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Financial Statement Misreporting: Does Monitoring Matter?

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Abstract: I find a positive relation between the likelihood that a firm misreports its financial statements and its use of bank debt. I next test whether this result might obtain from the greater likelihood of the detection of misreporting, rather than misreporting per se, but my results suggest that this is not the case. Finally I find that the relations between bank debt and misreporting are stronger among larger firms, those with bond ratings, and those covered by Execu Comp. In sum, my results suggest that when firms are subjected to a greater extent of monitoring by outside agents, misreporting is more likely.

Key words: Financial Statement Misreporting, Bank Monitoring, Delegated Monitoring
JEL: G10, G21, G32, M41

1. Introduction

A substantial literature demonstrates theoretically and empirically that banks are effective monitors who add value to borrowing firms (Diamond, 1984; Sharpe, 1990; James, 1987; Lummer and McConnell, 1989; Slovin, Johnson and Glascock, 1992). Banks have clear incentives to monitor their borrowers because of their financial stakes in these firms, which make it in the best interests of banks to ensure that their borrowers do not take any actions that would reduce the value of their stakes, such as asset substitution or financial statement misreporting.[11] Banks' incentives to monitor their borrowers' financial statements in particular derive from covenants in the bank loan agreements, because they are written on information obtained from the borrowers' financial statements. A covenant violation helps

[11]I use the terms "misreporting" and "earnings management" interchangeably, and define them as in Healy and Wahlen (1999), "when managers use judgment in financial reporting and in structuring transactions to alter financial reports to either mislead some stakeholders about the underlying economic performance of the company or to influence contractual outcomes that depend on reported accounting numbers."

to protect the bank from losses, and allows the bank to force a renegotiation of the loan, thereby shifting bargaining power from the borrower to the lender (Rajan and Winton, 1995). If a borrower's financial statements are inaccurate, then the bank is deprived of this increased bargaining power and opportunity to protect the value of its assets. Thus at firms financed with bank debt, monitoring by the lending bank should increase the probability that misreporting is detected. This increased threat of detection should act as a deterrent and therefore decrease the likelihood that managers choose to misreport. I test this hypothesis, and find instead that the likelihood of misreporting instead increases with firms' use of bank debt. This result is inconsistent with the literature on bank monitoring, rather it supports the debt covenant hypothesis, which suggests managers may use aggressive accounting techniques to avoid covenant violations or to lower borrowing costs.

But because I can observe only misreporting that has been detected and restated, these results are open to an alternative interpretation. The positive relations that I find may result because banks have monitored their borrowers and exposed their accounting irregularities. That is, a positive relation could exist because firms with bank debt are more likely to misreport, or because they are more likely to restate in order to correct their prior misreporting. I attempt to address this imperfection of the data by comparing firms whose misreporting is detected immediately to firms whose misreporting is initially undetected. I find that these firms do not differ in their use of bank debt. When I compare firms whose misreporting is initially undetected to control firms, I once again find a positive relation between misreporting and bank debt. These results suggest that it is misreporting per se, and not its detection and restating, that is related to the use of bank debt.

My results support theoretical models developed by Dye (1988) and Trueman and Titman (1988) that predict managers will manage earnings in order to alter outside claimants' perceptions regarding firm value. Empirically, my results accord with those of Defond and Jiambalvo (1994), Sweeney (1994) and Dichev and Skinner (2002) who find evidence consistent with earnings management to avoid debt covenant violations. More recently, Efendi, Srivastava and Swanson (2007) examine long-term borrowing and find misreporting is more likely when firms are constrained by debt covenants or need to raise new capital, and Jha (2013) finds that firms manage earnings in the quarters surrounding debt covenant violations. Hasan, Park and Wu (2012) find that earnings predictability improves the terms of bank loan agreements, so to the extent that managers might be tempted to misreport in

order to make earnings more predictable, my results accord with theirs. My results extend those of Stanley and Sharma (2011), who find that the likelihood of more material misreporting is related to firms' use of bank debt, by considering which types of firms might be most likely to misreport when financed with bank debt. My results are contrary to those of Ahn and Choi (2009) who find that earnings management decreases as bank monitoring increases, but our results are not directly comparable because of our different data sets. Interestingly, my results accord with those of Firth, Rui and Wu (2011) who find a positive relation between debt and earnings manipulation among firms in China. Given the large differences in the cultural, legal, regulatory and other environments that these firms face, it is remarkable to see that this relation between bank debt and misreporting still obtains.

This paper differs from most of those above and contributes to the literature on misreporting by examining bank debt,[12] which has tighter covenants and is subject to more monitoring than other types of debt. These tighter covenants may provide greater incentives to misreport; however more monitoring should also provide a greater deterrent to misreporting. So although the predictions I test hold for debt in general, the predicted relations are stronger for bank debt in particular.

If my interpretation of this positive relation is correct, and bank debt provides incentives for managers to misreport in order to avoid loan default or improve loan terms, I next consider if some firms might be more likely to do so than others. Banks are not the only outside agents who have clear incentives to monitor. Others like analysts, the media, fund managers, and bond market participants also have monitor the firm, and this additional attention and scrutiny should also suffice to make misreporting less likely. I consider three firm characteristics: size, whether or not the firm has a bond rating, and whether or not the firm is covered by ExecuComp (meaning that it is or was in the S&P 1500). I find that misreporting is more likely for larger firms, those with bond ratings, and those included in the Execucomp database. These results suggest that firms who are subject to the greatest extent of external monitoring are the most likely to misreport. These results are consistent with a body of literature that includes Abarbanell and Lehavy (2003), Bartov et al. (2002), Burgstahler and Eames (2006), Erickson and Wang (1999), Kasznik (1999), Richardson et al. (2002), Teoh et al. (1998), and others who find evidence to suggest that managers will

[12]My proxy for bank debt is a measure of short-term debt. Regardless of the lender, the renewable nature of short-term debt leads to increased monitoring of the borrower, and an increased need to uphold loan covenants. Thus the interpretation of results is largely unchanged if this measure is viewed as simply short-term debt rather than bank debt.

misreport in order to satisfy the expectations of outside agents.

