating firms. In addition, they find that the magnitude of the restatement's effect on net income is positively related to the fraction of exercisable options that executives exercise. In a paper that examines several aspects of the economics of financial misreporting such as corporate hiring and investment levels in years surrounding the restated period, [Kedia and Philippon \(2009\)](#) briefly report the results of CEO option exercises for a sample of 140 firms in the S&P 1500 that restated during 1997–2002. They find that CEOs of restating firms exercise more options during the misreported period than CEOs of control firms.

The market appears to use insider trading to gauge the seriousness of misstatements. [Badertscher et al. \(2011\)](#) find that the magnitudes of negative stock price reactions to restatement announcements tend to be larger (smaller) for restatements that follow periods during which managers are net sellers (purchasers) of their firms' shares. This finding is consistent with the recent literature that uses the existence of reported insider trades as an indicator of the merit of a securities lawsuit (see, e.g., [Choi et al., 2009](#); [Johnson et al., 2007](#)).

## 4. Sample and data

[Section 4.1](#) details our sample selection procedure and describes the sample of restating firms. [Section 4.2](#) describes the stock-price reactions to the full sample of restatement announcements and a number of subsamples. [Section 4.3](#) deals with the selection of our cross-sectional control sample and compares the restating and control samples. [Section 4.4](#) describes our time-series control samples. [Section 4.5](#) describes the insider trading data and our measures of insider trading.

### 4.1. Sample of restating firms

Our sample of restating firms is obtained from the United States General Accounting Office ([GAO, 2002](#)), which lists 919 restatements of financial statements announced by 845 publicly traded firms during the period January 1, 1997 to June 30, 2002. These restatements correct prior misstatements, i.e., GAAP violations. Most of the restatements correct quarterly or annual financial statements filed with the U.S. Securities and Exchange Commission (SEC).<sup>10</sup> The GAO database excludes routine or technical restatements prompted by mergers and acquisitions, discontinued operations, stock splits, accounting rule changes, and changes in accounting method. We consider the seriousness of restatements in our sample in [Section 6.1](#) below.

<sup>9</sup> [Goldman and Slezak \(2006\)](#) develop an agency model in which stock-based compensation is a double-edged sword, inducing managers to exert productive effort but also to misrepresent firm performance.

<sup>10</sup> Fifteen cases in our sample are restatements of earnings releases and do not result in restatement of quarterly or annual financial statements. Omitting these cases does not change any of our results.



**Table 1**

Sample selection.

The table shows sample selection out of the 919 restatements listed in the GAO database, announced during January 1, 1997 and June 30, 2002.

		Number of firms
Total number of restatements in GAO database		919
– Repeat restatements	87	
– Firms not listed on CRSP	47	
– Firms delisted from CRSP more than one year before the restatement announcement	13	
– Firms with incomplete CRSP coverage	5	
– Firms whose listing on CRSP began less than nine months before the restatement announcement	23	
– Real estate investment trusts, exchange traded funds, and firms incorporated outside of the U.S.	62	
– Cases where a misstatement did not occur	2	
– Firms whose restatements do not decrease net income	162	
Number of restating firms in the final sample		518

Table 1 summarizes our sample selection procedure. Starting with the 919 restatements in the GAO database, we omit 87 cases of repeat restatements by sample firms.<sup>11</sup> In order to obtain a control sample and to construct several control variables (see Sections 4.3 and 5.1 below), we require that a restating firm be listed on the Center for Research in Security Prices (CRSP) database of the University of Chicago starting at least nine months before the restatement announcement. We also require sample firms to have at least two-thirds of the daily stock returns available over the one-year period prior to the announcement date. A total of 88 (= 47 + 13 + 5 + 23) firms do not satisfy these requirements. We omit an additional 62 cases where the restating firm is a real estate investment trust, exchange-traded fund, or is incorporated outside of the U.S. We also omit two cases where our review of news reports and SEC filings indicates that a misstatement did not occur. Finally, we omit 162 cases where the restatement did not decrease earnings because auditors and investors appear to view such restatements as less serious (see, e.g., Agrawal and Chadha, 2005).<sup>12</sup> Our final sample consists of 518 firms that announce earnings-decreasing restatements.<sup>13,14</sup> For each restatement in our sample, we collect data on the original earnings, restated earnings, and the number of quarters restated by reading news reports and the original and amending 10-Qs and 10-Ks filed with the SEC.<sup>15,16</sup> We obtain news reports from the ProQuest Newspapers database, Lexis-Nexis News Wires database, and press releases attached to 8-Ks filed with the SEC.

Table 2 describes our sample of 518 restating firms. Panel A summarizes the distribution of our sample by the identity of the initiator and by the nature of accounts restated. About 79% of the restatements in our sample are initiated by the company itself,<sup>17</sup> and about 7% by the outside auditor. Most of the remaining restatements are initiated by the SEC. About 63% of the cases involve only core accounts, about 21% involve only non-core accounts, and the remaining cases involve both.<sup>18</sup>

Panel B provides descriptive statistics of firm age and the magnitude of the change in earnings due to restatement. The median firm has been listed on CRSP (i.e., on NYSE, AMEX or Nasdaq) for about 6.5 years. The mean and median changes in earnings are about –234% and –33%, respectively; both are significantly different from zero, each with a p-value of 0.000.<sup>19</sup> The median firm restated 4 quarters of earnings. The median length of the misstated period, defined as the period from the beginning date of the first misstated quarter to the restatement-announcement date, is 586 days.

Panel C of Table 2 shows the distribution of the number of quarters restated. About 20% of the sample firms restate a single quarter's financial statements. About 55% of the firms restate four or fewer quarters, 19% restate five to eight quarters, and the remaining 26% restate nine or more quarters. Approximately 3% of the sample firms restate more than 20 quarters.

Panel D shows the industry distribution of the sample based on the first two digits of firms' primary SIC codes, using Song and Walkling's (1993) industry classification. For comparison, we report the industry distribution of the active CRSP population as of

<sup>11</sup> Our final sample of 518 restating firms includes 50 firms that announced multiple restatements during the sample period. Second and subsequent restatements by these 50 firms are not included in our sample. Furthermore, our results are qualitatively similar when we omit these 50 repeat violators from the sample.

<sup>12</sup> Nevertheless, for completeness, we separately analyze the sample of non-earnings-decreasing restatements. There is essentially no evidence of unusual insider trading either in the full sample of such restatements or its various subsamples corresponding to Section 6 below. To save space, we do not present these results in tables.

<sup>13</sup> Our sample includes 16 firms that were dissolved or terminated their registration with the SEC after the announcement but before any restatement.

<sup>14</sup> In a few instances, a firm listed in the GAO database restated its financial statements because the financial statements of a newly acquired subsidiary were misstated for fiscal years or quarters ending prior to the acquisition date. In such cases, we replace the acquiring firm with the subsidiary.

<sup>15</sup> In several instances, news reports and SEC filings indicate that the restatement was announced before the announcement date listed in the GAO database. Because we use the earlier announcement date in such cases, eight firms in our sample have announcement dates prior to January 1, 1997, the beginning date of the GAO database.

<sup>16</sup> In determining the beginning date of the misstated period, we take into account any adjustments made to retained earnings for prior periods. In addition, if a firm restates its financials for, say, the fiscal year ending December 2000, but the amended 10-K indicates that the restatement relates only to the last two quarters of the year, we define the beginning date of the misstatement as July 1, 2000.

<sup>17</sup> Following Palmrose et al. (2004), we assume that the initiator is the company, unless it is explicitly identified as another party in the GAO database, news reports or SEC filings.

<sup>18</sup> We classify as core restatements cases involving routine accounts such as sales revenue, cost of sales, selling, general and administrative expenses, accounts receivable, inventory, accounts payable, and certain accrued liabilities (e.g., accrued workers' compensation expense). We classify cases involving non-routine accounts and one-time or special items as non-core restatements. For restatements that affect income statement accounts, our definition of core restatements is very similar to that of Palmrose et al. (2004).

<sup>19</sup> Change in earnings is defined as (Restated earnings – Original earnings) / |Original earnings|.

**Table 2**

Sample distribution and descriptive statistics of restating firms.

Panels A through D show the frequency distribution, descriptive statistics, distribution of the number of quarters restated, and the industry distribution of the sample. The sample consists of publicly traded U.S. companies that announced financial statement restatements during the period January 1, 1997 to June 30, 2002. Restatements that did not affect net income or that increased net income are excluded from the sample. The list of restating firms was obtained from *Financial Statement Restatements: Trends, Market Impacts, Regulatory Responses, and Remaining Challenges* (Washington, D.C.: GAO-03-138).

Panel A: Distribution by Identity of Initiator and Nature of Accounts Restated				
<b>Initiated by</b>			<b>Number</b>	
Auditor			38	
Company <sup>a</sup>			409	
Regulators:				
SEC			56	
Others			3	
Multiple parties			12	
Total			518	
<b>Accounts restated</b>			<b>Number</b>	
Core			325	
Non-core			108	
Mixed			85	
Total			518	
	Mean	Median	Sample size	
Firm age since CRSP listing (years)	10.7	6.5	518	
Original earnings <sup>b</sup> (\$million)	100.3	1.9	502	
Restated earnings <sup>b</sup> (\$million)	– 55.4	– 0.9	502	
Change in earnings <sup>c</sup> (%)	– 233.7	– 32.6	518	
Number of quarters restated	6.3	4.0	518	
Length of misstated period <sup>d</sup> (days)	733	586	518	
Panel C: Distribution by the Number of Quarters Restated				
<b>Number of quarters restated</b>			<b>Number of firms</b>	
1			105	
2			39	
3			105	
4			37	
5–8			96	
9–12			58	
13–16			41	
17–20			22	
21–24			13	
25–28			0	
29–32			2	
Total			518	
Panel D: Industry Distribution				
Industry (SIC2 codes)	Sample		CRSP Population <sup>e</sup>	
	Number of firms	% of total	Number of firms	% of total
Agriculture (01-09)	0	0	15	0
Mining (10-14)	9	2	154	3
Construction (15-19)	3	1	54	1
Food and tobacco (20-21)	9	2	100	2
Textiles and apparel (22-23)	7	1	45	1
Lumber, furniture, paper, and print (24-27)	12	2	140	3
Chemicals (28)	35	7	365	7
Petroleum, rubber, and plastics (29-30)	6	1	70	1
Leather, stone, glass (31-32)	6	1	42	1
Primary and fabricated metals (33-34)	9	2	116	2
Machinery (35-36)	79	15	677	13
Transport equipment (37)	9	2	83	2
Instruments and miscellaneous manufacturing (38-39)	32	6	359	7
Transport, communications, utilities (40-49)	40	8	379	7
Wholesale trade (50-51)	24	5	203	4
Retail trade (52-59)	37	7	330	6
Finance, insurance, real estate (60-69)	60	11	1033	20
Hotels and personal services (70-71)	3	1	26	0
Services (72-89)	138	26	1043	20
Public administration and others (90-99)	0	0	1	0
Total	518	100	5235	100

(continued on next page)

Table 2 (continued)

Panel E: Time distribution		
Year of restatement announcement	Sample	
	Number of firms	% of total
1996 <sup>f</sup>	7	1
1997	51	10
1998	61	12
1999	92	18
2000	127	25
2001	117	22
2002	63	12
Total	518	100

<sup>a</sup> Includes 245 cases where the initiator was not identified in the GAO database.

