HOW PERVASIVE IS CORPORATE FRAUD?

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ABSTRACT

We provide a new estimate of the proportion of corporate fraud that normally goes undetected. To identify the potential hidden part of the 'iceberg' we take advantage of Arthur Andersen's demise, which forces companies to change auditors and increases the likelihood to expose preexisting frauds. This experiment suggests that only one third of frauds are detected in normal times, and leads us to infer that in the 1996-2004 period one large publicly-traded US firms out of every eight was engaged in fraud. We obtain similar estimates by using alternative approaches. Combining this information with cost estimates suggests that in the 1996-2004 period fraud in large corporations destroyed between $180 and $360 billion a year.

Fifteen years after the passage of Sarbanes-Oxley (SOX) regulation there is still a very active debate about its costs and benefits (Coates and Srinivasan, 2014). While the direct costs are (relatively) easy to quantify, its benefit – a reduction in corporate fraud – is much more difficult to assess. Not only is the reduction in the cost of corporate fraud due to SOX difficult to estimate, but so is the magnitude of the pre-existing problem, i.e., how pervasive and costly fraud was before SOX. If there were just a few rotten apples, large-scale intervention might have been a waste of energy and resources.

While there are some reliable estimates on the cost of fraud (e.g., Karpoff, Lee and Martin (2008)), very little is known about the pervasiveness of fraud: we do not know if the fraud we observe (i.e, the fraud that is exposed) is roughly the whole iceberg, or just its visible tip. The key missing estimate is the detection likelihood (i.e., Pr(*caught|engage*)) that tells us the ratio of the exposed tip to the submerged portion.

In this paper we estimate this detection likelihood by exploiting a natural experiment and some basic probability rules. Our identification strategy relies on situations in which the likelihood of being caught increases to almost one. By comparing detection in normal circumstances to detection in this special circumstance, we produce an estimate of the size of the iceberg below the water. From there, it is easy to derive the unconditional likelihood of engaging in fraud.

Our primary test takes advantage of the natural experiment created by the 2002 demise of Arthur Andersen (AA) following the criminal indictment by the U.S. Department of Justice. That demise suddenly forced all firms that employed AA to seek another auditor. As Tirole (1986) points out, a forced turnover reduces the incentives to collude. Thus, new auditors have incentives to rat out pre-existing frauds to minimize their risk of litigation from former AA clients. This effect is likely to be particularly acute after the Enron and WorldCom accounting scandals, when the market was putting the accounting of all firms under the microscope.

As a primary sample we use the auditor-detected fraud in Dyck, Morse and Zingales (2010), (hereafter DMZ). These frauds are identified off securities class actions that do not settle for less than $3 million. To show the robustness of our main results, we complement the analysis with alternative definitions of "fraud": firm accounting restatements and firms receiving a Securities and Exchange Commission Accounting, Auditing, and Enforcement Release (AAER).

Consistent with our hypothesis, we find that fraud detection by auditors in former AA clients goes up significantly. Using the DMZ definition of fraud we find that firms that replaced AA with a new auditor are 3.2 times more likely to reveal an ongoing accounting fraud than firms that did not experience such a turnover. To be precise, the detection rate is 31%. Making the conservative assumption that all frauds in AA clients were revealed, this implies that the underwater part of the iceberg is more than two times its visible tip.

The detection likelihood estimate is very similar when we look at accounting restatements: 29% for accounting restatements that lead to an SEC investigation and 33% for all restatements. When we look at AAERs, the detection rate is larger (55%). For every fraud that is caught, we find that between 1 and 2 additional frauds go undetected.

All these estimates rely on the validity of the AA-experiment. To test the robustness of our approach we rely on a model of the probability of detection. Conditional on a fraud being committed, the probability it is revealed is a positive function of the incentives and the opportunities for detection. For example, DMZ show that a fraud is more likely to be detected in a company followed by a large number of analysts. Thus, instead of focusing just on the changes in auditors' incentive to detect fraud (as for the AA case), we also look at changes in incentives and opportunities across all potential fraud detectors.

DMZ identify six sources of variations in the incentives/opportunities to detect fraud. By using these six sources of variation, we estimate what happens to the likelihood of detection when all incentives are simultaneously 'high.' By comparing the odds of detection in this 'high' detection state to the odds in a 'normal' state, we derive an alternative estimate of the likelihood of detection. This alternative method produces estimates of the fraction of undetected fraud very similar to those obtained with the AA natural experiment.

We then move to estimating fraud pervasiveness. Using detected fraud in the 1996-2004 period as a benchmark, 4.0% of companies are engaging in fraud at any point in time. It follows that the fraction of large publicly traded corporations engaging in fraud in this period is 13%, as undetected fraud is simply detected fraud grossed up to reflect detection likelihood (i.e., 4.0%/0.311). More generally, fraud pervasiveness depends on the type of fraud, the fraud sample, and other factors. For the sub-component of all frauds that are financial frauds the corresponding estimate is 7.3%. This estimate is similar to that based on restatements involving SEC investigations, and lies between the much higher estimate using accounting restatements as a measure of financial fraud and the lower estimate using AAERs. While the estimated level of pervasiveness is highly dependent on the period used as a benchmark, the multiplier between undetected and detected fraud is not. Thus, the method can be used to estimate the pervasiveness of fraud in other periods, starting from different baselines of detected fraud.

Having estimated the pervasiveness of fraud, we can try to estimate the deadweight losses associated with fraud, detected and undetected. The costs we have in mind are the value destruction over and above poor fundamental performance which might have triggered the fraud in the first place. It is the value destruction due to the drop in reputation caused by the fraud, which will be capitalized in the firms' equity value. As Karpoff and Lott (1993) and Karpoff, Lee and Martin (2008) show (hereafter KLM), these reputational losses can be quite large. In fact, we expect these losses to be larger in countries like the United States, where the governance system is relatively good and thus fraud is unexpected.