The paper proceeds as follows. Section 2 discusses the data and methodology, and in Section 3 I present univariate results. The results of the relations between bank debt and the likelihood of misreporting are presented in Section 4. Section 5 discusses those results for three different cuts on the data and Section 6 concludes.

2. Data and methodology

This paper, like the work of Johnson, Ryan and Tian (2009), Erickson, Hanlon and Maydew (2006), Beasley (1996) and others, is limited to the examination of only misreporting that has been exposed. In the analysis that follows I use restatements as a proxy for misreporting, which is a noisy measure of misreporting, and might best be considered as simply a proxy for whether misreporting has actually occurred. Further, the control sample to which they are compared is likely to contain some observations on firms whose misreporting is undetected, but the presence of these firms in the control sample creates a bias against finding any results, and so should bolster confidence in any significant results that are found.

My proxy for misreporting is the sample of financial statement restatements generated by the General Accounting Office that examines financial statement restatements from 1997 – 2002.[13] I do not include restatements after 2002 due to the adoption of the Sarbanes-Oxley Act (SOX). SOX prompted a large number of restatements, many of which were fairly benign, as firms attempted to ensure that prior years were SOX compliant. Thus, including these early post-SOX years would contribute a non-trivial amount of noise to the data. To generate this sample the GAO searched Lexis-Nexis for press releases including some form of the word "restate", "adjust", "amend" or "revise" within fifty words of "financial statement" or "earnings." As a result, they created a database of firms that announced 919 restatements. Restatements are not uncommon and most are relatively benign. Firms routinely restate financials in response to changes in GAAP, to mergers and acquisitions, or to stock splits. Such restatements are not included in the GAO sample unless they represent some irregularity. Rather, the sample is meant to represent cases of "'aggressive' accounting practices, intentional and unintentional misuse of facts applied to

[13]This sample was augmented to include restatements beginning in 1994. I thank Sudheer Chava and Shane Johnson for providing this additional data.

financial statements, oversight or misinterpretation of accounting rules, and fraud."[14] It is important to note that the GAO sample does include some bona fide errors, and that I in no way mean to suggest that all of the misreporting events in the GAO sample are intentional. However, I do not expect that these bona fide errors should be related to the use of bank debt in any systematic way. Thus the presence of these observations in my sample adds noise to the data and creates a bias against finding a result, and as such should further bolster confidence in the results that I find. This sample includes restatements of both annual and quarterly financial statements, and our analysis treats both of these restatement types equivalently, creating an indicator variable that equals one for firm-years in the database and zero otherwise.

Since this database includes the date the restatement is announced, but not of the misreporting itself, I search Lexis-Nexis for press releases to identify the periods that are being restated. I then collect financial statement data for all firms in the Compustat database for 1994-2002, excluding financials (SIC codes 6000-6199). I omit a firm-year observation if it seems to be erroneous in some way, such as a ratio of bank debt to total debt that is greater than one. All dollar values are adjusted for inflation to 1983 dollars, and most variables are winsorized at the 5% and 95% level to mitigate the influence of outliers.[15]The final sample includes 65,295 firm-year observations on 12,347 firms. Of these firms, 495 are included in the GAO database.[16] The remaining firms in Compustat, those that do not restate their financial statements, represent my control sample.

Because bank debt is not reported in a uniform way, I use two distinct measures of bank debt to test my hypotheses. First, I hand collect data on bank loans from Moody's Industrial Manuals and 10-Ks for 104 of the firms in the GAO restatement database.[17.] Since hand collecting this data for the entire Computstat universe would be prohibitively costly, I instead compare these firms to a set of size- and industry-matched control firms. I match first on size, requiring that the matched firm be within 30% of the market value of equity as the sample firm. For industry, I look first for matches within the same three-digit

[14]U.S. General Accounting Office, 2002, Financial statement restatements: Trends, market impacts, regulatory responses, and remaining challenges, Report to Chairman, Committee on Banking, Housing, and Urban Affairs, page 76.

[15] Some variables are naturally bound (for example, changes are bound below at -1) and as such are not winsorized since there are no extreme outliers.

[16] The GAO specified no restrictions regarding which firms would be included in their database. The sample size declines primarily because I require firms to be in Compustat and I discard financial firms.

[17] I use the first year in which the firm misreports and search for data on every firm included in my sample. The sample size decreases because the sample firm's 10-K cannot be found, an appropriate matched control firm cannot be found, or the control firm's 10-K cannot be found.

SIC code, and if a match cannot be found then I look within the same two-digit SIC code. I use the best match for ninety-seven of the restating firms, and the second-best match for the remaining seven firms due to missing data.

Second, I use a proxy for bank debt defined by Slovin et al. (1990). This proxy is calculated as debt in current liabilities (Compustat data # 34) less the current portion of long-term debt (Compustat data # 44). The first term includes most commercial bank debt, but it may also include other forms of short-term debt due to non-bank entities, and should be an effective proxy for the degree to which banks and other creditors monitor the firm. Short-term debt is subject to greater monitoring by the lender for a number of reasons. First, the process of renewing a loan requires the lender to periodically re-evaluate the borrower's credit worthiness and choose whether or not to provide capital (Fama, 1985). Second, if the lender decides not to renew the loan, the result could be bankruptcy or liquidation of the firm (Diamond, 1991a). Because lenders prefer to lend to creditworthy borrowers and to avoid inefficient liquidation, those who provide short-term financing have greater incentives to monitor than those who provide long-term debt. Finally, Manove, Padilla and Pagano (2001) argue that collateral and monitoring are substitutes. Berger and Udell (1995) and Voordeckers and Steijvers (2006) find that firms with longer relationships with their lending banks are less likely to pledge collateral. To the extent that reputation and monitoring are substitutable (Diamond, 1991b), this result is consistent with the Manove et al. (2001) model. Because short-term debt is less likely to be collateralized than long-term debt (Qian and Strahan, 2005), it should be subject to a higher degree of monitoring since the lender has no recourse should the borrower become unable to pay. Because of this increased monitoring, much of my interpretation of the results is unchanged if this measure is viewed as simply short-term debt.