<sup>b</sup> The sum of net income for all quarters affected by the restatement.

<sup>c</sup> Defined as  $(\text{Restated earnings} - \text{Original earnings}) / |\text{Original earnings}|$ . Both mean and median values of the change are significantly different from zero (p-value = 0.000 for each).

<sup>d</sup> The period from the first date of the first misstated quarter to the restatement announcement date.

<sup>e</sup> Industry distribution of active CRSP firms as of December 31, 2002.

<sup>f</sup> These firms announced restatements in 1996 (reported as 1997 in the GAO database).

December 31, 2002. In the sample (CRSP population), 26% (20%) of the firms are in services; 15% (13%) are in machinery manufacturing; 11% (20%) are in finance, insurance and real estate; and 8% (7%) are in transport, communications and utilities. The remaining 40% (40%) of the firms are distributed over 14 other industries.

Panel E of Table 2 shows the distribution of the sample by the year of restatement announcement. There is a sharp increase in the number of restatements announced starting in 1999. The data for 2002 is for the first half of the year. About 41% of the restatements in the sample were announced during 1996–1999, and the remaining 59% were announced during 2000–2002.

#### 4.2. Stock-price reaction

We next examine the stock-price reaction to restatement announcements. We compute the abnormal return for stock *i* on day *t* as:

$$e_{it} = r_{it} - r_{mt}, \quad (1)$$

where  $r_i$  and  $r_m$  are the stock returns for firm *i* and the market, respectively.<sup>20</sup> The market return is defined as the return on the CRSP (i.e., NYSE, AMEX and Nasdaq) equal-weighted stock index. The cumulative abnormal return for firm *i* over days ( $t_1, t_2$ ) is measured as

$$CAR_{t_1, t_2}^i = \sum_{t=t_1}^{t_2} e_{it}. \quad (2)$$

Table 3 shows mean CARs for the full sample of restating firms and its various subsamples over five windows covering trading days  $(-1, +1)$ ,  $(-5, +1)$ ,  $(-5, +5)$ ,  $(-20, +1)$  and  $(-20, +20)$  around the announcement date (day 0). The table also presents corresponding values for the control sample (discussed in Section 4.3 below) to examine a possible industry effect and to assess the validity of our matching procedure for identifying control firms.

Restatement announcements have large effects on stock prices. For the full sample of restating firms, the mean value of CAR ranges from  $-10.1\%$  over days  $(-1, +1)$  to  $-20.3\%$  over days  $(-20, +20)$ ; the mean CARs for all five event windows are significantly different from zero at the 1% level. The announcement CARs are more negative for the subsamples of more (versus less) serious restatements (discussed in Section 6.1 below), cases where the restated earnings are negative (versus non-negative), cases where the restatement causes a larger (versus smaller) percentage reduction in earnings, cases with more than four (versus four or fewer) restated quarters, and cases with lower (versus higher) returns to insiders. For each event window, the difference between the mean CARs of the two corresponding subsamples are highly statistically significant.

For the full sample of control firms, the mean CARs are statistically indistinguishable from zero over all five windows. This finding suggests that the restatement announcement has no discernible effect on similar-size firms in the restating firm's industry and that our matching procedure results in a reasonable control sample that does not experience a similar stock price decline. The mean CARs are also insignificantly different from zero at the 5% level for control firms in most subsamples, except for subsamples with more than four restated quarters, and over some event windows for subsamples with non-negative restated earnings and low returns to insiders. Even in these subsamples of control firms, the magnitudes of the CARs are substantially smaller than those for the corresponding restatement subsamples.

<sup>20</sup> As Brown and Warner (1985) point out, this simple market-adjusted model is quite adequate for computing short-run abnormal returns performs as well as other models such as the one-factor market model or CAPM.



**Table 3**

Mean announcement abnormal returns for restating and control firms over days (−20, +20).

This table shows the mean cumulative abnormal returns (CARs, %) of restating firms from 20 trading days before to 20 days after the announcement date. For each firm, the abnormal return for trading day *t* is computed by subtracting the return on the equal-weighted CRSP (i.e., NYSE, Nasdaq and AMEX) index from the return on a stock on day *t*. Both returns include dividends. Restatements are classified as less serious if they are triggered by SAB 101 or certain EITF consensuses, correct earnings releases, or involve only non-core accounts; the remaining cases are more serious. *Large (small)* and *high (low)* refer to the top (bottom) 40% of the full sample when ranked according to the subsample characteristic of interest. Restatement size is the absolute percentage change in reported earnings. High and low announcement return subsamples are based on CAR (−5, +5). The return to insiders is calculated by multiplying a firm's CAR (−5, +5) by the total dollar value of insider shareholdings before the misstated period. The column for sample size shows the number of observations for CAR (−20, +20). The superscripts a, b and c denote significantly different from zero at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

	Sample size	Restating firms Days around announcement					Control firms Days around announcement				
		(−1,+1)	(−5,+1)	(−5,+5)	(−20,+1)	(−20,+20)	(−1,+1)	(−5,+1)	(−5,+5)	(−20,+1)	(−20,+20)
Full sample	386	−10.1 <sup>a</sup>	−12.5 <sup>a</sup>	−12.6 <sup>a</sup>	−17.2 <sup>a</sup>	−20.3 <sup>a</sup>	−0.4	−1.1	−1.0	−1.5	−1.2
Less serious cases	143	−4.2	−5.3	−3.9 <sup>b</sup>	−7.8	−7.4 <sup>a</sup>	−0.6	−1.8 <sup>c</sup>	−1.6	−3.7 <sup>c</sup>	−3.9
More serious cases	243	−13.5 <sup>a</sup>	−16.6 <sup>a</sup>	−17.6 <sup>a</sup>	−22.7 <sup>a</sup>	−27.9 <sup>a</sup>	−0.3	−0.6	−0.5	−0.3	0.4
p-value for the difference		0.000	0.000	0.000	0.000	0.000					
Non-negative restated earnings	184	−6.5 <sup>a</sup>	−7.9 <sup>a</sup>	−8.3 <sup>a</sup>	−10.8 <sup>a</sup>	−13.4 <sup>a</sup>	−0.9	−2.6 <sup>a</sup>	−2.1 <sup>b</sup>	−1.6	−0.2
Negative restated earnings	202	−13.2 <sup>a</sup>	−16.4 <sup>a</sup>	−16.3 <sup>a</sup>	−22.9 <sup>a</sup>	−26.6 <sup>a</sup>	0.1	0.3	0.1	−1.4	−2.1
p-value for the difference		0.000	0.000	0.003	0.000	0.001					
Small restatements	155	−8.2 <sup>a</sup>	−9.4 <sup>a</sup>	−7.9 <sup>a</sup>	−13.0 <sup>a</sup>	−13.1 <sup>a</sup>	−0.9	−1.9 <sup>c</sup>	−1.3	−1.5	1.1
Large restatements	150	−14.8 <sup>a</sup>	−19.0 <sup>a</sup>	−18.2 <sup>a</sup>	−23.7 <sup>a</sup>	−27.6 <sup>a</sup>	−0.2	−0.2	0.2	−2.0	−3.0
p-value for the difference		0.005	0.001	0.001	0.002	0.002					
≤4 restated quarters	225	−8.3 <sup>a</sup>	−9.7 <sup>a</sup>	−9.1 <sup>a</sup>	−12.1 <sup>a</sup>	−15.2 <sup>a</sup>	0.2	0.1	0.1	0.7	0.8
>4 restated quarters	161	−12.6 <sup>a</sup>	−16.3 <sup>a</sup>	−17.4 <sup>a</sup>	−24.3 <sup>a</sup>	−27.5 <sup>a</sup>	−1.2 <sup>b</sup>	−2.7 <sup>a</sup>	−2.5 <sup>b</sup>	−4.6 <sup>a</sup>	−3.9 <sup>c</sup>
p-value for the difference		0.032	0.008	0.004	0.000	0.005					
High announcement returns	158	0.3	3.6 <sup>a</sup>	8.5 <sup>a</sup>	−1.5	−0.1	0.6	0.0	−0.1	−0.8	−1.2
Low announcement returns	152	−23.7 <sup>a</sup>	−31.5 <sup>a</sup>	−36.7 <sup>a</sup>	−38.3 <sup>a</sup>	−45.8 <sup>a</sup>	−1.2 <sup>c</sup>	−1.7	−1.7	−2.1	−1.9
p-value for the difference		0.000	0.000	0.000	0.000	0.000					
High returns to insiders	157	−4.4 <sup>a</sup>	−2.5	−0.1	−7.8 <sup>a</sup>	−8.2 <sup>b</sup>	0.1	−0.4	0.0	−1.5	−2.1
Low returns to insiders	152	−16.3 <sup>a</sup>	−22.3 <sup>a</sup>	−25.3 <sup>a</sup>	−26.9 <sup>a</sup>	−31.2 <sup>a</sup>	−1.2 <sup>c</sup>	−2.0 <sup>b</sup>	−2.1 <sup>c</sup>	−1.9	−0.1
p-value for the difference		0.000	0.000	0.000	0.000	0.000					

**Table 4**

Descriptive statistics of restating and control firms.