We provide a range of fraud costs using both cost estimates from the literature and within-sample cost estimates. We use the KLM estimate for the costs of detected fraud, and the Beneish, Lee and Nichols (2012) estimate for the costs of undetected fraud. To provide validity for the fraud cost estimates, we use a new measure of the cost of detected fraud for firms in our sample, equal to the difference between the enterprise value after the fraud is revealed and what the enterprise value would have been in the absence of fraud. We use the normally-not-detected frauds that arise from the AA experiment to estimate the costs of undetected fraud.

Putting together our estimates of the pervasiveness of fraud with these estimates of the per-firm cost of fraud, we arrive to an expected cost of fraud between 2.0 to 4.0% of equity value of large firms, with the range depending on whether we use costs from the literature or from our sample. The annual cost of fraud among large US corporations is \$181 - \$364 billion.

Our paper builds on a rich literature that measures financial fraud, summarized in Karpoff, Koester, Lee and Martin (2013) (hereafter KKLM). Besides detected fraud, we focus on measuring undetected fraud, as in Wang (2013) and Zakolyukina (2014). In addition, we do not restrict ourselves to financial misrepresentations, but we also look at other types of fraud (e.g. lying about the future). Finally, we build on Karpoff and Lott (1993) and Karpoff, Lee and Martin (2008) to compute the total cost of this fraud.

The rest of the paper proceeds as follows. Section I describes the data and presents summary statistics on caught frauds. Section II provides our primary experiment result, the detection likelihood of fraud, estimated via the demise of Arthur Anderson. Section III takes the detection likelihood and generalizes it to the economy to estimate the pervasiveness of corporate fraud. Section IV validates these estimates by applying a similar logic to a broader set of fraud detectors. Section V provides estimates of the deadweight loss from such frauds for the economy. In Section VI, we discuss the implications for regulation. Section VII concludes.

I. Observed Fraud Incidence: Data and Statistics

Section 10b of the Securities and Exchange Act (1933) and rule 10b-5 promulgated by the SEC characterize a fraud to include any ‘untrue statement of a material fact or to omit to state a material fact.’ Further, the SEC requires an intent to cause reliance, that parties did rely on the information and experienced harm. In going from this definition to identifying firms that committed fraud, we encounter several problems. The most important one is that companies have a very compelling interest in settling without admitting guilt. Directors’ and officers’ insurance does not cover firm management when courts find the firm guilty of security fraud. Thus, all of the cases settle before reaching a court verdict, and settlements almost always involve no admittance of wrongdoing. Thus, even if we refer to the cases as fraud, from a legal point of view we should refer to them as alleged frauds.

KKLM provide a comprehensive overview of the strengths and weaknesses of the most popular databases of financial frauds. By far the most used one includes all financial restatements (as reported in Audit Analytics). Financial restatements are triggered by a misapplication of some accounting rules. As KKLM states, restatements include many non-material cases. For example, Hennes et al. (2008) categorize 73.6% of GAO restatements as unintentional misapplications of GAAP accounting. One potential solution is to restrict the sample to restatements followed by an SEC investigation or to those that Audit Analytics identifies as the result of failure to apply accounting rules, rather than mere clerical errors.

Another solution, followed by Dechow, Ge, Larson and Sloan (2011), is to focus on the companies that were subject to enforcement actions by the U.S. Securities and Exchange Commission (SEC) for allegedly misstating their financial statements (Accounting, Auditing, and Enforcement Releases or AAERs). This sample certainly eliminates all frauds lacking materiality, but may be overly restrictive because the SEC does not have the budget to go after all frauds (e.g. Dechow, Sloan and Sweeney (1996), and Miller (2006)).[1] For example, the SEC decided three times not to investigate Bernie Madoff in spite of evidence brought to its attention by Harry M. Markopolos, a securities industry executive.[2] Thus, this method would severely understate actual fraud.

A reasonable middle-of-the-road compromise is represented by DMZ, who focus on firms subject to securities class action lawsuits, as compiled by the Stanford Securities Class Action Clearinghouse (SSCAC)[3]. Given the structure of the incentives to file a suit, every time in a large company there is a large drop in the share price and the hint of a misrepresentation, specialist attorneys file a suit. Thus, for large (over $750 million in assets) publicly-traded companies, it is highly unlikely that SSCAC would miss any sizeable corporate fraud (Coffee, 1986) and thus this sample would contain all caught fraud.

[1] See AAER-1, 1982 SEC LEXIS 2565, May 17, 1982 and discussion in KKLM.
[2] Tofel, Richard J. "Shadowing a Swindler; Early on, he figured out what Bernard Madoff was up to". Wall Street Journal Online.
[3] The list of firms and the classification of whistleblowers is available from the websites of the authors.

Consistently, KKLM find that the SSCAC had the lowest omission rate relative to their hand collected sample across the databases they considered.

The biggest potential problem with using class action data is that it might over include frivolous cases. DMZ use a filtering process, summarized in the appendix of the paper, to remove this concern. The gist of the screening is to restrict attention: first, to cases after a 1995 change in the law forced courts to became more stringent about evidence to certify a case; second, to cases that are not dismissed; and third, to cases without low settlements amounts. We follow the legal literature's guidance as to what settlement amounts constitute nominal payments to make the suit go away.

For all these reasons, we are going to use the DMZ sample as the baseline, always comparing the results with the Restatements sample and the AAER sample. An added advantage of the DMZ sample is that the frauds include financial and non-financial frauds, both of which can affect the reputation of a firm and destroy value.

Figure 1 presents the fraction of firms that are engaged in fraud that are eventually caught each single year. Not surprisingly, when fraud is defined as failure to apply accounting rules (Accounting Restatements), the fraction of firms involved at any point in time is rather large (on average 13%). All the other definitions produce a much lower incidence: samples focused solely on detected financial fraud average between 2% and 3% of firms. The frequency of detected financial fraud and the frequency of all detected fraud (both financial and non-financial) lies between the restatement figure and the AAER one. By using the DMZ fraud definition the average percentage of firms engaging in a fraud that was later detected averages 4.5%.