Slovin et al. (1990) then scale this measure by the market value of the firm's equity. Since in this study I investigate accounting manipulations, such scaling is problematic. If the purpose or consequence of the misreporting is to inflate the firm's stock price, such an increase will cause a decrease in the ratio of bank debt to market value of equity. This would give the appearance of reducing the firm's use of bank debt, which would obscure the hypothesized relations between bank debt and misreporting. I therefore scale instead by the book value of the firm's assets. Although book value of assets can be manipulated through misreporting, I expect that the effect should be smaller than it would be for market value of

equity since it is not subject to interpretation by market participants. Regardless of the scaling, the resulting ratio provides the same underlying intuition in that a higher value should indicate greater monitoring on the part of banks and other short-term creditors. Both the hand-collected measure of bank debt and the proxy for bank debt are scaled in this way.

To estimate whether the likelihood of misreporting depends on firms' use of bank debt I estimate logit regressions and control for a number of variables that have been found to influence the likelihood of misreporting. Richardson, Tuna and Wu (2002) find restating firms have higher levels of debt than those that do not restate, therefore Erickson et al. (2006) include leverage as a control variable in their analysis. Their measure of leverage, total debt (Compustat data # 34 + #9) scaled by total assets, includes the proxy for bank debt described above that is the key variable of interest in this paper. So to control for the total leverage effect I partition total debt into two parts, the bank debt variable already described plus other debt (long-term debt including the current portion, Compustat data # 44 + #9), which is also scaled by total assets. In addition, I use the following control variables, most of which are described by Erickson et al. (2006).

Numerous studies find that the motive for misreporting is to conceal financial distress or declining performance (Johnson et al., 2009), so I control for financial performance using sales growth (measured in the year prior the alleged incident for misreporting firms) and the three-year change in the ratio of operating income before depreciation to total assets (industry-adjusted). I also include Altman's Z to control for financial distress (Altman, 1968).

Equity markets have requirements with which firms must comply, and the longer a firm has been publicly traded the more likely it meets these requirements (Beasley, 1996). Alternatively, firms may manage earnings prior to an initial public offering to increase their offer price, and reverse the effects with a restatement following the IPO (Teoh, Wong and Rao, 1998). Diamond (1991b) argues that firms develop reputations by repaying bank loans over time, suggesting that firm age also proxies for reputation. Young firms, with less reputation, have less to lose so the incentives to report accurately provided by bank monitoring are smaller. These papers all suggest a negative relation between the likelihood of misreporting and firm age, so I include age as a control variable.

Erickson and Wang (1999) suggest that firms manipulate earnings in order to raise stock prices prior to acquisitions, so I include an indicator variable to control for an acquisition

having occurred. Larger firms are subject to more attention from outsiders such as analysts and the media. This attention may impact the market reaction to restatement (Palmrose, Richardson and Scholz, 2004).

Unpredictable environments make monitoring more difficult and expensive, yet more necessary (Demsetz and Lehn, 1985), as such environments can facilitate misreporting. To control for this lack of predictability, I include the volatility of stock returns for the prior sixty months as a proxy.

Diamond (1991b) suggests that reputation can substitute for monitoring by external agents such as banks, both of which should reduce the likelihood of misreporting. Firms with greater reputation have access to public debt markets, so as an additional proxy for reputation I use an indicator variable defined by Denis and Mihov (2003) that identifies firms that have bond ratings.

If a firm-year observation is missing one of these control variables, I set the variable equal to zero and create a dummy variable that equals one if the variable is missing and zero otherwise. This allows me to retain these observations for the purpose of estimating the effects of the other variables. These dummy variables rarely generate significant coefficients, and so are excluded from the tables in the interest of brevity. Their significance is discussed in the text.

2.1 Univariate Results

Table 1 reports summary statistics and the results of difference in means tests of firms in the GAO restatement database versus control firms, which are those firms in Compustat that do not restate. Restating firms differ from control firms for most of the variables I use. On average, there is more bank debt (p-value = 0.01) and more non-bank debt (p-value = 0.00) outstanding at misreporting firms.

The average misreporting firm has higher sales growth (p-value = 0.00), consistent with the use of aggressive accounting to give the appearance of consistent growth in sales. Firms that misreport are on average older (p-value = 0.00) and larger (p-value = 0.00) than control firms. If age and size proxy for reputation, then these results are inconsistent with Diamond (1991b) who suggests that reputation reduces the need for external monitoring. Also inconsistent with Diamond (1991b) is that misreporting firms are more likely to have a bond rating (p-value = 0.00). Finally, misreporting firms also have more volatile stock returns (p-value = 0.00) than control firms on average, consistent with misreporting being

more prevalent when monitoring is difficult due to an unpredictable environment (Demsetz and Lehn, 1985).

3. Relations between bank debt and the likelihood of misreporting

3.1 Conditional logit results using hand-collected bank debt

My first test uses hand-collected data from firms' annual reports. I compare firms in the GAO database to a set of size- and industry-matched control firms. Since I have matched pairs, I use a conditional logit model for this analysis. The drawback of using this methodology is that the unconditional probability of misreporting is dramatically over-stated (by construction it is 50%), thus interpretation of the results is limited to the sign and significance, but not the magnitude, of the coefficients. Table 2 reports these results. The coefficient on bank debt is positive (p-value = 0.01), inconsistent with the bank monitoring literature. The coefficient on other debt is also positive (p-value = 0.00). Among the control variables, only sales growth (p-value = 0.09) and the dummy variable indicating that the firm does not have a bond rating (p-value = 0.00) generate significant coefficients, and both are of the predicted sign. The dummy variable indicating that the three year change in OIBD is missing generates a positive coefficient (p-value = 0.02).