The table shows the mean and median values for matched samples of restating and control firms and tests for differences between the two groups. The restatement sample consists of 518 publicly traded U.S. firms that announced earnings-decreasing restatements during the period January 1, 1997 to June 30, 2002, as identified by the GAO database. Each restating firm is matched with a control firm that has the closest size (i.e., market capitalization one year before the announcement date of the restatement) from among all firms in its industry that did not restate their financial statements during the period January 1, 1995 to June 30, 2002. All dollar values have been adjusted for inflation and converted to 2004 dollars.

Variable	Mean			Median			Sample size
	Restate	Control	p-value <sup>a</sup>	Restate	Control	Wilcoxon p-value <sup>b</sup>	
<i>Panel A: General firm characteristics</i>							
Firm size <sup>c</sup> :							
Sales (\$million)	1217	986	0.104	90	100	0.549	443
Total assets (\$million)	1689	1361	0.019	112	120	0.177	443
Market value of equity (\$million)	2645	2390	0.175	158	145	0.520	383
Firm value <sup>d</sup> (\$million)	3981	3458	0.026	283	264	0.019	383
Number of employees ('000s)	6.3	4.9	0.098	0.6	0.7	0.604	392
Operating performance: (%)							
OPA <sup>e</sup> (−1)	−2.29	−0.50	0.418	5.86	7.72	0.001	436
OPA <sup>e</sup> (−2)	−5.67	−3.67	0.596	5.71	7.83	0.010	401
OPA <sup>e</sup> (−3)	−0.66	−1.18	0.842	6.96	6.69	0.785	327
OPA <sup>f</sup>	0.29	0.04	0.905	6.46	6.93	0.146	327
Growth:							
Sales growth rate <sup>g</sup> (%)	22.59	18.64	0.109	15.76	11.29	0.056	193
Firm value/total assets	2.78	2.76	0.952	1.62	1.65	0.544	383
B/M <sup>h</sup>	0.53	0.55	0.583	0.39	0.40	0.959	383
Financial leverage:							
Long-term debt/total assets	0.17	0.17	0.857	0.11	0.11	0.854	443
Long-term debt/firm value	0.13	0.12	0.294	0.07	0.06	0.560	383
Stock volatility and prior returns: (%)							
$\sigma_s^i$	4.09	4.17	0.590	3.73	3.52	0.747	324
$\Delta\sigma_s^j$	0.36	0.29	0.562	0.18	0.10	0.137	324
RET <sub>−1</sub> <sup>k</sup>	0.00	0.11	0.003	−0.02	0.02	0.021	366
RET <sub>−2</sub> <sup>k</sup>	0.05	0.02	0.333	0.01	0.00	0.178	350
RET <sub>−3</sub> <sup>k</sup>	0.06	0.05	0.787	0.02	0.00	0.584	330
RET <sub>−4</sub> <sup>k</sup>	−0.02	0.04	0.115	−0.02	−0.02	0.513	314
<i>Panel B: Shareholdings<sup>l</sup></i>							
Top management							
# of individuals	1.7	1.7	0.639	1	1	0.978	518
# shares	1.4	1.9	0.472	0.1	0.1	0.959	518
\$ shares	26.0	45.6	0.323	1.5	1.6	0.656	518
% of outstanding equity	7.0	8.3	0.379	0.6	0.6	0.404	518
Top financial officers							
# of individuals	0.9	0.9	0.787	1	1	0.706	518
# shares	0.1	0.1	0.795	0.0	0.0	0.850	518
\$ shares	2.5	4.1	0.299	0.0	0.0	0.941	518
% of outstanding equity	1.0	0.7	0.466	0.0	0.0	0.513	518
All officers							
# of individuals	5.6	5.6	0.976	5	4	0.526	518
# shares	1.4	1.8	0.522	0.2	0.2	0.783	518
\$ shares	28.0	49.5	0.290	2.8	2.8	0.893	518
% of outstanding equity	6.9	7.2	0.749	1.2	1.0	0.575	518
All directors							
# of individuals	5.3	5.1	0.251	5	4	0.243	518
# shares	1.7	2.1	0.542	0.3	0.3	0.615	518
\$ shares	31.0	54.0	0.251	3.8	3.9	0.829	518
% of outstanding equity	8.7	9.4	0.562	1.8	1.9	0.370	518
Blockholders							
# of individuals	0.8	0.9	0.363	0	0	0.825	518
# shares	4.1	3.0	0.198	0	0	0.519	518
\$ shares	132.3	80.1	0.413	0	0	0.597	518
% of outstanding equity	18.2	15.6	0.418	0	0	0.865	518

<sup>a</sup> For the matched pairs *t*-test (2-tailed).<sup>b</sup> For the Wilcoxon signed-ranks test (2-tailed).<sup>c</sup> As of the end of or for the last fiscal year ended before the beginning date of the misstated period.<sup>d</sup> Firm value = Book value of total assets − Book value of equity + Market value of equity.<sup>e</sup> OPA(*t*) = Operating performance to total assets for year *t* relative to the beginning of the misstated period = Operating income after depreciation / Total assets.<sup>f</sup> OPA = [OPA(−3) + OPA(−2) + OPA(−1)] / 3.<sup>g</sup> Sales growth rate = [Sales(−1) / Sales(−5)]<sup>1/4</sup> − 1.<sup>h</sup> Book value of equity divided by the market value of equity as of the last fiscal year ended before the beginning of the misstated period.<sup>i</sup> Standard deviation of daily stock returns for the period (−250, −126) trading days relative to the beginning of the misstated period.<sup>j</sup> Equals the standard deviation of the firm's daily stock returns for the period (−125, −1) trading days relative to the beginning of the misstated period minus  $\sigma_s$ .<sup>k</sup> The mean daily stock return over quarter *t* minus the corresponding mean return on the market.<sup>l</sup> Shareholdings of the group indicated (as defined in Table 5) prior to the first quarter restated. #Shares and \$Shares are in millions.

### 4.3. Cross-sectional control sample

We compare the insider trading activity of restating firms to that of a cross-sectional control group. The restating and control firms are matched by size (market value of equity from CRSP)<sup>21</sup> and industry one year before the announcement date of the restatement.<sup>22</sup> Specifically, we match each restating firm with a control firm incorporated in the U.S. that has the same two-digit primary SIC industry code, is the closest in size, and did not announce a restatement during the period January 1, 1995 to June 30, 2002.

The insider trading activity of each restating firm and its matched control firm is observed during the restating firm's misstated period. We use the beginning date of the first quarter restated as our estimate of when a restating firm began misstating its financial statements. The misstated period extends from the first day of the first quarter restated to the announcement date of the restatement. We discuss general characteristics of restating and control firms in Section 4.3.1 and insider shareholdings in Section 4.3.2.

#### 4.3.1. Characteristics of restating and control firms

Panel A of Table 4 shows characteristics of our restating and control samples. All dollar values reported in the paper are in inflation-adjusted 2004 dollars. All firm-size measures are as of or for the last fiscal year ended before the misstated period. The typical restating firm in our sample is relatively small compared to the typical firm traded on the major U.S. stock markets. The median sales of restating (control) firms are about \$90 million (\$100 million). Their market capitalization is about \$158 million (\$145 million), and they employ about 600 (700) people. None of these differences between restating and control firms is statistically significant.

The median operating performance of restating firms is significantly lower than that of control firms over each of the two fiscal years ending prior to the misstated period. The median ratio of operating performance to assets (OPA) for the restating (control) sample is about 5.9% (7.7%) in year  $-1$  and 5.7% (7.8%) in year  $-2$ . At 15.8%, the median four-year sales growth rate of restating firms is higher than the 11.3% growth rate of control firms. This difference has a p-value of .056. But other measures of growth opportunities, such as the ratio of firm value to total assets and the ratio of book value to market value of equity, are similar for the two groups of firms.<sup>23</sup> The two groups also have similar financial leverage ratios. For example, the median ratio of long-term debt to total assets for both groups is about 11%.

Panel A of Table 4 also shows daily stock volatility ( $\sigma_s$ ) measured over trading days ( $-250, -126$ ) relative to the beginning of the misstated period, and the change in this volatility over the subsequent 125 trading days ( $\Delta\sigma_s$ ). The two samples have similar stock volatilities and changes in volatilities. Median  $\sigma_s$  for our restating (control) sample is 3.73% (3.52%),<sup>24</sup> and median  $\Delta\sigma_s$  is 0.18% (0.10%). Differences in the mean and median values of  $\sigma_s$  and  $\Delta\sigma_s$  between the restating and control samples are statistically insignificant.

Panel A also reports the mean daily abnormal return for each of the four three-month periods immediately prior to the beginning date of the misstated period for the restating and control samples. The abnormal return on a stock equals its mean return minus the mean return on the equal-weighted CRSP (NYSE, AMEX and Nasdaq) market index over the relevant period. The mean abnormal return in quarter  $-1$ , denoted  $PRET_{-1}$ , is significantly lower for restating firms than for control firms: 0.00% and 0.11%, respectively. For quarters  $-2$  through  $-4$ , the abnormal returns are similar across the two samples.

#### 4.3.2. Insider shareholdings

In Panel B of Table 4, we report the shareholdings of five groups of corporate insiders. *Top management* consists of the Chairman, Chief Executive Officer (CEO), Chief Operating Officer (COO), and President. *Top financial officers* are the Chief Financial Officer (CFO), Controller and Treasurer. *All officers* are all corporate officers as defined by the SEC under Section 16a of the Securities Exchange Act of 1934.<sup>25</sup> *All directors* are all members of the company's board of directors. Finally, *blockholders* are beneficial owners of 10% or more of the company's outstanding equity. For each group, we report the mean and median group size and shares owned (number, dollar value, and percentage of outstanding equity).