Note that there appears to be a cyclical component to fraud and the late 90s could be an abnormal period. Wang, Winton and Yu (2010), for example, find that fraud propensity increases with the level of investor beliefs about industry prospects. Thus, during the internet boom fraud can be abnormally high.

Indeed, as Figure 1 shows, fraud seems to be procyclical according to all the definitions of fraud, but particularly according to the restatement definition. The cyclical fluctuations are not as pronounced using the DMZ or AAER samples as for the restatement sample.

II. Inferring the Detection Likelihood in the Arthur Andersen Natural Experiment

The above figure represents only the incidence of those frauds that were eventually revealed. It ignores the frauds that were never revealed.

II.1. Experiment Overview

To estimate the unobserved fraud we will rely on the Kolmogorov axiom of conditional probability. What we observe is the joint event of a firm engaging in fraud and being caught: **Pr(*F*,*caught*)**. (We will use the convention of bolding the variables we observe.) Our actual variable of interest is the probability of a firm engaging in fraud, regardless of whether it is caught or not: Pr(*F*). By the law of conditional probability, the unconditional probability of engaging in a fraud can be written as:

$$\Pr(F) = \frac{\mathbf{Pr(F,}\boldsymbol{caught}\mathbf{)}}{\Pr(caught \mid F)}. \tag{1}$$

Thus, if we knew the denominator, the detection likelihood (Pr(*caught*|*F*)), we could calculate Pr(*F*).

Our main experiment uses the sudden demise of Arthur Andersen (AA) to isolate a sample of firms for which the detection likelihood for financial frauds substantially increases. In the fall of 2001, Enron's collapse raised questions about AA. In March 2002 AA was indicted and then convicted (June, 2002) for obstruction of justice. While some AA clients started to switch auditors in 2001 (Chen and Zhou (2007)), by the end of 2002 all of AA's clients had to change their external auditor. Since roughly one fifth of all large publicly traded firms had AA as their auditor in 2001, we regard the eventual switch as a forced rotation of the auditing firm (not necessarily of the auditing partner, since AA partners sometimes followed their clients and joined the new auditing firm).

II.2. Identification Assumption 1

Our first assumption states that prior to 2002 financial fraud was equally likely in AA firms and non-AA firms ($\overline{AA}$):[4]

Assumption 1: $$\Pr(FF \mid \overline{AA}) = \Pr(FF \mid AA)$$

AA's indictment by the Department of Justice and its initial conviction for obstruction of justice may make this a surprising assumption. Yet a range of studies have concluded that there was no difference between AA and other auditors in terms of auditing rigor. In a matched sample, Agrawal & Chada (2005) find that the existence of AA as the auditor is not associated with firms having more restatements. Likewise, controlling for client size, region, time and industry, Eisenberg & Macey (2004) find that AA clients did not perform any better or worse than other firms. Furthermore, the initial conviction of AA was for obstruction of justice in the particular case of Enron, not for being a bad auditor, and it was unanimously overturned upon appeal.

Nevertheless, we want to re-test this assumption. Our sample of large U.S. corporations is different from the aforementioned studies. Table 1 reports summary statistics, comparing AA clients and non-AA clients in firm characteristics and industry during the period 1998-2000. We use two sets of non-AA clients: all non-AA clients that meet our size criteria and all non-AA clients that meet our size criteria and also are clients of one of the other Big Five audit firms (perhaps a more appropriate reference group).

On average, AA client-firms are smaller (using means) and more leveraged. As Panel B illustrates, there are also differences in the industry composition. AA client firms are less likely to be in Banks & Insurance, Computers, and Retail & Wholesale. By contrast, they are more likely to be in Refining & Extractive, Services & Health, Communication & Transport, and Utilities.

Were AA clients more fraudulent? To answer this question, we consider various indicators of the presence of fraud: (i) the probability of accounting manipulation according to Beneish's (1999) definition

[4] To keep notation simple, we do not include time subscripts, but for all the equations, the AA marker means that the firm was an Arthur Andersen client sometime in the 2000-2002 period.

[*ProbM*][5] (unobserved fraud), (ii) the accounting-based fraud score of Dechow, Ge, Larson and Sloan (2011) [*FScore*] [6] (unobserved fraud), (iii) caught fraud from Dyck, Morse and Zingales (2010) [*Fraud*] (observed fraud), and (iv) restatements identified by SEC investigation of the registrant in AuditAnalytics [*Restatement*] (observed fraud)[7]. Looking first at univariate statistics, in Panel A of Table 2 we find no significant differences between AA and non-AA clients (Big 5 non-AA clients) in any of the five fraud measures before 2001. This result, however, does not account for the above-mentioned size, leverage and industry differences.

To test further whether AA clients were more fraudulent, we need to use a multivariate framework that can control for observed differences as in Table 2, Panel B. We estimate a linear probability model, where the dependent variable is ProbM (or alternatively FScore) and the explanatory variables are an AA dummy and a set of control variables for size, debt and profitability, including year fixed effects (columns 1, 4, 7, 10). Because we are concerned about the different industry composition, in some specifications we absorb 2-digit SIC industries-crossed-with-year fixed effects (columns 2, 5, 8, 11). We also restrict the non-AA firms to be Big 5 accounting firm clients (columns 4-6, 10-12). Finally, we report quantile estimates at the median for the ProbM score and FScore continuous dependent variables.