3.2 Logit results using a proxy for bank debt

My second test uses a logit model to compare firms in the GAO database to all other firms in the Compustat universe, using the proxy for bank debt defined by Slovin et al. (1990). In a nonlinear model like the logit, the coefficient estimate is not the partial derivative. Rather the partial derivative of x is a function of x, thus the effect of a unit change in x differs depending on the level of x, and can depend on the values of all variables in the model. I therefore report, in addition to coefficients, marginal effects (partial derivatives) for which I set all explanatory variables to their sample means except dichotomous variables that are set to zero. These marginal effects allow for interpretation of the economic significance of my results.

The coefficient on bank debt is positive (p-value = 0.00). The marginal effect of 0.03, when multiplied by the standard deviation of 0.05 reported in Table 1, suggests that for a standard deviation increase in bank debt, the predicted probability of misreporting increases by 0.15%, holding all other variables constant. Economically this is quite significant, as it represents a substantial increase over the unconditional probability of misreporting of

1.12%.[18]

The coefficient on other debt is also positive (p-value = 0.00) with a marginal effect of 0.01. When multiplied by the standard deviation of other debt of 0.23 from Table 1, this suggests that holding all other variables constant, a standard deviation increase in other debt increases the predicted probability of misreporting by 0.23%. The coefficients of five of the control variables are significant. Both sales growth (p-value = 0.00) and volatility (p-value = 0.00) are positively related to the likelihood of misreporting, as predicted and consistent with the extant literature. Altman's Z (p-value = 0.00), firm age (p-value = 0.05) and size (p-value = 0.00) are also positively related to the likelihood of misreporting, contrary to my predictions. The dummy variables indicating that the three year change in OIBD is missing (p-value = 0.00) and that Altman's Z is missing (p-value = 0.00) both generate negative coefficients.

These results fail to support the bank monitoring literature and suggest that monitoring by banks is insufficient to deter or detect financial statement misreporting. Rather, these results are consistent with the debt covenant hypothesis of the earnings management literature, which suggests managers will misreport in order to uphold loan covenants or to lower borrowing costs.

These results are robust to the exclusion of quarterly restatements, and to the inclusion of industry and year dummy variables. I have omitted these results from the tables in the interest of brevity. The results using the proxy for bank debt are similar to those using hand-collected data on bank debt, and so I use the proxy for the remaining tests in the paper. This facilitates the use of a much larger dataset and allows me to consider the economic significance of the results, rather than simply sign and significance of the coefficients.

3.3 Alternative interpretation

An alternative interpretation of these results is also possible since I use restating as a proxy for misreporting, thus data are available only for those firms whose misreporting is detected and exposed. So a positive relation between bank debt and restatements would also obtain because bank monitoring effectively detects misreporting. That is, a positive relation could exist because firms with bank debt are more likely to misreport, or because they are more likely to restate. To unequivocally distinguish between these alternative

[18]This calculation is based on firm-year observations. Based on the number of firms in the sample, the unconditional probability of misreporting is 4.07%.

interpretations requires a sample of firms whose misreporting is undetected, but such a sample does not exist. There are however firms whose misreporting is initially undetected. I use data from the GAO restatement database and Lexis-Nexis newswire searches to identify these firms. Many firms restate in the same year (for misreporting of quarterly financial statements) or the following year, and I use these firms as a sample of detected misreporting. Those firms that restate more than one year after misreporting I use as a sample of initially undetected misreporting. I compare this sample of initially undetected misreporting to both the sample of detected misreporting, and to the control group (those firms that do not restate), to distinguish between the following:

Alternate Hypothesis 1A: The likelihood of misreporting is increasing in bank debt, because borrowing firms' managers have incentives to misreport to uphold covenants or to lower borrowing costs.

Alternate Hypothesis 1B: The likelihood of restating is increasing in bank debt, because effective bank monitoring helps to detect and expose misreporting.

If the likelihood of misreporting is positively related to bank debt, consistent with the debt covenant hypothesis, then I should observe no differences in the use of bank debt between the sample of initially undetected misreporting versus the sample of detected misreporting.[19.] I should observe a greater use of bank debt among the sample of initially undetected misreporting compared to the control group.

If the likelihood of restating is positively related to bank debt, consistent with the bank monitoring literature, then I should observe a greater use of bank debt in the sample of detected misreporting compared to the sample of initially undetected misreporting. I should observe no difference in the use of bank debt between the sample of initially undetected misreporting compared to the control group.

I test these relations using a logit regression analysis with the same control variables discussed in the last section. In Table 3 Panel A, I compare firms whose misreporting is detected versus firms whose misreporting is initially undetected. The dependent variable

[19]A third scenario is that managers of firms financed with bank debt, knowing they will incur the scrutiny of bank monitoring, must engage in relatively more complex accounting manipulations in order to avoid detection by the bank. If this were the case, I should observe a greater use of bank debt among the group of firms whose misreporting is initially undetected both versus those whose misreporting is detected, and versus the control group.

takes a value of one if misreporting is detected immediately and a value of zero otherwise. I find no significant difference in the use of bank debt (p-value = 0.92) or other debt (p-value = 0.75) among these firms. The three-year change in OIBD is negatively related to the likelihood of detection (p-value = 0.01), whereas the merger indicator variable (p-value = 0.03) and volatility of stock returns (p-value = 0.05) are both positively related to the likelihood of detection. The dummy variable indicating missing values of the three-year change in OIBD is also significant (p-value = 0.00). None of the other control variables generates a significant coefficient.