The median number of top managers is one for both restating and control firms. The median value of their stockholdings in restating (control) firms is \$1.5 million (\$1.6 million); the mean value is much larger, \$26.0 million (\$45.6 million). Top managers own a median of 0.6% (0.6%) of the outstanding equity; the corresponding mean value is 7.0% (8.3%). Top financial officers own a mean of \$2.5 million (\$4.1 million) of stock, but this group owns essentially no stock in over one-half of restating (control) firms.

The median all-officers group consists of 5 (4) individuals in restating (control) firms and owns a median of \$2.8 million (\$2.8 million) of stock; its mean ownership is substantially larger, \$28.0 million (\$49.5 million). The median board consists of 5 (4) individuals in restating (control) firms, who collectively own about 1.8% (1.9%) of the outstanding equity. The median board's stock ownership is \$3.8 million (\$3.9 million) in restating (control) firms; the mean value is \$31.0 million (\$54.0 million). Finally, the mean number of blockholders in restating (control) firms is 0.8 (0.9), and the group's mean stock ownership is \$132.3 million (\$80.1 million). The blockholders group owns a mean of 18.2% (15.6%) of the restating (control) firms' outstanding equity. The average ownership of the

<sup>21</sup> We include all publicly traded common share classes when calculating market value of equity.

<sup>22</sup> We match firms one year before the announcement date because the stock of a restating firm can drop before the restatement announcement due to possible information leakage. Agrawal and Chadha (2005, Fig. 1) examine abnormal long-run stock returns over months ( $-24, +24$ ) around the month of restatement announcement (month 0). They find that the stock of restating firms starts to drop starting in month  $-3$ , but find no evidence of a systematic drop before that. Some restating firms were not listed on CRSP one year before the announcement date. For these firms, the matching date is the restating firm's first trading day on CRSP. We exclude restating firms whose beginning date on CRSP is less than nine months before the announcement date.

<sup>23</sup> Firm value equals the book value of total assets minus the book value of equity plus the market value of equity.

<sup>24</sup> The corresponding annual volatility, found by multiplying the daily volatility by  $\sqrt{252}$ , is 59.21% (55.88%).

<sup>25</sup> This group includes top management, principal financial officer, principal accounting officer, vice presidents in charge of principal business units, divisions or functions, and any other person who performs a policy-making function for the company.

blockholders group seems especially large, given that more than one-half of the firms in each sample have no blockholders (i.e., owners of 10% or more of the outstanding equity). None of the differences in ownership between the two samples is statistically significant.

#### 4.4. Time-series control sample

We compare the levels of insider trading in restating and control firms during the misstated period to their levels before the misstated period. The length of this time-series control, or pre-misstated period, equals that of the misstated period.<sup>26</sup> To obtain the beginning of the pre-misstated period, the number of days in the misstated period is subtracted from the first day of the misstated period. We do not examine insider trading after the restatement announcement because insiders' actions are under a spotlight during that period, so the level of insider trading is unlikely to be normal.

#### 4.5. Insider trading data

Insider trading data is obtained from the Thomson Financial Insider Filing Data Files (hereafter, TF). TF reports ownership, insider transactions and changes in ownership that insiders report on Forms 3, 4, and 5 filed with the SEC.<sup>27</sup> For each restating and control firm, we obtain open-market purchases and sales by insiders during the misstated and pre-misstated periods.<sup>28</sup> We employ five parametric measures of the level of insider trading during the misstated and pre-misstated periods: number of insiders trading, number of shares traded, dollar value of shares traded, percentage of equity traded, and percentage of insider shareholdings traded.<sup>29</sup> In addition, we use two non-parametric measures: the percentage of sample companies with at least one or at least two insiders trading during the period. The dollar value of shares traded is computed by multiplying the number of shares traded by the transaction price reported by TF. If TF does not report the transaction price, we use the closing share price on the transaction date reported by CRSP. Prices are adjusted for inflation and converted to 2004 dollars. The percentage of equity traded equals the number of shares traded divided by the number of shares outstanding on the transaction date. To obtain the percentage of insider shareholdings traded, shares purchased are divided by the resulting shares held, and shares sold are divided by shares held prior to the sale.

### 5. Full sample results

We next estimate cross-sectional regressions to control for other determinants of the normal level of insider trading. Section 5.1 discusses our regression specification. We present the results for top management in Section 5.2. Section 5.3 presents the results for each of the five insider groups for the full sample and for subsamples partitioned by returns at the announcements of the restatements. For brevity, we report the results on insider sales. The results for net sales are similar, unless noted otherwise.

#### 5.1. Regression specification

Prior studies find that the level of insider trading is affected by several variables such as firm size, the level and change in stock volatility, prior stock returns, insider shareholdings, firm valuation, and innovation. In addition, one would expect the level of insider trading before the announcement of an event to depend upon the potential effect of the announcement on stock prices. We control for these variables via regressions of the level of insider sales.<sup>30</sup> Our measures of each of these control variables are also described below.

##### 5.1.1. Firm size

Seyhun (1986) finds that insiders at small firms are net purchasers on average, while insiders at large firms are net sellers. We measure firm size as the natural logarithm of market capitalization, denoted  $\ln(\text{Market cap})$ . Market capitalization equals the number of common shares outstanding multiplied by the stock price. Stock price and number of shares outstanding are obtained from CRSP as of the last trading day during the fiscal year ending before the misstated or pre-misstated period.

<sup>26</sup> The length of the misstated and pre-misstated periods is not equal for one firm whose pre-misstated period starts before January 1, 1986, the beginning date of our insider trading data.

<sup>27</sup> Most insider transactions are reported on Form 4. Form 3 is the initial statement of beneficial ownership that insiders must file. Form 5 is an annual statement of changes in beneficial ownership and contains activity from small or exempt transactions that are not reported on Form 4.

<sup>28</sup> We review the TF database for obvious coding and transposition errors and make corrections where appropriate. We remove transactions that are amended by subsequent filings, and transactions involving shares indirectly owned by insiders via a partnership, corporation, trust or other entity.

<sup>29</sup> Shareholdings data are from Form 3 reports as contained in TF. We have tried to correct for obvious coding errors in this data. Despite that, this data may be noisy because it is not reported consistently. So the results on percentage holdings should be interpreted cautiously.

<sup>30</sup> We also explore whether the level of insider sales is affected by a firm's level of antitakeover protection, as measured by Gompers et al. (2003) governance (G) index. Data on the G index is available only for the subset of our sample that belongs to the S&P 1500. When we add the G index to the regressions in Tables 5 to 7, its coefficient estimate is statistically insignificant in all of the regressions, with test statistics ranging from  $-0.09$  to  $-0.94$  in the Table 5 regressions. The addition of this variable reduces the sample sizes in the regressions dramatically and reduces the power of our tests; e.g., for the full sample regressions in Table 5, the sample size drops from 728 to 208. Despite this, our main results remain qualitatively similar to those reported in Tables 5 to 7 below. For brevity, we do not tabulate these results.

### 5.1.2. Stock return volatility ( $\sigma_s$ )

Meulbroek (2000) finds that managers in more risky companies tend to sell equity more aggressively. We measure risk using the standard deviation of stock returns for the period from 250 to 126 days before the beginning of the relevant misstated or pre-misstated period.<sup>31</sup>

### 5.1.3. Change in stock return volatility ( $\Delta\sigma_s$ )

Demsetz and Lehn (1985), Aggarwal and Samwick (1999, 2003), and Jin (2002) show theoretically and empirically that managers' equity holdings are determined by optimal contracting considerations. Their findings imply that changes in equity risk should induce changes in managers' holdings via stock purchases or sales. We measure the change in stock return volatility by subtracting  $\sigma_s$  (defined above) from the standard deviation of the firm's daily stock returns for the period from 125 days to 1 day before the relevant misstated or pre-misstated period.

### 5.1.4. Prior stock return ( $PRET_{-t}$ )

Lakonishok and Lee (2001) find that insiders are contrarian investors who buy (sell) stock with poor (good) past performance. We control for prior stock returns using the average daily stock return for each of the four 3-month periods prior to the beginning of the misstated or pre-misstated period, denoted  $PRET_t$  for quarter  $t$ ,  $t = -1$  to  $-4$ . The daily stock returns are market-adjusted by subtracting the daily return on the CRSP (NYSE, AMEX and Nasdaq) equal-weighted market index.

### 5.1.5. Insider shareholdings (holdings)

Ofek and Yermack (2000) find that executives with large shareholdings sell stock after receiving new equity incentives. The stock sales are attributed to executives seeking portfolio diversification. We use the direct shareholdings last reported by insiders prior to the relevant misstated or pre-misstated period.

### 5.1.6. Book-to-market decile (B/M decile)

Jenter (2005) finds that insiders tend to be contrarian investors who buy a stock when it is selling at a low valuation, and sell it when it has a high valuation. Book-to-market (B/M) decile is our measure of a firm's valuation ratio relative to other firms. We calculate the B/M ratio by dividing the book value of stockholders' equity by the market capitalization of common shares outstanding. Each firm's B/M ratio is assigned to a decile using NYSE decile breakpoints for the applicable year.<sup>32</sup> Each firm is assigned a value of 1 through 10 based on its assigned B/M decile.

### 5.1.7. R&D/sales

Aboody and Lev (2000) argue that research and development (R&D) activities increase the information asymmetry between insiders and outsiders, thereby allowing insiders to reap greater profits on their trades. Their finding implies that insiders will trade more in firms with greater R&D expenses. We divide R&D expense by sales revenue for the fiscal year. R&D/Sales equals zero for firms whose R&D expenses are not reported by Compustat.

### 5.1.8. $CAR_{-5,+5}$

The greater the potential effect of a restatement announcement on stock price, the greater is an insider's incentive to trade before the announcement. We measure the potential stock-price effect of a restatement announcement as the cumulative abnormal stock return over days  $-5$  to  $+5$  around the announcement (denoted  $CAR_{-5,+5}$ ), as defined in Eq. (2) in Section 4.2 above.