Across all specifications, the coefficient on the AA dummy is insignificant. Being an AA client does not significantly impact the likelihood of manipulation or fraud in the pre-demise period, either at the mean or the median. Furthermore, as we saw in the univariate statistics, controlling for industry fixed

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[5] Appendix II details the calculations for the ProbM score. ProbM is a score with no natural scale. The mean and standard deviation of ProbM in our sample are -2.325 and 1.357, respectively. According to Beneish (1999), a score greater than -2.22 indicates a strong likelihood of a firm being a manipulator.
[6] We thank Weili Ge for the Fscore data. We use their FScore measure from model 2 of the paper, which includes financial statement variables and market data. FScore is a score variable, scaled to imply that a score of 1.00 indicates that the firm has no more probability of AAER fraud than the unconditional probability. In our sample, the mean and standard deviation of ProbM are 1.785 and 10.57, respectively.
[7] AuditAnalytics provides a number of restatement measures. Since our analysis concerns fraud, we avoid the more prevalent measures of accounting accidents or aggressive accounting. We instead chose their variable res_SEC_investigation. The other possibility is res_fraud; however, there were very few instances of coding to this restatement classification during our sample period.

effects does not change much the coefficient of the AA-dummy, suggesting that industry composition does not affect the results.

In Panel C we repeat these tests using as dependent variables DMZ fraud, AAERs and restatements without and with SEC investigations. As before, in all cases, the coefficient on the AA dummy is insignificant.[8]

A paper that reports a difference between AA and non-AA firms is Krishnan and Visvanathan (2008). They find that earnings management in the AA Houston office was more pronounced than in Houston offices of other Big Five auditors. To address this concern in Appendix Table A1 we restrict our sample to large corporations located in the state of Texas. For each AA client, we find a match among other Big Five clients within the two-digit SIC code, based on a propensity score to be an AA client. We generate the propensity score based on assets, sales, EBITDA, and leverage within industry. Table A1 (columns 1,2) shows that with the ProbM score as dependent variable, the Arthur Andersen indicator is not significant either in the Texas sample (Panel A) or in the United States sample (Panel B). We repeat the same tests using as alternative dependent variables different definitions of caught fraud (DMZ fraud, AAER, and Restatements). The results are unchanged. Thus, we do not find any evidence that AA clients are statistically or economically different from other auditors' clients as per Assumption 1.

II.3. Identification Assumption 2

In 2001-2002 the revelation of accounting fraud in Enron and WorldCom put accounting firms under the microscope for perceived litigation risk. As Tirole (1986) points out, a forced turnover reduces incentives to collude. Hence in general a new auditor has stronger incentives to check new accounts they assume and to expose any additional fraud. Our claim is simply that the new auditors of former AA clients had such stronger incentives. This incentive was stronger if the AA audit partner did not follow the client in the new audit firm, but it was present even when the audit partner remained the same. In this

[8] Because in these tests the dependent variable is dichotomous, in these tests we do not estimate median regressions.

latter case the review partners of the new audit firm are those who will do the extra monitoring, fearful to assume some liability for omissions due to AA.

We provide estimates of higher levels of fraud revelation in former AA clients in Table 3, described more fully below. After 2002, the percentage of firms in which fraud was exposed was significantly larger in former AA-clients, regardless of the definition of fraud we use.

These results are not in contradiction with Blouin et al. (2007). They find that the audit quality of former AA clients does not improve after the forced turnover of the auditor. This is consistent with AA not being an inferior auditor. But it does not speak to the incentives of the new auditor to air out potential problems. That forced auditor rotation might improve auditors' independence and thus audit quality is at the center of a very controversial debate in accounting (e.g., Harris and Whisenant, 2012). Positions in this debate differ on the size of the net benefit of forced rotation, not on the existence of some benefits. Thus, we are not claiming that the quality of audits will necessarily be better after the rotation, but only that the incentives to reveal the omissions and mistakes of previous auditors go up, consistent with the evidence and much of the literature.

For reasons that will be clear momentarily, we make here a stronger assumption: not only did the probability of discovering the fraud go up after the turnover, but it went to 1 (i.e., all the frauds are discovered). This extreme assumption will have the effect of overestimating the detection ratio and thus of underestimating the hidden amount of fraud.

Assumption 2: $$\Pr(caught \mid FF, AA) = 1$$

Note that this assumption refers only to the frauds started before or during 2002, i.e. detectable at the time of the auditor's turnover.

II.4. Conditional Probabilities within the AA Experiment

With Assumptions 1 and 2 in hand, we can use the law of conditional probability to derive an estimator for the detection likelihood of fraud. If we write down two versions of equation (1) (one for AA and one for non-AA), bringing the denominator to the other side, and dividing one by the other , we have a relationship that puts only observable detections of fraud on the right-hand side:

$$\frac{\Pr\left(caught|FF,\overline{AA}\right)\cdot\Pr\left(FF|\overline{AA}\right)}{\Pr\left(caught|FF,AA\right)\cdot\Pr\left(FF|AA\right)}=\frac{\boldsymbol{Pr\left(FF,caught\mid\overline{AA}\right)}}{\boldsymbol{Pr\left(FF,caught\mid AA\right)}}. \qquad (2)$$

Substituting Assumptions 1 and 2 into the left hand side of equation (2) implies:

$$\Pr\left(caught|FF\right)=\Pr\left(caught|FF,\overline{AA}\right)=\frac{\boldsymbol{Pr\left(FF,caught\mid\overline{AA}\right)}}{\boldsymbol{Pr\left(FF,caught\mid AA\right)}}. \qquad (3)$$

Equation (3) provides an estimator for the likelihood of detection of financial fraud that is based solely on two observables: the detection rate of financial fraud by auditors in non-AA firms and the detection rate of financial frauds by auditors in former AA firms.

Note that we derive the detection likelihood by comparing fraud detection in two groups (former AA clients and non AA clients) at the same time. Thus, this estimate should not be affected by fluctuations in the probability of fraud, as long as these fluctuations are similar between the two groups.

It could be, however, that trends in industry distribution of fraud vary over time. If this is the case and if frauds detectability varies across industries, then our detection likelihood estimates would not necessarily generalize to other periods. To address this concern, momentarily we will consider the sectoral and regional distributions of fraud *within our sample*.