I next compare the sample of initially undetected misreporting to the control group and report the results in Table 3 Panel B. The dependent variable takes a value of one if misreporting is initially undetected and a value of zero for firms in the control group (those that do not restate). I find a significant difference in the use of bank debt among these firms. Firms whose misreporting is initially undetected use more bank debt and more other debt (p-values = 0.00) than control firms. Among the control variables, positive coefficients are generated by Altman's *Z* (p-value = 0.02), sales growth (p-value = 0.00), firm age (p-value = 0.00), total assets (p-value = 0.00) and volatility (p-value = 0.00). These results accord with the main results reported in Table 2. In addition, dummy variables indicating that the firm has engaged in a merger (p-value = 0.05), and that the firm does not have a bond rating (p-value = 0.02) both generate negative coefficients. The dummy variable indicating missing values of the three-year change in OIBD is also significant (p-value = 0.06).

Overall these results confirm my interpretation of the results presented in Table 2. Firms that make greater use of bank debt are more likely to misreport, whether that misreporting is detected or not, than firms with less bank debt. And firms whose misreporting is detected do not have more bank debt outstanding than firms whose misreporting is initially undetected, as would be expected if misreporting was being detected through bank monitoring. Together these results suggest that it is misreporting per se, and not restating, that is positively related to bank debt, which is inconsistent with the bank monitoring literature.

4. The relations between bank debt and misreporting when the extent of external monitoring differs

If my interpretation of the results thus far is correct, and firm managers are more likely to misreport if they have more bank debt in their capital structures, then it may still be some types of firms are more likely to do so than others. Theoretically, the temptation to misreport that arises from the use of bank debt should be mitigated by the presence of other external monitors such as analysts. My next set of tests cuts the data on three different firm characteristics, which should serve as proxies for the extent to which these firms are monitored.

4.1 Firms covered by ExecuComp

I first cut the sample based on whether firms are covered by ExecuComp. In order to be covered, a firm must have been included in the S&P 1500 at some point in time. To be included in the S&P 1500, firms are representative of their industries, but must also meet certain standards, such as maintaining adequate liquidity and a reasonable stock price, and having four consecutive quarters of positive earnings. Once a firm is included in the index, it is likely to gain more exposure in the media and be subjected to greater analyst coverage. Perhaps most importantly, once added to the S&P 1500, the firm's stock is purchased by many fund managers whose funds track the index. Therefore I cut my sample into two groups based on whether or not firms are covered by ExecuComp and repeat the logit analysis.

These results are reported in Table 4. For firms in ExecuComp, the coefficients on bank debt (p-value = 0.00) and other debt (p-value = 0.00) are once again positive, consistent with the results for the full sample reported in Table 2. When multiplied by the standard deviation of 0.05 reported in Table 1, the marginal effect of 0.12 suggests that for a standard deviation increase in bank debt, the predicted probability of misreporting increases by 0.60%, holding all other variables constant. Given that the unconditional probability of misreporting is only 1.12%, the economic significance of this relation is considerable. In fact, it is larger for this sub-sample than for any other sub-sample that I test.

Volatility (p-value = 0.00) generates a significant coefficient consistent with my prediction. The coefficients on Altman's Z (p-value = 0.09), sales growth (p-value = 0.00), age (p-value = 0.01), and total assets (p-value = 0.03) are all positive, which is not predicted sign. The merger indicator variable (p-value = 0.01) generates a negative coefficient,

which is also contrary to that predicted.

In Table 4 Panel B I report results for the subset of firms that are not covered by ExecuComp. I find an insignificant coefficient on bank debt (p-value = 0.70). The coefficient on other debt (p-value = 0.00) is once again positive, consistent with the results for the full sample reported in Table 2. Four of the control variables generate significant coefficients. Sales growth (p-value = 0.00) and the lack of a bond rating (p-value = 0.01) are both positively related to the likelihood of misreporting, as predicted. Firm age (p-value = 0.07) generates a negative coefficient, as predicted. Finally, the coefficient on Altman's Z (p-value = 0.01) is positive contrary to my prediction. The dummy variables indicating that the three year change in OIBD is missing (p-value = 0.00) and that Altman's Z is missing (p-value = 0.00) both generate negative coefficients.

4.2 Firms with bond ratings

My next cut on the data is whether or not firms have a bond rating. There is clearly an inherent selection bias, as firms must want to borrow publicly (that is, they self-select into this sample), but firms of lower credit quality will not be able to do so. Once a firm gains access to public debt markets, it also subjects itself to monitoring by the participants in those markets.

The results for firms with bond ratings are reported in Table 5 Panel A. Using the proxy for bank debt, the coefficients on bank debt (p-value = 0.00) and other debt (p-value = 0.00) are once again positive, consistent with the results for the full sample reported in Table 2. The marginal effect of 0.07, when multiplied by the standard deviation of 0.05 reported Table 1, suggests that for a standard deviation increase in bank debt, the predicted probability of misreporting increases by 0.35%, all else constant. The economic significance of this effect is substantial when considered relative to the unconditional probability of misreporting of 1.12%.

The coefficient on volatility (p-value = 0.00) is positive consistent with my prediction. The coefficients on Altman's Z (p-value = 0.01), sales growth (p-value = 0.00), age (p-value = 0.00) and the merger indicator variable (p-value = 0.03) are significant but not of the predicted sign. The dummy variable indicating that the three-year change in OIBD is missing generates a negative coefficient (p-value = 0.06).

In Table 5 Panel B I report results for the subset of firms that do not have bond ratings. Using the proxy for bank debt, I find a positive coefficient on bank debt (p-value = 0.09).

Although this is statistically significant, the marginal effect is only 0.01. Multiplied by the standard deviation of 0.05 reported in Table 1, this suggests that for a standard deviation increase in bank debt, the predicted probability of misreporting increases by just 0.05%, holding all other variables constant. This increase seems economically trivial and is markedly smaller than that of the other sub-samples that I test.