We construct the explanatory variables using stock-price data from CRSP and financial statement data from Compustat. Financial statement data are for the last fiscal year ending prior to the relevant misstated or pre-misstated period. To be included in the regressions, we require that two observations (one before the misstated period, the other before the pre-misstated period) be available for all explanatory variables for both the restating firm and the matched control firm. Accordingly, the regression includes observations pooled from these four matched samples. We estimate the following equation:

$$\begin{aligned} IT_i = & \beta_0 + \beta_1 \ln(\text{Market cap})_i + \beta_2 \sigma_{si} + \beta_3 \Delta\sigma_{si} + \beta_4 PRET_{-1i} + \beta_5 PRET_{-2i} + \beta_6 PRET_{-3i} \\ & + \beta_7 PRET_{-4i} + \beta_8 \text{Holdings}_i + \beta_9 \text{B/M decile}_i + \beta_{10} \text{R\&D/Sales}_i + \beta_{11} CAR_{-5,+5,i} \\ & + \beta_{12} \text{MPERIOD}_i + \beta_{13} \text{RESTATER}_i + \beta_{14} \text{MPERIOD}_i * \text{RESTATER}_i + \varepsilon_i, \quad i = 1, 2, \dots \end{aligned} \quad (3)$$

where  $IT$  is one of the five measures of insider trading: #Ins,  $\ln(1 + \#Sh)$ ,  $\ln(1 + \$Sh)$ , %Eq, or %Hold. #Ins is the number of insiders trading shares during a given period. #Sh, \$Sh, %Eq, and %Hold are the number of shares, dollar value of shares, percentage of outstanding shares, and percentage of direct shareholdings, respectively, traded by insiders during the period. MPERIOD is a binary dummy variable equal to 1 (0) if the insider trading activity occurs during the misstated (pre-misstated) period. RESTATER is a binary variable equal to 1 (0) if the firm is a restating (control) firm. The error term is  $\varepsilon$ . All other variables are defined above. The first dependent variable used in the regressions is the number of insiders (#Ins) buying or selling shares during the period of interest. This variable takes integer values from 0 to 10 in most cases. For example, the last two columns in Panel A of Table 5 show that the number of top managers of restating

<sup>31</sup> We require that at least two thirds of the daily stock returns over this period be available on CRSP. We impose the same requirement when calculating the average daily stock returns for a period.

<sup>32</sup> The NYSE breakpoints were obtained from Professor Kenneth French's website: <http://mba.tuck.dartmouth.edu/pages/faculty/ken.french>.



**Table 5**

Regressions of insider sales for top management.

This table shows coefficient estimates from regressions of measures of insider trading of top management (Chairman, Chief Executive Officer, Chief Operating Officer, and President) on several explanatory variables. The sample consists of 1) 518 publicly traded U.S. firms that announce earnings-decreasing restatements during the period January 1, 1997 to June 30, 2002, and 2) 518 industry-size matched control firms that do not announce restatements during this period. There are two observations for each restating and control firm: one measures insider trading activity during the misstated period; the other measures it during the pre-misstated period. The misstated period is the period from the first day of the first quarter restated to the announcement date of the restatement. The number of days in the misstated period is subtracted from the first day of the misstated period to obtain the beginning of the pre-misstated period. #Ins is the number of insiders trading shares during the misstated or pre-misstated period. #Sh, \$Sh, %Eq, and %Hold are the number of shares, dollar value of shares, percentage of outstanding shares, and percentage of direct shareholdings, respectively, traded by insiders during the misstated or pre-misstated period. #Sh is in '000s, and \$Sh is in millions. Observations of #Sh, \$Sh, %Eq, and %Hold are annualized. All dollar values are in inflation-adjusted 2004 dollars. The standard deviation of daily stock returns ( $\sigma_s$ ) is computed over trading days ( $-250, -126$ ) relative to the beginning of the misstated or pre-misstated period. The change in standard deviation ( $\Delta\sigma_s$ ) equals the standard deviation of the firm's daily stock returns computed over trading days ( $-125, -1$ ) relative to the misstated or pre-misstated period minus  $\sigma_s$ .  $RET_{-t}$  is the average daily stock return over quarter  $t$  minus the return on the equal-weighted CRSP (NYSE, AMEX and Nasdaq) market index. Holdings are the total number of shares, dollar value of shares, or percentage of outstanding equity directly owned by the insider prior to the misstated period. B/M decile equals 1 through 10 depending on the firm's book-to-market ratio as of the last fiscal year ending prior to the misstated or pre-misstated period. NYSE B/M decile breakpoints are used to assign B/M deciles. Market capitalization,  $M$ , equals the number of common shares outstanding times the share price as of the last fiscal year ending prior to the misstated period. R&D/Sales is R&D expense divided by sales revenue; the ratio is calculated using the income statement for the last fiscal year ending prior to the misstated or pre-misstated period.  $CAR_{-5,+5}$  is the cumulative abnormal return on the firm's stock for the period beginning 5 trading days before and ending 5 trading days after the announcement date. MPERIOD is a dummy variable equal to 1 if the insider trading activity occurs during the misstated period and equal to 0 otherwise. RESTATER is a dummy variable equal to 1 if the firm is a restating firm and equal to 0 otherwise. Regressions of #Ins use the negative binomial model, regressions of  $\ln(1 + \#Sh)$  and  $\ln(1 + \$Sh)$  use the single-censored Tobit model, and regressions of %Eq and %Hold use the double-censored Tobit model. The top and bottom 1% of the observations on the dependent and independent variables, except for B/M decile, MPERIOD, RESTATER, and the interaction term, have been winsorized. Test statistics are calculated using robust standard errors. Panel A shows regression results for the full sample period. Panel B shows coefficient estimates of MPERIOD\*RESTATER in similar regressions, where the misstated and pre-misstated periods are partitioned into two sub-periods of equal length; these regressions are estimated separately for each misstated sub-period, using the second pre-misstated period as the control period in each case. In the test for parallel paths in Panel C, the prior period is a period of equal length to the pre-misstated period immediately preceding the latter.

Independent variable	Dependent variable									
	#Ins		$\ln(1 + \#Sh)$		$\ln(1 + \$Sh)$		%Eq		%Hold	
	Coef.	z-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
<i>Panel A: Full sample period</i>										
Ln (Market cap)	0.18	4.58 <sup>a</sup>	0.67	5.41 <sup>a</sup>	1.20	5.32 <sup>a</sup>	0.05	2.83 <sup>a</sup>	16.43	5.26 <sup>a</sup>
$\sigma_s$	-4.08	-0.94	16.12	1.25	18.84	0.80	1.24	0.65	436.31	1.56
$\Delta\sigma_s$	-12.66	-2.15 <sup>b</sup>	-32.69	-1.88 <sup>c</sup>	-61.19	-1.92 <sup>c</sup>	-4.07	-1.58	-395.20	-1.11
$RET_{-1}$	35.00	1.99 <sup>b</sup>	130.57	2.62 <sup>a</sup>	238.88	2.61 <sup>a</sup>	18.72	2.73 <sup>a</sup>	2798.81	2.51 <sup>b</sup>
$RET_{-2}$	48.46	2.61 <sup>a</sup>	68.35	1.23	141.20	1.40	10.54	1.26	1547.08	1.28
$RET_{-3}$	-13.99	-0.82	-29.37	-0.57	-66.79	-0.71	-2.86	-0.39	-382.79	-0.34
$RET_{-4}$	11.53	0.67	34.72	0.64	62.33	0.63	5.46	0.66	682.95	0.58
Holdings <sup>1</sup>	0.05	1.64	0.03	0.59	0.05	0.71	0.58	1.55		
B/M decile	-0.05	-1.59	-0.14	-1.59	-0.28	-1.74 <sup>c</sup>	-0.02	-1.40	-1.98	-1.06
R&D/Sales	-0.15	-1.01	-0.49	-1.29	-0.82	-1.22	-0.08	-1.46	-9.00	-1.15
$CAR_{-5,+5}$	-0.72	-2.01 <sup>b</sup>	-0.89	-0.74	-1.83	-0.84	-0.05	-0.25	-15.10	-0.61
MPERIOD	-0.04	-0.25	-0.07	-0.12	-0.16	-0.16	-0.04	-0.57	-4.67	-0.37
RESTATER	-0.28	-1.44	-0.72	-1.17	-1.49	-1.33	-0.10	-1.20	-19.29	-1.45
MPERIOD* RESTATER	0.49	1.88 <sup>c</sup>	0.99	1.20	1.92	1.27	0.16	1.33	29.82	1.65 <sup>c</sup>
Constant	-3.02	-4.45 <sup>a</sup>	-11.01	-5.28 <sup>a</sup>	-19.30	-5.13 <sup>a</sup>	-0.97	-3.15 <sup>a</sup>	-270.42	-5.18 <sup>a</sup>
Number of observations	728		728		728		728		728	
p-value <sup>2</sup>	0.0000		0.0000		0.0000		0.0003		0.0000	
Pseudo R-squared	0.0720		0.0467		0.0422		-0.0376		0.0773	
Marginal effect of MPERIOD* RESTATER <sup>3</sup>	0.170		5.599		0.279		0.039		7.362	
Mean of dependent variable <sup>4</sup>	0.434		24.098		1.028		0.078		18.701	
% Marginal effect of MPERIOD* RESTATER <sup>5</sup>	39.1		23.2		27.1		49.8		39.4	
<i>Panel B: Sub-periods</i>										
First half	0.57	2.04 <sup>a</sup>	0.48	0.44	0.94	0.48	0.05	0.25	32.49	1.13
Second half	0.42	1.39	0.29	0.24	0.69	0.32	-0.02	-0.09	18.82	0.64
<i>Panel C: Test for parallel paths</i>										
	#Ins		#Sh		\$Sh		%Eq		%Hold	
Restating firms										
1 Pre-misstated period	0.30		21.4		946		14.1		12.6	
2 Prior period	0.19		42.8		3590		13.8		12.0	
Control firms										
3 Pre-misstated period	0.35		66.8		3312		12.1		14.8	
4 Prior period	0.20		84.6		5367		7.3		9.5	
Test Statistics										
1-2	3.05 <sup>a</sup>		-0.93		-0.97		0.05		0.13	
1-3	-0.96		-0.97		-0.93		0.33		-0.73	
3-4	4.50 <sup>a</sup>		-0.81		-0.97		1.97 <sup>b</sup>		2.07 <sup>b</sup>	
(1-2) - (3-4)	-0.69		-0.12		-0.17		-0.55		-1.16	

firms who sell during the misstated period is zero for 67% of the sample, one for 19% of the sample, and two or more for the remaining 14% of the sample. Given that the observations of this dependent variable represent count data, we do the equi-dispersion test to determine whether to use the Poisson regression or the Negative Binomial model (see Greene, 2003 for an exposition). In all cases, the null hypothesis of the equality of mean and variance is rejected, so we estimate Eq. (3) using the Negative Binomial model.