II.5. Detection Likelihood Results

Table 3 reports the results for the equation (3) estimation of the detection likelihood. The main experiment sample and four robustness samples are listed below:

Main: [*Auditor-Detected Fraud*] Auditor-detections of financial fraud in securities class actions. (DMZ data based on Stanford Securities Class Action)

Robustness:

R1. [*Restatements: Accounting*] Accounting restatements where the restatement emerged from accounting rule application failures. (AuditAnalytics data)

R2. [*Restatements: SEC Investigations*] Accounting restatements where the restatement emerged from accounting rule application failure accompanied by an SEC investigation. (AuditAnalytics data)

R3. [*AAERs*] Investigations of the SEC. (Data from Center for Financial Reporting and Management, Berkeley Haas)

R4. [*Financial Misrepresentation Class Actions* and *All Class Actions*] Two broader fraud samples using the DMZ data. The financial misrepresentation class action sample includes all alleged frauds that involve misrepresentation on financial statements. The all class action sample includes all frauds, irrespective if the allegation involved financial misrepresentations(DMZ data)

The main estimate of the detection likelihood compares AA and non-AA client fraud in DMZ class action fraud data, focusing explicitly on frauds exposed by auditors. Auditor-detected frauds include two sets of cases – cases where the auditor resigned or refused to sign outright and cases where the auditor issued a qualified opinion.[9]

For robustness, we also consider four alternative samples of fraud. The first robustness sample is composed of Accounting Restatements from AuditAnalytics. The second robustness sample is composed of Accounting Restatements accompanied by SEC investigations from AuditAnalytics. The third robustness sample is composed of AAER investigations by the SEC. The fourth robustness sample uses broader samples of frauds from the DMZ fraud data based on class actions.

Cross-sectional correlations (across large corporations active during the experiment) among these samples are provided in Appendix Table A2. These correlations range from 0.164 to 0.730; thus the samples are indeed capturing different aspects of fraud.

Table 3 provides the detection likelihood results that are the main results of the paper. Panel A presents the experiment estimates. The first two columns report that during 2001-2002 auditors uncovered pre-existing fraud in 2.9% of former AA clients. This compares with auditor detection of fraud in 0.9% of non-AA clients in the same period. These numbers are statistically different from each other at the 1% level. The ratio of these two numbers, reported in column 1, gives us the probability of detection of financial fraud. Only 31.1% of financial frauds are detected.

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[9] In the latter cases, we consider the qualified opinion to be a trigger event. We re-read the accusation details from DMZ (2010) for all of these cases, available on the authors' websites. We eliminated two extreme cases where the event could not plausibly be related to auditing, but more generally we did not want to subjectively assert whether the auditor could or could not have played a role. Rather, we rely on alternative samples for robustness to the auditor-detection role.

In column 2 we use a somewhat longer time period (2001-2003) for the fraud to be uncovered. We obtain a very similar estimate: 29.1% of the frauds are detected.[10]

Panel B presents the robustness estimates of detection of financial fraud, starting with restatements. Because many restatements are not fraud, this number is a mixture of accounting infractions and fraud. Whether we use all restatements or only restatements that are followed by an SEC investigation, we find that detected frauds are significantly larger for non AA-former clients than for AA clients (p-value of <0.001). The detection rate estimate based on restatements varies from 33.0% for all restatements to 29.5% for restatements accompanied by SEC investigations.

For the AAER sample, the difference between the incidences in the two groups is different only at the 6% confidence level. The implied detection rate is 55.4%. For the financial misrepresentations in the DMZ class action sample, the difference in incidence between the two samples is statistically different from zero at the 2% level and the implied detection rate is 46.5%. In panel C we report the results obtained by using the entire DMZ class-action sample. The difference in incidence between the two samples is statistically different from zero at the 1% level and the implied detection rate is 51.5%. As a further check on our estimates we also include in the Table (column 3) a placebo test, comparing the estimates of detected fraud in the 2004-2006 period, long past the demise of AA, between former AA clients and non-AA clients. As expected, in this period there is no significant difference.

We view all these estimates as providing a bound within which we interpret our main estimation of the paper. Our main point estimate is that 3.1 out of every 10 frauds that start are caught, with a range, depending on alternative fraud samples, from 2.9 to 5.5 out of every 10 frauds that start are caught.

Of all these estimates the most likely to violate Assumption 2 are the ones based on SEC enforcement. The SEC has limited resources, which reduce the likelihood of enforcements even when a

[10] This result is not particularly sensitive to the timing of when AA clients switched. Chen and Zhou (2007) document differences in the timing of firms' decision to replace AA, and determinants of that timing decision. In our estimation, when we consider only AA clients as of the year 2000, this only shifts our estimate from 0.311 to 0.321.

fraud is brought to their attention. For this reason, in what follows we will not use the estimates based on SEC enforcements (both the AAER sample and the restatements followed by SEC investigations).

II.6. Detection Likelihood Generalizability: Robustness to Industry, Geography and Time

As noted above, our estimated detection likelihood does not rely on any assumption on how prevalent fraud is during this time period (e.g., because of business cycle fluctuations affecting the level of fraud in the economy level (Wang, Winton and Yu (2010)), but rather just on the exogeneity of the AA experiment. However, one concern could be that if sector or regional variation were driving our estimates, then detection likelihood would not be general and applicable to other periods. In this section, we thus assess the robustness of our detection likelihood estimate to industry, geography and type of fraud detectors. To the extent we find little variation in detection likelihood across these dimensions, this suggests limited variation in detection likelihood estimate over time, as the relative importance of these dimensions fluctuate over the business cycle. Our particular time period of study is well-suited for such an exploration as the unusual events in the period (e.g., Enron, Sarbanes-Oxley, etc.) have asymmetric impact in the cross-section of firms and detectors.

We showed in section *II.2*. that AA firms have more industry representation in Communications & Transport, Refining & Extractive, Services & Healthcare, and Utilities with the offset of less representation in Banks & Insurance, Computers, and Retail & Wholesale. If, during our time period, fraud were more prevalent among sectors in which AA clients were over-represented, then our detection likelihood estimate could derive from this industry composition and not represent a stable estimate.