The coefficient on other debt (p-value = 0.03) is once again positive, consistent with the results for the full sample reported in Table 2. Four of the control variables generate significant coefficients. Altman's Z (p-value = 0.03), sales growth (p-value = 0.00) and total assets (p-value = 0.01) are all positively related to the likelihood of misreporting, contrary to my predictions. Volatility (p-value = 0.00) also generates a positive coefficient, as predicted. The dummy variables indicating that the three year change in OIBD is missing (p-value = 0.00) and that Altman's Z is missing (p-value = 0.00) both generate negative coefficients.

For firms with bond ratings, for a standard deviation increase in bank debt, the predicted probability of misreporting increases by 0.35%, holding all other variables constant. For firms without bond ratings, a standard deviation increase in bank debt increases the predicted probability of misreporting by only 0.05%, all else constant.

4.3 Firm Size

Because larger firms are subject to greater scrutiny by media, analysts, and other outside agents, I cut the sample based on firm size (total assets). To make this cut, I rank all firms in the sample by book value of assets, and cut the sample into quintiles. I use firms in the top (bottom) quintile as the set of large (small) firms. The difference in sample size here is telling; the quintile of largest firms includes 223 observations of misreporting firms whereas the quintile of smallest firms includes only 29.

The results for larger firms are reported in Table 6 Panel A. Using the proxy for bank debt, the coefficients on bank debt (p-value = 0.00) and other debt (p-value = 0.00) are once again positive, consistent with the results for the full sample reported in Table 2. The marginal effect of 0.05, when multiplied by the standard deviation of 0.05 reported in Table 1, suggests that for a standard deviation increase in bank debt, the predicted probability of misreporting increases by 0.25%, all else constant. This is an economically significant relation given the unconditional probability of misreporting of 1.12%, and is nearly on par with the effect of having a bond rating as reported in the prior section.

The coefficient on volatility (p-value = 0.00) is positive consistent with my prediction. Five other control variables generate significant coefficients that are not of the predicted sign. Altman's Z (p-value = 0.01), three-year change in OIBD to total assets (p-value = 0.09), sales growth (p-value = 0.00), age (p-value = 0.00) and the bond rating indicator variable (p-value = 0.04) all have positive coefficients. The dummy variable indicating that the three year change in OIBD is missing generates a negative coefficient (p-value = 0.04) and that volatility is missing generates a positive coefficient (p-value = 0.05).

In Table 6 Panel B I report results for the subset of smaller firms. The dummy variable indicating that the firm has a bond rating is dropped for this subsample because it is equal to one for all misreporting firms. Using the proxy for bank debt, I find an insignificant coefficient on bank debt (p-value = 0.69). The coefficient on other debt (p-value = 0.02) is once again positive, consistent with the results for the full sample reported in Table 2. Altman's Z (p-value = 0.03) generates a positive coefficient, contrary to my prediction. Firm age (p-value = 0.04) and the merger indicator variable (p-value = 0.0) are both related to the likelihood of misreporting as predicted.

In sum, the results of this section are consistent with a body of literature that suggests managers may misreport in an attempt to satisfy the expectations for performance set by outside agents. Theoretically, Dye (1988) suggests that the firm's existing shareholders, who wish to sell their shares, demand earnings management to alter new investors' perceptions of firm value. Trueman and Titman's (1988) model suggests that managers can alter debt holders' perceptions regarding the volatility of earnings, thereby lowering the perceived likelihood of bankruptcy, which lowers the cost of borrowing. Degeorge, Patel and Zeckhauser (1999) develop a model in which managers manipulate earnings to meet analysts' forecasts, and provide empirical evidence consistent with their predictions. Numerous other empirical papers also find evidence of earnings management in response to analysts' forecasts (Abarbanell and Lehavy, 2003; Bartov, Givoly and Hayn, 2002; Burgstahler and Eames, 2006; Kasznik, 1999; and others), although recent work by Mintchik, Wang and Zhang (2014) suggests that only certain types of institutional investors may find predictable earnings to be appealing. My results complement these by suggesting that the greater the extent of external monitoring, the greater the temptation for managers to misreport. It appears that larger firms, those with bond ratings, and those included in the S&P 1500, that is, firms that are subject to the most attention and scrutiny by outside agents, are the most

likely to misreport.

5. Conclusion

I find a positive relation between firms' use of bank debt and the likelihood that they engage in financial statement misreporting. In a test of detected versus initially undetected misreporting, I find evidence to suggest that it is misreporting per se, and not its subsequent detection and restatement, that is related to its use of bank debt. In sum, my results are consistent with a large body of literature (Defond and Jiambalvo, 1994; Dichev and Skinner, 2002; Dye, 1988; Efendi et al., 2007; Jha, 2013; Stanley and Sharma, 2011; Sweeney, 1994 and others) that suggests managers may misreport in order to avoid loan default or improve loan contracting terms.

I further find that this positive relation between bank debt and misreporting is the strongest among firms that are larger, have bond ratings, or are included in the ExecuComp database (meaning that they are or were in the S&P 1500). These results accord with a large body of literature (Abarbanell and Lehavy, 2003; Bartov et al, 2002; Burgstahler and Eames, 2006; Degeorge et al., 1999; Erickson and Wang, 1999; Kasznik, 1999; Richardson et al., 2002; Teoh et al., 1998; Trueman and Titman, 1988 and others) that suggests managers may misreport in order to live up to the expectations for their performance set by other (non-bank) outside monitors such as analysts and the media. The results of this paper complement and extend this literature by suggesting that the firms most likely to misreport their financial statements are those that are subjected to the greatest degree of attention and scrutiny by these outside agents.