The second and third dependent variables are the number of shares traded by an insider group (#Sh) and the dollar value of those shares (\$Sh). As with #Ins, many observations of #Sh and \$Sh equal zero, and the distribution of the remaining observations is highly skewed. We take the natural logarithm of one plus #Sh or \$Sh to normalize observations with non-zero values. The resulting variables are censored from below at zero. We use the single-censored Tobit model to estimate these regressions.

The fourth and fifth dependent variables are the percentage of outstanding equity traded by an insider group (%Eq) and the percentage of their shareholdings traded (%Hold). These variables are censored from below at zero and from above at one. We use the double-censored Tobit model to estimate these regressions (see Greene, 2003) for a detailed exposition of both variants of the Tobit model). Finally, since our sample of insider sales contains some influential outliers, we winsorize the top and bottom 1% of the dependent-variable observations in each regression.

## 5.2. Sales by top management

We start by presenting the results for sales by top management. As defined in Section 4.3.2 above, this group consists of individuals holding the titles of Chairman, CEO, COO, or President. Panel A of Table 5 presents these results for the full sample period. The coefficient of MPERIOD measures the abnormal level of insider trading in the misstated period relative to the pre-misstated period (i.e., the time-series control), after controlling for other cross-sectional determinants of insider trading. Similarly, the coefficient of RESTATER measures the abnormal level of insider trading in restating firms relative to that in control firms (i.e., the cross-sectional control). Our main interest is in the coefficient of MPERIOD\*RESTATER, which represents the difference-in-differences (DID) estimate of the abnormal level of insider trading. This interaction term measures the abnormal level of insider trading in restating firms relative to the time-series and cross-sectional controls (i.e., the dual control).

Before we present the results from the DID regressions, we need to test whether our restating and control samples meet the parallel paths assumption. The null hypothesis is that period-to-period changes in insider sales for restating firms are not significantly different from changes in insider sales for control samples. We test the null hypothesis by comparing the level of insider sales during our pre-misstated period to insider sales during the period leading up to the pre-misstated period. We define the prior period as a period of equal length to the pre-misstated period immediately preceding the latter. The five columns in Panel C show the results of this test for each of our five measures of insider sales. The first two rows show mean values of each measure for restating firms for the pre-misstated period (1) and the prior period (2). The third and fourth rows show corresponding values for the control sample. These are followed by test statistics for the difference in means for restating firms and for control firms over the two periods [(1–2) and (3–4)]. The last row shows the test statistics for tests of parallel paths [(1–2) – (3–4)]. The test statistics indicate that for each of the five measures of insider trading, the average change in insider sales between the prior period and the pre-misstated period for restating firms is not significantly different from the average change observed for control firms. The results reported in Panel C of Table 5 suggest that insiders in the control sample were behaving similarly to insiders in the restating sample at the time of sample formation.

Panel A of Table 5 shows that consistent with prior research, top managers sell significantly more stock if they lead larger firms, firms that experience a decrease in stock volatility, or firms with better stock performance over the prior three to six months. The estimated coefficients of the individual time-series and cross-sectional controls show no evidence of unusual selling by top managers of restating firms. Using the DID approach, there is weak evidence that top management of restating firms sold abnormally large amounts of stock during the misstated period. The estimated coefficient of MPERIOD\*RESTATER is positive and statistically significant at the 10% level in regressions of the number of insiders selling (#Ins) and the percentage of insider holdings sold (%Hold).

To estimate the economic magnitude of this effect, we compute the marginal effect of the interaction term, MPERIOD\*RESTATER, as:

$$\left[ E\left(y|D_1 = 1, D_2 = 1, D_1 * D_2, X\right) - E\left(y|D_1 = 1, D_2 = 0, D_1 * D_2, X\right) \right] \\ - \left[ E\left(y|D_1 = 0, D_2 = 1, D_1 * D_2, X\right) - E\left(y|D_1 = 0, D_2 = 0, D_1 * D_2, X\right) \right],$$

where  $y$  is the dependent variable,  $D_1 =$  MPERIOD,  $D_2 =$  RESTATER, and  $X = x_1$  to  $x_n$  represents other explanatory variables. The term in the first square brackets measures the difference during the misstated period in sales of top managers of restating firms relative to control firms. The term in the second square brackets measures the corresponding difference in sales during the pre-misstated

### Notes to Table 5:

<sup>a,b,c</sup> Denote statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

<sup>1</sup> Across columns 1 through 4, respectively, this variable refers to the natural logarithm of one plus: the number of insiders with shareholdings, the number of shares held by insiders, the dollar value of those holdings, or the percentage of outstanding equity held.

<sup>2</sup> Of the chi-squared test.

<sup>3</sup> Marginal effect of  $D_1 * D_2 = [E(y|D_1 = 1, D_2 = 1, D_1 * D_2, X) - E(y|D_1 = 1, D_2 = 0, D_1 * D_2, X)] - [E(y|D_1 = 0, D_2 = 1, D_1 * D_2, X) - E(y|D_1 = 0, D_2 = 0, D_1 * D_2, X)]$ , where  $y$  is the dependent variable,  $D_1 =$  MPERIOD,  $D_2 =$  RESTATER, and  $X = x_1$  to  $x_n$  are all of the explanatory variables except for  $D_1$ ,  $D_2$  and  $D_1 * D_2$ . The expectations are evaluated at the sample means of the  $x$ 's. Columns 2 and 3 report marginal effects computed from regressions of #Sh and \$Sh instead of  $\ln(1 + \#Sh)$  and  $\ln(1 + \$Sh)$ , to allow interpretation of the magnitudes of the effects.

<sup>4</sup> Columns 2 and 3 report mean values of #Sh and \$Sh.

<sup>5</sup> % Marginal effect =  $100 * (\text{Marginal effect} / \text{Mean of dependent variable})$ .

**Table 6**

Regressions of sales by insider group, partitioned by announcement returns.

The table shows the percentage marginal effect and test statistic (in parentheses below) for  $MPERIOD \times RESTATER$  computed from regressions similar to those shown in Table 5. Each cell in the table reports the result of one regression. Restatements with worse (better) announcement returns are those in the bottom (top) 40% among restating firms in the regression sample based on the cumulative abnormal returns of restating firms over days  $(-5, +5)$  around the announcement. The results reported in Panels A, B, and C are estimated using 728, 292, and 292 observations, respectively.

Insider Group	Panel A. Full sample					Panel B. Worse announcement returns					Panel C. Better announcement returns				
	#Ins	#Sh	\$Sh	%Eq	%Hold	#Ins	#Sh	\$Sh	%Eq	%Hold	#Ins	#Sh	\$Sh	%Eq	%Hold
Top management	39.1 (1.88 <sup>c</sup> )	23.2 (1.20)	27.1 (1.27)	49.8 (1.33)	39.4 (1.65 <sup>c</sup> )	83.9 (2.37 <sup>b</sup> )	112.0 (2.39 <sup>b</sup> )	112.3 (2.38 <sup>b</sup> )	115.1 (2.25 <sup>b</sup> )	93.3 (2.37 <sup>b</sup> )	-6.9 (-0.35)	-77.6 (-0.93)	-68.8 (-0.90)	-26.3 (-0.46)	-25.6 (-0.70)
Top financial officers	-9.4 (-0.43)	-2.6 (0.45)	-22.2 (0.44)	21.7 (0.62)	7.3 (0.26)	10.6 (0.26)	22.0 (0.79)	4.5 (0.81)	44.4 (0.76)	23.0 (0.48)	-16.5 (-0.63)	-17.0 (0.32)	-37.7 (0.28)	33.7 (0.61)	9.5 (0.30)
All officers	-0.2 (0.14)	-4.9 (-0.06)	-11.1 (-0.17)	-1.6 (-0.04)	2.2 (0.21)	30.5 (1.12)	-17.6 (-0.06)	-9.1 (-0.13)	-33.9 (-0.62)	1.8 (0.03)	-24.0 (-1.07)	-18.1 (-0.38)	-34.0 (-0.42)	14.1 (0.33)	0.3 (0.03)
All directors	-3.6 (-0.12)	-19.1 (-0.19)	-23.7 (-0.10)	-24.6 (-0.71)	1.9 (0.12)	23.3 (0.87)	-35.2 (-0.41)	-36.8 (-0.30)	-47.3 (-0.80)	-4.0 (-0.14)	-16.7 (-0.69)	12.4 (0.37)	2.1 (0.30)	34.4 (0.73)	5.0 (0.17)
Blockholders	22.7 (0.62)	-25.6 (0.32)	-33.3 (0.36)	-21.6 (-0.13)	-3.6 (0.12)	-21.1 (0.15)	-51.3 (-0.07)	-56.8 (0.06)	-34.1 (-0.30)	-5.1 (0.31)	2.2 (-0.02)	-1.9 (-0.06)	-4.2 (-0.05)	2.7 (-0.15)	-1.3 (-0.44)

<sup>a,b,c</sup>Denote statistical significance at the 1%, 5%, and 10% levels, respectively, in two-tailed tests.

(normal) period. Thus, the overall expression represents the difference-in-differences (DID) estimate. The expectations are evaluated at the sample means of the  $x$ 's. Columns 2 and 3 report marginal effects computed from regressions of #Sh and \$Sh instead of  $\ln(1 + \#Sh)$  and  $\ln(1 + \$Sh)$ , to allow interpretation of the magnitudes of the effects.