Table 4 presents detection likelihoods estimates in sub-samples removing iteratively AA over- and under-represented industries.[11] We do the removal procedure and only focus on the Auditor-Detected Fraud main experiment (Table 3, column 2) and the Restatements: Accounting samples because we lose

[11] Appendix Table A2, Panels B and C report the industry and regional distribution of fraud for all the fraud measures. We report both the incidence of fraud within an industry (e.g., of all communication corporations, how many commit fraud?) and the incidence of each industry in the overall count of frauds (e.g., what percent of frauds are in the communication industry?).

observations in this procedure. We find that our results are statistically robust to excluding all industries where AA was over-represented or under-represented. Economically, the magnitude of the detection likelihood estimate marginally declines when we exclude Services & Healthcare. In contrast, the estimate increases in excluding Banks & Insurance and Communication & Transport. For the estimate excluding Banks, the larger detection likelihood is driven by non-AA firms having a very low detection for fraud in Bank and Insurance firms (perhaps foreshadowing ill events that were to come in the Great Recession). Irrespectively, the industry results suggest that our detection likelihood magnitude is not positively driven by industries which might be over-represented in fraud (e.g., the energy, communications and tech industries) during the period.

Likewise, if AA had a particular regional tilt in its client distribution and during our experiment period particular regions may have been more (or less) prone to fraud or fraud detection, the origin and stability of our estimate would be in question. In particular, one might worry about the Houston area following the Enron scandal. To test the robustness of our results we subsample by region. We again use the process of excluding regions one by one to preserve sample size. In Panel B of Table 4, we show that the detection likelihood results are highly robust to sorting corporations by headquarter location and to excluding regions (like the Southwest) and large states, like Texas.

Having shown that our detection likelihood results are robust to industry and geography variation in fraud, we turn to the second possibility of unusual detection incentives by certain fraud detectors during the period of our study. Consider the auditors. One possibility is that an AA client switched audit firms but not audit partners when a new audit firm took over the AA account. In this case, however, our estimates would be quite conservative, since transferred partners would not have had incentives to reveal dirty laundry that they themselves presided over. A quantification of the conservatism in our approach is provided by Kohlbeck, Mayhew, Murphy and Wilkins (2009), who estimate that 25% of former AA clients are audited by a firm that hired their former auditor. Another possibility is that non-AA firms also audited differently in their own clients in the post-AA period. If non-AA auditors increased scrutiny across the board, not just in AA clients, then again our estimates are conservative.

A more general concern is that the increased attention on fraud that followed the Enron and WorldCom scandals might have prompted other fraud detectors, such as the SEC or plaintiff attorneys, to become more active in detecting fraud. As we report in Table 3, we find very similar detection likelihood estimates across various measures of fraud. The fact that we find very similar detection time patterns in the AAERs sample (which includes only SEC enforcement actions) and DMZ (class actions where SEC enforcement actions were rare and plaintiff attorneys' incentives are more relevant) suggests that this source of variation is second order.

Beyond this evidence, we note an aspect of our experiment design that insulates us from this concern. If these changes affect all firms, they do not impact our estimates, since our identification comes from comparing AA and non-AA firms at the same point in time. Furthermore, the beauty of our AA-demise experiment is that it was completely unexpected and so it could not have altered the ex-ante incentives to commit fraud.

III. Fraud Pervasiveness: The Unconditional Probability of Engaging in Fraud

We now take the next step of applying equation (1), $\Pr(F) = \frac{\mathbf{Pr(F,} \boldsymbol{caught}\mathbf{)}}{\Pr(caught \mid F)}$), to estimate the overall pervasiveness of fraud.

III.1 From Financial Fraud Detection Likelihood to Fraud Detection Likelihood

We first focus on the denominator of equation (1). Using notation of NFF for non-financial fraud, the detection likelihood of any type of fraud for firms is:

$$\Pr(caught|F) = (\mathbf{1} - \%\boldsymbol{FF}) * \Pr(caught|NFF) + \%\boldsymbol{FF} * \Pr(caught|FF) \quad (4)$$

This equation shows that the overall fraud detection likelihood depends on three pieces of information: the detection likelihood for financial frauds ($\Pr(caught|FF)$) we estimated in the last section, the fraction of financial frauds among all frauds ($\%\boldsymbol{FF}$), and the detection likelihood for non-financial frauds ($\Pr(caught|NFF)$).

The DMZ database provides an estimate of the fraction of financial frauds amongst all frauds, classifying frauds as financial if one of these conditions is satisfied: they involved restatements, had accounting-related charges, accounting-related investigations, or public filings indicated an intent to restate. DMZ report that in the 1996-2004 period, the fraction of financial frauds was 64.7%.

The third piece of information required, ($\Pr(caught|NFF)$), is an estimate of the detection likelihood of non-financial fraud. Unfortunately, we do not have an experiment like AA sudden demise to estimate the undetected non-financial fraud. Therefore, we extrapolate this number by making an assumption as to how non-financial fraud detection compares to that for financial frauds. For our core estimate, we assume that non-financial frauds are no more hidden than financial frauds:

Assumption 3: $$\Pr(caught \mid NFF) = \Pr(caught \mid FF)$$

We believe this is a conservative assumption as there is relatively greater public information to infer financial misrepresentations – insiders need to produce financial statements, and thus fraud detectors can compare these disclosures with their history and with disclosures of competitors. Thus, it seems quite reasonable to assume that given a fraud is committed, it is at least as likely that a financial fraud is detected as a non-financial fraud is.

Assumption 3 leads to the overall pervasiveness estimator:

$$Pr(F) == \frac{\boldsymbol{Pr(F, caught)}}{detection\ likelihood\ estimate\ from\ AA\ experiment}. \tag{5}$$

For robustness, we also consider a more conservative assumption that all non-financial frauds that start are caught.