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Table 1: Descriptive Statistics and Difference in Means Tests for Control and Test Variables

Variable	Mean	Standard Deviation	Upper Quartile	Median	Lower Quartile	Difference in Means, *p*-value
Bank debt proxy						
GAO sample	0.03	0.05	0.02	0.00	0.00	0.01
Control sample	0.02	0.05	0.01	0.00	0.00	
Other debt						
GAO sample	0.25	0.23	0.39	0.22	0.03	0.00
Control sample	0.22	0.22	0.35	0.16	0.02	
Altman's Z						
GAO sample	23.62	49.55	11.17	4.41	2.44	0.38
Control sample	21.91	48.46	11.04	3.77	1.67	
3-yr OIBD/TA						
GAO sample	-0.01	0.11	0.04	-0.00	-0.06	0.31
Control sample	-0.00	0.13	0.06	-0.00	-0.06	
Sales growth						
GAO sample	0.25	0.56	0.36	0.11	-0.02	0.00
Control sample	0.15	.058	0.26	0.05	-0.10	
Age of firm						
GAO sample	14.32	15.83	18	8	4	0.00
Control sample	11.39	12.77	15	7	3	
Merger						
GAO sample	0.12	0.32	0	0	0	0.35
Control sample	0.11	0.31	0	0	0	
Total Assets						
GAO sample	792.50	1365.88	709.71	137.86	40.36	0.00
Control sample	522.88	1138.04	332.78	59.04	11.49	
Volatility						
GAO sample	0.28	0.14	0.42	0.25	0.16	0.00
Control sample	0.23	0.14	0.32	0.20	0.01	
Unrated						
GAO sample	0.76	0.43	1	1	1	0.00
Control sample	0.82	0.38	1	1	1	

"GAO sample" is a set of firms that restated their financial statements (n =730 firm-years). Firms in Compustat that are not accused of misreporting are included as control firms (n = 64,565 firm-years). Bank debt proxy is debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise.

Table 2: Results of Logit Regressions Comparing GAO Sample to Control Firms

	Bank Debt Coefficient	Proxy Coefficient	Proxy Marginal effect
Bank debt	1.27	2.56	0.03
	(0.01)	(0.00)	(0.00)
Other debt	1.15	1.03	0.01
	(0.00)	(0.00)	(0.00)
Altman's Z	0.00	0.00	0.00
	(0.30)	(0.00)	(0.00)
3-yr OIBD/TA	0.05	-0.17	-0.00
	(0.94)	(0.59)	(0.59)
Sales growth	0.24	0.38	0.00
	(0.09)	(0.00)	(0.00)
Age	0.01	0.01	0.00
	(0.17)	(0.05)	(0.05)
Merger indicator	-0.02	-0.14	-0.00
	(0.91)	(0.29)	(0.20)
Total assets	0.00	0.00	0.00
	(0.18)	(0.00)	(0.00)
Volatility	0.81	2.20	0.02
	(0.40)	(0.00)	(0.00)
Unrated	0.84	0.16	0.00
	(0.00)	(0.16)	(0.14)
Intercept		-5.37	
		(0.00)	
LR ^{2} (14)	50.78	251.89	
	(0.00)	(0.00)	

"GAO sample" is a set of firms that restated their financial statements (n =730 firm-years). Firms in Compustat that are not accused of misreporting are included as control firms (n = 64,565 firm-years). The hand-collected sample includes 104 matched pairs and uses a conditional logit model. Dependent variable takes a value of one if firm issued a restatement. Bank debt proxy is debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. Table reports coefficient estimates and marginal effects, with *p*-values in parentheses.

Table 3: Results of Logit Regressions: GAO Sample, Detected vs. Undetected Misreporting

	Panel A: Detected vs. Initially undetected misreporting		**Panel B: Initially undetected misreporting vs. Control firms**	
	Coefficient estimate	**Marginal effect**	**Coefficient estimate**	**Marginal effect**
Bank debt proxy	-0.16	-0.04	5.13	0.00
	(0.92)	(0.92)	(0.00)	(0.00)
Other debt	0.13	0.03	1.62	0.00
	(0.75)	(0.75)	(0.00)	(0.00)
Altman's Z	0.00	0.00	0.01	0.00
	(0.35)	(0.35)	(0.02)	(0.02)
3-yr OIBD/TA	-1.90	-0.46	0.74	0.00
	(0.01)	(0.01)	(0.37)	(0.38)
Sales growth	0.02	0.00	0.57	0.00
	(0.91)	(0.91)	(0.00)	(0.00)
Age	-0.00	-0.00	0.02	0.00
	(0.85)	(0.85)	(0.00)	(0.00)
Merger indicator	0.57	0.13	-0.62	-0.00
	(0.03)	(0.02)	(0.05)	(0.02)
Total assets	-0.00	-0.00	0.00	0.00
	(0.25)	(0.25)	(0.00)	(0.01)
Volatility	1.29	0.31	5.43	0.00
	(0.05)	(0.05)	(0.00)	(0.00)
Unrated	0.09	0.02	-0.51	-0.00
	(0.70)	(0.70)	(0.02)	(0.06)
Intercept	-0.16		-8.07	
	(0.65)		(0.00)	
LR ^{2} (14)	39.86		268.69	
	(0.00)		(0.00)	

A restatement is classified as *detected* if the restatement occurs within one year of the misreporting (n = 423), and as *initially undetected* if the restatement occurs more than one year after the misreporting (n = 307). Firms in Compustat that are not accused of misreporting are included as control firms (n = 64,565 firm-years). Bank debt proxy is debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. Table reports coefficient estimates followed by marginal effects. Table reports coefficient estimates and marginal effects, with p-values in parentheses.