In Panel A of Table 5, the marginal effect of 0.17 in the regression of the number of insiders (#Ins) implies that on average, there were 0.17 more top managers of misstating firms who sold stock during the misstated period using the DID approach (i.e., the dual control), after controlling for other things. Relative to the mean value of the dependent variable in the regression (0.434 top managers selling), the marginal effect is economically substantial: 39% more top managers of misstating firms sold during the misstated period. Similarly, the marginal effect of 7.36 in the regression of %Hold implies that on average, top managers of misstating firms sold an additional 7.36% of their holdings during the misstated period. Relative to the mean of the dependent variable (18.7% of the holdings sold), the percentage of holdings sold by top managers of misstating firms during the misstated period is 39.4% higher.

As shown in Panel C of Table 2, the length of the misstated period varies considerably across firms, spanning several years in some cases. Examining the entire misstated period may not reveal abnormal trading activity if unusual insider trading is confined to a portion of the misstated period. We address this possibility by subdividing our misstated and pre-misstated periods into two sub-periods of equal length. For each misstated sub-period, we then re-estimate Eq. (3) for top management, using the second pre-misstated period as the time-series control in each case. The results are summarized in Panel B of Table 5. To save space, we only report coefficient estimates of MPERIOD\*RESTATE. Except for the significantly positive ( $t = 2.04$ ) coefficient estimate in the first sub-period regression of the number of insiders selling, Panel B shows no evidence of abnormal selling by top managers of misstating firms.

### 5.3. Sales by other insiders and subsamples by announcement returns

Table 6 shows regression estimates of Eq. (3) for all five insider groups described in Section 4.3.2: top management, top financial officers, all officers, all directors, and blockholders. We report results for the full sample in Panel A, and for the subsample with worse (better) announcement returns in Panel B (C). Restatements with worse (better) announcement returns are those in the bottom (top) 40% of the sample based on the CAR over days  $(-5, +5)$ . For brevity, we only report the percentage marginal effects of MPERIOD\*RESTATE and the test statistics of the variable's estimated coefficients computed from regressions similar to those shown in Table 5. Each cell in the table reports the result of one regression. The results reported in Panels A, B, and C of Table 6 are estimated using 728, 292, and 292 observations, respectively. The sample sizes in Panels B and C do not add up to those in Panel A because of missing data on stock returns.

Row 1 in Panel A of Table 6 simply summarizes the results for top management from Table 5. Panel B shows that top managers sold abnormally large amounts of stock before restatements with worse announcement returns, based on all five measures of insider sales; the five coefficients estimated for MPERIOD\*RESTATE are positive and statistically significant at the 5% level. The average dollar value of their sales is 112% higher than its normal level. In the subsample with better announcement returns in Panel C, there is no abnormal selling by top managers. The results for these two subsamples are consistent with the hypotheses discussed in Section 2 for these two cases. Given the costs of illegal insider trading, top managers sell before revealing the manipulation when the gain from doing so is large, while they abstain from selling during misstated periods when the potential gain is smaller. Besides top managers, there is no evidence of abnormal selling by any other insider group in misstating firms during the misstated period, either for the full sample or for the worse or better announcement returns subsamples.

Why do top managers trade profitably on earnings manipulation but top financial officers, who are likely involved in the manipulation, refrain from doing so? There are at least three potential explanations of this finding. First, accountants and financial managers tend to be conservative, while top managers tend to be aggressive, as an extensive recent finance literature on CEO overconfidence suggests (see, e.g., Goel and Thakor, 2008; Malmendier and Tate, 2005). A behavioral explanation is also consistent with the findings of Bhattacharya and Marshall (2012) and Hillier et al. (2015).

Second, it is much riskier for a CFO to trade on the manipulation than a CEO because it would be hard, even farcical, for a CFO to claim ignorance of the manipulation like CEOs sometimes do (see, e.g., testimony at the accounting fraud trials of Bernard Ebbers and Richard Scrushy, then-CEOs of Worldcom and HealthSouth, respectively. See Latour and Young, 2005; Morse and Shmukler, 2005).

Third, CEOs generally have much larger stockholdings and more of their wealth at risk than CFOs, so they have greater incentive to sell before the announcement of the restatement. What about directors? For one thing, directors may not know about the manipulation. Directors usually meet only for a few hours about four to six times per year, and usually only become aware of issues that are presented to them by the CEO. In addition, concerns about loss of reputation, which is crucial for success in the director labor market, can prevent directors from trading on knowledge of the misstatement.

## 6. Other subsample results for sales by top management

We next examine five other partitions of our sample where managers have different incentives to sell before the disclosure of accounting problems. These partitions are based on: (1) the seriousness of the restatement, (2) whether the restated earnings are negative, (3) the number of restated quarters, (4) the dollar loss to insiders upon the restatement announcement, and (5) the percentage change in reported earnings. Table 7 summarizes these results. To save space, for each regression we only report the percentage marginal effect of MPERIOD\*RESTATE and the test statistic of the estimated coefficient for the variable. Panel A reports results for the full sample. Panel B (C) reports results for the subsample of restatements initiated by the company (outsiders such as auditors or the SEC). Row 1 in Panel A summarizes the results for the full sample of restatements from Table 5. In row 1, while the magnitude of

**Table 7**

Subsample regressions of sales by top management.

The table shows the percentage marginal effects of MPERIOD\*RESTATE and the test statistics in parentheses below for the estimated coefficients computed from regressions similar to those shown in Table 5. Each cell in the table reports the result of one regression. The numbers in parentheses below row headings show the sizes of the subsamples for Panels A, B and C, respectively. Restatements are classified as less serious if they are triggered by SAB 101 or certain EITF consensuses, correct earnings releases, or involve only non-core accounts; the remaining cases are more serious. Restatements with worse (less serious) potential loss are those in the bottom (top) 40% among restating firms in the regression sample based on insiders' potential loss avoided from selling before the announcement, defined as the dollar value of insider shareholdings before the misstated period multiplied by CAR over days (-5, +5). The superscripts a, b and c denote significantly different from zero at the 1%, 5%, and 10% levels, respectively, in two-tailed tests. Some values in Panel C for the 'less serious potential loss' subsample are missing because the Tobit model did not converge.

Subsample	Panel A. Full sample					Panel B. Initiated by the company					Panel C. Initiated by outsiders				
	#Ins	#Sh	\$Sh	%Eq	%Hold	#Ins	#Sh	\$Sh	%Eq	%Hold	#Ins	#Sh	\$Sh	%Eq	%Hold
Full sample (728, 568, 160)	39.1 (1.88 <sup>c</sup> )	23.2 (1.20)	27.1 (1.27)	49.8 (1.33)	39.4 (1.65 <sup>c</sup> )	39.4 (1.56)	25.6 (0.92)	30.7 (0.99)	39.2 (0.91)	33.8 (1.20)	20.0 (0.77)	4.2 (0.69)	-2.3 (0.71)	62.0 (1.24)	44.2 (1.27)
Less serious restatements (316, 244, 72)	4.4 (0.28)	-20.9 (-0.05)	-14.4 (-0.09)	39.6 (0.78)	-3.0 (0.00)	-5.7 (-0.18)	-19.2 (-0.40)	-13.4 (-0.41)	20.8 (0.35)	-18.5 (-0.38)	-1.4 (0.41)	-8.7 (0.83)	1.8 (0.87)	103.3 (1.79 <sup>c</sup> )	31.1 (0.68)
More serious restatements (412, 324, 88)	66.9 (2.11 <sup>b</sup> )	50.5 (1.65 <sup>c</sup> )	60.4 (1.79 <sup>c</sup> )	57.8 (1.30)	78.7 (2.26 <sup>b</sup> )	75.6 (2.05 <sup>b</sup> )	60.9 (1.68 <sup>c</sup> )	85.1 (1.79 <sup>c</sup> )	56.4 (1.20)	82.9 (2.08 <sup>b</sup> )	15.3 (0.46)	-14.6 (0.29)	-45.9 (0.38)	5.1 (0.40)	41.6 (0.86)
Non-negative restated earnings (372, 280, 92)	15.2 (0.55)	13.4 (0.17)	13.8 (0.25)	49.0 (0.91)	11.2 (0.34)	15.4 (0.64)	13.2 (0.40)	13.3 (0.43)	42.1 (0.83)	9.7 (0.22)	2.5 (0.31)	-63.2 (-0.47)	-61.6 (-0.33)	28.3 (0.49)	3.1 (0.25)
Negative restated earnings (356, 288, 68)	81.8 (2.34 <sup>b</sup> )	59.6 (1.71 <sup>c</sup> )	69.5 (1.69 <sup>c</sup> )	67.8 (1.37)	84.7 (2.05 <sup>b</sup> )	78.9 (1.77 <sup>c</sup> )	58.1 (1.04)	64.5 (1.07)	64.9 (0.76)	69.3 (1.52)	76.6 (2.46 <sup>b</sup> )	101.6 (2.83 <sup>b</sup> )	92.2 (2.78 <sup>b</sup> )	137.2 (2.67 <sup>a</sup> )	119.4 (2.15 <sup>b</sup> )
≤4 restated quarters (552, 428, 124)	-0.7 (-0.05)	-13.0 (0.29)	-8.8 (0.32)	26.2 (0.61)	15.6 (0.52)	-8.1 (-0.28)	-16.5 (0.04)	-10.9 (0.10)	14.3 (0.30)	6.5 (0.19)	3.3 (0.26)	-12.0 (0.46)	-10.8 (0.44)	33.6 (0.73)	28.3 (0.81)
>4 restated quarters (176, 140, 36)	91.2 (2.77 <sup>a</sup> )	145.5 (2.13 <sup>b</sup> )	135.4 (2.16 <sup>b</sup> )	119.7 (2.44 <sup>b</sup> )	120.2 (2.65 <sup>a</sup> )	110.8 (2.51 <sup>b</sup> )	159.6 (2.22 <sup>b</sup> )	146.1 (2.16 <sup>b</sup> )	143.7 (2.30 <sup>b</sup> )	130.3 (2.32 <sup>b</sup> )	57.9 (2.51 <sup>b</sup> )	7.6 (1.50)	3.0 (1.89 <sup>c</sup> )	288.3 (2.84 <sup>a</sup> )	142.8 (2.24 <sup>b</sup> )
Less serious potential loss (292, 236, 56)	-7.2 (-0.22)	-54.6 (-0.65)	-52.0 (-0.62)	-28.8 (-0.46)	-18.7 (-0.46)	-16.8 (-0.53)	-62.4 (-0.95)	-59.1 (-0.94)	-40.0 (-0.67)	-33.4 (-0.87)	44.8 (7.59 <sup>a</sup> )	-	-	-	-
Worse potential loss (292, 220, 72)	83.9 (2.73 <sup>a</sup> )	120.4 (2.24 <sup>b</sup> )	125.5 (2.24 <sup>b</sup> )	109.4 (2.28 <sup>b</sup> )	90.2 (2.51 <sup>b</sup> )	111.8 (2.94 <sup>a</sup> )	148.6 (2.55 <sup>b</sup> )	149.2 (2.49 <sup>b</sup> )	112.3 (2.24 <sup>b</sup> )	105.6 (2.54 <sup>b</sup> )	26.8 (0.75)	21.9 (0.17)	6.7 (0.21)	67.9 (0.97)	42.2 (0.72)



abnormal selling by top managers in the subsample of cases initiated by the company (Panel B) is larger than in cases initiated by outsiders (Panel C), both are statistically insignificant.