Assumption 3A: $$\Pr(caught \mid NFF) = 1$$

Assumption 3A leads to a very conservative lower bound estimate of the pervasiveness of corporate fraud:

$$\Pr(F) == \frac{\mathbf{Pr(F, caught)}}{(\mathbf{1 - \%FF}) \cdot 1 + (\mathbf{\%FF}) \cdot (detection\ likelihood\ estimate\ from\ AA\ experiment)}. \tag{6}$$

Table 5 shows how detection likelihood estimates change based on assumption 3 or 3A. With assumption 3, reported in column 1, our core estimate for fraud detection likelihood is 31.1% based on the DMZ auditor sample as in the prior section. If we restrict attention to financial frauds only, we again arrive at 31.1% and we have a result of 33.0% if we look at financial restatements. We see more variability in panel C. Here as we report in column 1 if we make the most conservative assumption 3a, and assume all non-financial frauds are detected, the detection likelihood rises to 55.4% for the DMZ auditor sample. If we use an alternative estimate based on regressions using the DMZ data described below, we arrive at a detection likelihood of 21.0%. In summary, our core estimate of detection likelihood results in 3 out of 10 frauds being caught, with a range based on alternative assumptions of 2 to 5.5 out of every 10 frauds that start are caught.

III.2. – Detected Fraud Rates

The numerator in equations (5) and (6) is the observable incidence of fraud that starts and is caught, **Pr(*F, caught*)**. As Figure 1 shows, detected fraud varies over time across all fraud data samples. One can apply the detection likelihood estimate to the detected fraud rate at different points along the cycle, or use an average over a long period. For the main estimate we take seriously prior literature that suggests fraud may be cyclical (Wang, Winton and Yu (2010)). Rather than relying on a specific point in time we use a period that captures both periods of strong economic activity and declining economic activity.

By using the NBER Business Cycle dates, we notice that the 1996-2004 period covers two periods of expansion (March 1991- March 2001, November 2001 – December 2007) and one period of recession (March 2001-November 2001). The start in January of 1996 and the end point in December of 2004 are both almost exactly halfway through the respective expansion periods, so the period covers two full business cycles from mid-point to mid-point. Concerns could be raised that the boom in 2000 (followed by recession in 2001) was particularly extreme and may have led to more frauds being started than in normal times. To avoid our results being driven by this episode, we exclude the 2000 and 2001 years from our base estimation period.

In column 2 we report the average percentage of firms in a given year that are engaged in fraud that ultimately is detected for the 1996-2004 period when we exclude the 2000 and 2001 years. For our core estimate using the full DMZ sample, 4.04% of firms per year are engaged in detected fraud. In column 4 we report the average for the full 1996-2004 period where we find that on average 4.46% of firms are engaged in fraud at any point in time.

Alternatively one could focus on the financial frauds that are the focus of section II. For the 1996-2004 period, an average of 2.50% of sample firms are engaged in financial fraud at any point in time. This estimate lies between (untabulated) estimates based on samples that involve financial misrepresentations with SEC investigations (the restatements with SEC investigations sample and the AAER sample each suggest an average of 2% of firms are engaged in financial fraud at any point in time) and the broader sample of all restatements (that produces an estimate of 12.6% of firms engaged in fraud). Excluding the 2000 and 2001 period results in a core estimate of detected financial fraud of 2.26%.

III.3. –Fraud Pervasiveness Estimates

We now are in position to report estimates of the pervasiveness of corporate fraud. We follow equation (5) and calculate this by dividing the detected fraud rate by the detection likelihood. In the Table one simply divides column (2) by column (1) for our core estimates, and divides column (4) by column 1 if one uses the full 1996-2004 period to estimate detected fraud rates. We find that on average 13.0% (i.e. 4.0%/0.311) of firms engage in fraud at a point in time, implying 9.0% undetected fraud along with 4% detected fraud.

If one wants to focus on sub-samples (such as financial fraud), or use other data sources, assumptions or time periods for detected fraud, fraud pervasiveness estimates vary, as we illustrate in panels B and C. Panel B shows that fraud pervasiveness is understandably smaller if we restrict attention solely to financial frauds, finding pervasiveness of 7.3%. If we focus on restatements, fraud pervasiveness is substantially larger at 36.6%. In panel C we report that using the most conservative assumption 3a) lowers our estimate of pervasiveness of fraud to 7.3%, while using the detection likelihood reported in the next section raises pervasiveness estimates to 19.3%.

In summary, our estimate of pervasiveness is 13% with estimates that range from 7.3% to 36.6% depending on sample and assumptions.

IV. Incentives and Opportunities Estimation Validation of Fraud Pervasiveness

To explore the validity of our fraud pervasiveness estimates based on the AA experiment, in this section we produce pervasiveness estimates using an alternative approach and then compare our estimates with others existing in the literature.

IV.1. Incentives and Opportunities Estimation: Approach

We employ an empirical design that estimates the fraud detection likelihood by exploiting situations where incentives or opportunities for detections are high (H) across the village of fraud detectors that DMZ show are involved in fraud detection. To illustrate the idea, consider one of the fraud detectors in DMZ: analysts. Yu (2008) studies the role of analysts and earnings management. Using an instrumented design, he is able to conclude that analysts effectively monitor firms, and thus more monitors imply a higher detection of existing frauds.[12]

Our idea is to use this logic across five groups of known detectors identified by DMZ -- analysts, media, shortsellers, industry regulators, and employees with monetary incentives to whistleblowing -- to estimate a level of detection when incentives and opportunities for detection are high. We lay out an estimation model where we can simulated a setting in which firms face high incentives or opportunities for fraud detection for all of the village of detectors. Then we simply ask: what would be the detection probability if all the villagers were looking for fraud full force.

[12] We discuss the ex ante disincentives of committing fraud (i.e., Becker incentives) that more monitoring may create momentarily.

To estimate this setting of heightened detection, we can also employ a time series element; namely, *Sarbanes Oxley*. DMZ find higher incidence of fraud detection after *Sarbanes-Oxley*. Higher incentives for detection may result from the legislation itself or more simply from the considerable public scrutiny on fraud after the major scandals at the beginning of the new millennium.