Table 4: Results of Logit Regressions Comparing GAO Sample to Control Firms Cut on ExecuComp Coverage

	Proxy Coefficient	Proxy Marginal effect	Proxy Coefficient	Proxy Marginal effect
	Panel A: Firms in ExecuComp		*Panel B: Firms not in ExecuComp*	
Bank debt proxy	7.18	0.12	0.37	0.00
	(0.00)	(0.00)	(0.70)	(0.70)
Other debt	1.64	0.03	0.82	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Altman's Z	0.00	0.00	0.00	0.00
	(0.09)	(0.09)	(0.01)	(0.00)
3-yr OIBD/TA	-0.53	-0.01	-0.14	-0.00
	(0.48)	(0.48)	(0.68)	(0.68)
Sales growth	0.76	0.01	0.28	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.01	0.00	-0.01	-0.00
	(0.01)	(0.01)	(0.07)	(0.07)
Merger indicator	-0.54	-0.01	0.04	0.00
	(0.01)	(0.00)	(0.77)	(0.78)
Total assets	0.00	0.00	0.00	0.00
	(0.03)	(0.03)	(0.88)	(0.88)
Volatility	3.91	0.06	0.59	0.00
	(0.00)	(0.00)	(0.34)	(0.34)
Unrated	-0.06	-0.00	0.49	0.00
	(0.68)	(0.68)	(0.01)	(0.00)
Intercept	-6.35		-5.02	
	(0.00)		(0.00)	
LR ^{2} (14, 20)	121.37		124.61	
	(0.00)		(0.00)	

"GAO sample" is a set of firms that restated their financial statements (n =260 firm-years ExecuComp sample; n =470 firm-years non-ExecuComp sample). Firms in Compustat that are not accused of misreporting are included as control firms (n = 12,549 firm-years ExecuComp sample; n = 52,016 firm-years non-ExecuComp sample). Dependent variable takes a value of one if firm issued a restatement. Bank debt proxy is debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. Table reports coefficient estimates and marginal effects, with p-values in parentheses.

Table 5: Results of Logit Regressions Comparing GAO Sample to Control Firms Cut on Access to Public Debt Markets

	Proxy Coefficient	Proxy Marginal effect	Proxy Coefficient	Proxy Marginal effect
	Panel A: Firms with Bond Ratings		*Panel B: Firms without Bond Ratings*	
Bank debt proxy	6.64	0.07	1.45	0.01
	(0.00)	(0.00)	(0.09)	(0.09)
Other debt	3.08	0.03	0.50	0.00
	(0.00)	(0.00)	(0.03)	(0.03)
Altman's Z	0.01	0.00	0.00	0.00
	(0.01)	(0.01)	(0.03)	(0.03)
3-yr OIBD/TA	-1.09	-0.01	-0.10	-0.00
	(0.32)	(0.32)	(0.74)	(0.74)
Sales growth	0.88	0.01	0.29	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.02	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.34)	(0.34)
Merger indicator	-0.55	-0.00	-0.04	-0.00
	(0.03)	(0.01)	(0.76)	(0.76)
Total assets	0.00	0.00	0.00	0.00
	(0.71)	(0.71)	(0.00)	(0.00)
Volatility	4.04	0.04	1.38	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Intercept	-6.94		-4.74	
	(0.00)		(0.00)	
LR ^{2} (12)	154.79		158.97	
	(0.00)		(0.00)	

"GAO sample" is a set of firms that restated their financial statements (n =179 firm-years rated sample; n = 551 firm-years unrated sample). Firms in Compustat that are not accused of misreporting are included as control firms (n = 11,673 firm-years rated sample; n = 52,892 firm-years unrated sample). Dependent variable takes a value of one if firm issued a restatement. Bank debt proxy is debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Table reports coefficient estimates and marginal effects, with p-values in parentheses.

Table 6: Results of Logit Regressions Comparing GAO Sample to Control Firms

Cut on Firm Size as a Proxy

	Proxy Coefficient	Proxy Marginal effect	Proxy Coefficient	Proxy Marginal effect
	Panel A: Large Firms		*Panel B: Small Firms*	
Bank debt proxy	4.11	0.05	-1.60	-0.00
	(0.00)	(0.00)	(0.69)	(0.69)
Other debt	1.38	0.02	2.04	0.00
	(0.00)	(0.00)	(0.02)	(0.02)
Altman's Z	0.01	0.00	0.01	0.00
	(0.01)	(0.01)	(0.03)	(0.04)
3-yr OIBD/TA	1.77	0.02	-1.19	-0.00
	(0.09)	(0.09)	(0.26)	(0.26)
Sales growth	0.71	0.01	-0.43	-0.00
	(0.00)	(0.00)	(0.15)	(0.16)
Age	0.02	0.00	-0.08	-0.00
	(0.00)	(0.00)	(0.04)	(0.02)
Merger indicator	-0.26	-0.00	1.79	0.01
	(0.23)	(0.19)	(0.00)	(0.08)
Total assets	0.00	0.00	0.07	0.00
	(0.56)	(0.56)	(0.47)	(0.47)
Volatility	3.47	0.05	-0.39	-0.00
	(0.00)	(0.00)	(0.89)	(0.89)
Unrated	0.32	0.00		
	(0.04)	(0.05)		
Intercept	-6.16		-6.03	
	(0.00)		(0.00)	
LR ^{2} (13)	119.43		33.50	
	(0.00)		(0.00)	

Large (small) firms are those that are in the top (bottom) quintile of book value of assets. "GAO sample" is a set of firms that restated their financial statements (n = 223 firm-years large sample; n = 29 firm-years small sample). Firms in Compustat that are not accused of misreporting are included as control firms (n = 12,834 firm-years large sample; n = 13,028 firm-years small sample). Dependent variable takes a value of one if firm issued a restatement. Bank debt proxy is debt in current liabilities less the current portion of long-term debt, scaled by total assets. Other debt is long-term debt including the current portion, scaled by total assets. Altman's Z is Altman's (1968) proxy for financial distress risk. 3-year OIBD/TA is the change in the industry-adjusted ratio of operating income before depreciation to total assets over the three years preceding the event. Sales growth is the percent change in sales versus the prior year (measured as of the year preceding the alleged incident for misreporting firms). Age of firm is the first year the firm is publicly traded subtracted from the observation year. Merger is an indicator variable that takes the value of one in the event of an acquisition and zero otherwise. Total Assets is book value of assets (measured as of the year preceding the alleged incident for misreporting firms). Volatility is the standard deviation of returns over the previous sixty months. Unrated is indicator variable that takes a value of one if the firm has no existing debt rating, zero otherwise. Table reports coefficient estimates and marginal effects, with p-values in parentheses.