### 6.1. Less serious vs. more serious misstatements

As discussed in Section 4.1, our sample consists of firms that announced restatements to correct prior financial misstatements (i.e., GAAP violations). Because the financial reporting boundaries set by GAAP are wide, and a violation is termed a misstatement only when material, a misstatement indicates a serious infraction (see, e.g., Palmrose and Scholz, 2004). Nevertheless, our sample includes some cases where firms restated due to technical, and arguably less serious, reasons. One such group of less serious restatements was triggered by the SEC's adoption of revenue recognition rules under Staff Accounting Bulletin (SAB) 101.<sup>33</sup> Our sample of 518 restating firms contains 61 firms that attribute their restatements to SAB 101. Our sample contains one additional restatement prompted by guidance issued by the Emerging Issues Task Force (EITF) of the Financial Accounting Standards Board (FASB). The EITF periodically identifies emerging accounting issues and releases guidelines to establish a uniform set of accounting practices before divergent methods arise and become widespread.<sup>34</sup> As noted in Section 4.1 above, our sample includes 15 firms that restate earnings releases and not financial statements issued in 10-K or 10-Q filings. Earnings releases are somewhat preliminary, so restating them is a less serious infraction than restating financial statements included in SEC filings, which are supposed to be definitive. Finally, restatements involving non-core accounts, used to record non-routine transactions and one-time or special items, are less serious than restatements involving core accounts. While restatements involving both types of accounts affect investor estimates of current profits, those involving core accounts can also affect investor forecasts of future profits and cash flows. Our sample includes an additional 103 cases of restatements that involve only non-core accounts. We examine whether the results from estimating the Table 5 regression model using these 180 (= 61 + 1 + 15 + 103) 'less serious' cases differ from those for the remaining 'more serious' cases. As shown in Table 3, the average abnormal return around the announcement is substantially worse for the latter subsample than the former. As discussed in Section 2, we expect managers to refrain from profitable trading in less serious cases, but to engage in it in more serious cases.

Rows 2 and 3 in Panel A of Table 7 report, respectively, the results of five regressions each for the subsamples of less serious and more serious restatements. Consistent with our hypothesis, while there is essentially no evidence of abnormal selling by top managers of restating firms in the subsample of less serious restatements in row 2, there is such evidence in the more serious restatements subsample in row 3. The magnitude of their abnormal sales as a percentage of their holdings in the latter subsample is quite large, about 79% above its usual levels. When we further partition these subsamples by the identity of the initiator of the restatement, we find that the abnormal selling by top managers in the subsample of more serious restatements is confined to restatements initiated by the company (Panel B), as hypothesized in Section 2. Here, the magnitude of their abnormal sales as a percentage of their holdings is even larger, at about 83% above its usual levels.

### 6.2. Non-negative versus negative restated earnings

While the announcement of any restatement is bad news because it damages the credibility of management, the news tends to be more detrimental if restated earnings are negative. As Table 3 shows, the average abnormal return around the announcement is substantially worse when the restated earnings are negative than when they are positive. To examine whether insiders behave differently in the two cases, we partition our sample by the sign of restated earnings. We then estimate Eq. (3) separately for the two subsamples.

Consistent with our hypothesis, while there is no evidence of abnormal selling by top managers in cases where the restated earnings are non-negative (row 4 of Panel A), there is evidence of significant abnormal selling by them in cases of negative restated earnings (row 5 of Panel A). Surprisingly, in row 5, their abnormal selling is more pronounced in cases initiated by outsiders than those initiated by the misstating firms themselves.

### 6.3. Restatements with fewer versus more quarters restated

We next partition the sample into two groups by the number of quarters restated: firms restating four or fewer quarters versus those restating at least five quarters. The former restatements are viewed as being less serious by investors than the latter, as seen in the stock price reaction to the two groups in Table 3. In addition, insiders in the first subsample have less time to sell their stockholdings during the misstated period than insiders in the second subsample. We use four quarters as the breakpoint because that is the median number of quarters restated in the sample (see Table 2, Panel B).

Consistent with our hypothesis, Panel A of Table 7 shows that while there is no abnormal selling by top managers in firms that restated up to four quarters (row 6), there is substantial and statistically significant selling by them in firms that restated more than four quarters (row 7). Our estimate of the average dollar value of abnormal sales by top managers in row 7 is as much as 135% of its usual level. Also consistent with our hypothesis, their abnormal dollar sales are usually larger in restatements initiated by the company than in those initiated by outsiders.

<sup>33</sup> Although SAB 101 restatements are viewed as less serious, Rountree (2003) finds that on average, stock price reactions to such announcements are negative.

<sup>34</sup> See <http://www.investopedia.com>. Both SAB 101 and EITFs represent clarifications of, rather than changes to, GAAP.

#### 6.4. Restatements partitioned by insiders' potential loss

Finally, while it is difficult to measure the dollar cost of trading on accounting manipulation to managers (e.g., job loss, jail time, and adverse career consequences), we can make reasonable estimates of their benefit from trading. This benefit equals the dollar value of the loss avoided by selling before the restatement announcement. We measure the gain to insiders upon the announcement as the dollar value of insider shareholdings before the misstated period (i.e., our Holdings variable in Section 5.2.1) multiplied by  $CAR_{-5,+5}$ . For each group of top managers, we examine two subsamples: managers in the bottom 40% or top 40% of the sample partitioned by their potential loss from not selling before the restatement announcement. Consistent with our hypothesis, in row 8 of Panel A of Table 7, while there is no abnormal selling by managers with less serious potential loss from restatement, there is significant and substantial selling by managers with more serious potential loss in row 9. The dollar value of top managers' abnormal sales is as much as 126% above its normal level. Also consistent with our hypothesis, this abnormal selling in row 9 is largely confined to restatements initiated by the company (Panel B), where its magnitude is even bigger.

#### 6.5. Restatements with small versus large effects on earnings

We next examine the idea that managers have greater incentives for selling during the manipulation period when the manipulation is bigger. Table 3 shows that the decline in stock prices is larger upon the announcement of restatements that result in larger percentage declines in reported earnings. Accordingly, we partition the sample by the size of the decline in reported earnings, and examine two groups: those in the top 40% by the magnitude of the decline and those in the bottom 40%. We estimate Eq. (3) separately for each group. There is little evidence of unusual selling by top managers in either subsample. To save space, we do not tabulate these results.

### 7. Summary and conclusions

Following the wave of accounting scandals during the early 2000s at prominent companies such as Enron, HealthSouth, and Tyco, there has been public and media outrage about insider trading that preceded the revelation of these scandals. This paper empirically examines the prevalence of such insider trading. We analyze insider trading activity in a sample of 518 publicly traded U.S. companies that announced earnings-decreasing restatements over the 1997–2002 time period to correct misstated financial statements. We compare the level of insider trading in restating firms to its level in an industry-size matched sample of control firms. We examine insiders' sales, purchases and net sales during the misstated period and a pre-misstated period, using a difference-in-differences approach. We focus on open-market stock transactions of five insider groups: top management, top financial officers, all officers, all directors, and blockholders. We analyze five parametric and two non-parametric measures of the level of insider trading, and use cross-sectional regressions that control for other determinants of the level of insider trading.

Our conclusions are tempered by three caveats that apply to most studies of insider trading.<sup>35</sup> First, we only examine trades of registered corporate insiders; we do not observe the activities of other potentially informed parties (such as auditors) who are not required to report their trades to the SEC. Second, even registered insiders may trade via friends or extended family members (outside their immediate family), who are not required to report their trades. However, an insider who trades illegally may think twice about involving others, because expanding the circle of participants increases the likelihood that the crime will be revealed. Third, our tests assume that registered insiders report their trades to the SEC as required by law.

For the full sample of restating firms, we find weak evidence that top managers of misstating firms sell more stock during the misstated period than during the pre-misstated period, relative to the control sample and after controlling for other determinants of the level of insider trading. But in a number of subsamples where insiders had greater incentives to sell before the revelation of accounting problems, we find strong evidence that top managers of restating firms sell substantially more stock during the misstated period. These subsamples include restatements that are more serious, lead to negative restated earnings, correct more misstated quarters, lead to larger stock-price declines upon the announcement, and result in greater dollar losses to insiders when announced. The magnitudes of the effects that we find are striking. For example, in restatements that correct more than four quarters of earnings, there are on average about twice as many misstating-firm top managers selling in misstated periods than usual; their abnormal stock sales are about \$1.4 million in constant 2004 dollars; and these abnormal sales represent about 22% of their stockholdings, a 120% increase relative to their normal level of stock sales.

Our findings suggest that top managers' desire to sell their stockholdings at inflated prices is a motive for earnings manipulation. Finally, our finding that insiders boldly trade on a crime for which they are potentially liable suggests that insider trading is more widespread than has been found in the previous literature. These findings provide a justification for the prohibition on profitable insider selling during periods of financial misstatement adopted by the Sarbanes–Oxley Act.

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<sup>35</sup> An exception is Meulbroek (1992), who examines illegal insider trading uncovered by, rather than reported to, the SEC.

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