More formally, we denote the potential for a firm *i*'s fraud to be detected in year *t* as D_{it}, where the firm is caught if D_{it} is positive:

$$\begin{aligned} D_{it} &= X_{it}^{D}\Gamma_{D} + \nu_{it} \\ caught_{it} &= 1 \; if \, D_{it} > 0. \end{aligned} \tag{7}$$

D_{it} is a function of observables X_{it}^{D}. Our strategy is to include a set of indicators $\left[I_{it}^{HiIncentOpp} \right]$ in X_{it}^{D} that equal one when the circumstance leading to detection by each of fraud detectors is high. Following Wang (2013), we address the possibility that there are other observables that would also influence detection unrelated to detector incentives by including in X_{it}^{D} general firm and market characteristics $\left[\mathrm{x}_{it}^{D} \right]$ that would lead to detection in all firms (e.g., size and stock performance). We denote these two groups: $X_{it}^{D}{}' = \left[I_{it}^{HiIncentOpp} \quad \mathrm{x}_{it}^{D} \right]$.

By comparing the ratio of detection in the 'high' setting indicated by H with that in the normal situation indicated by $\overline{H}$ (i.e. when all of the indicators are at their mean level), we produce an alternative estimate of detection likelihood. As in the prior section, it is instructive to make clear the assumptions we rely on to produce our estimates.

We start by assuming that the detection likelihood approaches 1 when all detection incentives and opportunities are high:

Assumption 4: $\Pr(caught|F, H) = 1$. (8)

Assumption 4 is clearly conservative. More analysts, for example, provide greater opportunities for detection but this does not fundamentally change the limited incentives for analysts to bring frauds to light.

We also assume that the probability of engaging in a fraud is the same in both environments (the high detection and the low detection):

Assumption 5: $\Pr(F|H) = \Pr(F|\overline{H})$ (9)

This assumption is unlikely to be satisfied. Potential fraudsters are aware of the presence of high incentives and opportunities for detection. Thus, they are less likely to commit fraud. Assumption 5, thus, will overestimate the detection rate, underestimating the unconditional probability of committing fraud.

With these two assumptions, we can produce an estimate of the likelihood of detection by comparing the observed detection rates when not all indicators are high and when all detection incentives and opportunities are high:

$$\widehat{Pr}\left(caught|F\right) = \frac{\boldsymbol{Pr(caught, F \mid \overline{H})}}{\boldsymbol{Pr(caught, F \mid H)}} \qquad (10)$$

IV.2 Incentives & Opportunities Estimation: Probit Results

In Appendix Table A3, we list the variables we use in the probit estimations of what influences the likelihood of detection. Panel A defines the six indicator variables, showing how we split the sample into high and low analyst groups, high and low media groups, high and low shortability groups, industries with an industry regulator and not, industries with high monetary incentives for employees (i.e. industries where the 1986 False Claim Act apply or Qui Tam Industries) and not, and post SOX and pre SOX.

Panel B lists the firm and market covariates that might also influence the likelihood of detection. First are variables concerning the firm size and status in the industry. When firms operate in markets that are more competitive and rents are smaller, there is less scope to engage in self-dealing (e.g., Shleifer and Vishny (1997)). Thus, we include the log of firm assets, as well as the firm's market share and the Herfindahl-Hirschman Index (HHI) for its SIC2 industry.[13] Second, we follow Wang (2013) and include

[13] While there is a potential selection concern by not including private firms in constructing industry concentration, other papers such as Kadryzhanov and Rhodes-Kropf (2009) have found little difference in replacing these measures with census-based data measures of competition that include private in addition to publicly-traded firms.

R&D as a measure of the opacity of firm fundamentals, under the assumption that when financials are more opaque it is easier to engage in fraud and it is harder to detect it. Third, we include leverage under the assumption that high debt level raise risks for equity holders, sharpening the incentives of equity-based monitoring. Fourth, we include performance monitoring triggers. Disappointing accounting or stock market performance likely leads to lead to more activity by fraud detectors (e.g. by plaintiff attorneys) and more detection and accordingly we include both accounting performance (ROA) and stock performance (stock return), as well as abnormal movements in these variables relative to their industry peers. Fifth, we include measures of overall market uncertainty, under the assumption that more volatile market environments spur market detectors into action. In particular, we introduce the VIX and the 1-year innovation to VIX in percentage change.

The final covariate that we include is Beneish's probability of manipulation (Prob M) measure.This variable provides a parsimonious way to capture a set of observable omitted variables that might also affect the probability of fraud detection. Constructed from cases of infractions, it may in fact over-control for the information the market sees that might trigger more detection, but we prefer to err on the side of reducing our power than having omitted variables. We present results with and without this measure.

Table 6 presents our probit estimates of the determinants of detection and the implied detection likelihood estimates. The reported coefficients show the marginal effects. Column (1) and (2) just include the covariates, but not the high incentive indicator variables. The coefficients on these covariates are largely as expected, although lacking in power in many cases. Fraud detection is higher in large firms with higher leverage. The positive and significant coefficient on the VIX shows that detection is more likely when the stock price is more volatile. In this simple specification, inconsistent with Wang (2013), firms with high R&D have more detection.

Our focus is on the results in columns (3) and (4) when we include addition the high incentive indicator variables. Across both specifications we find that 4 of the 6 indicators are positive and significant. Specifically, detection is significantly higher with high analysts, high shortability, in Qui Tam industries and post SOX.

In terms of magnitude, the reported results are marginal effects. Thus, by using equation (10), we can estimate the detection likelihood by comparing implied detection rates in the high state $\Pr(F, caught \mid H)$ to detection in the mean state $\Pr(F, caught \mid \overline{H})$. As we report at the bottom of the table for model (4), $\Pr(F, caught \mid H)$ is 9.78%, while $\Pr(F, caught \mid \overline{H})$ is 2.05%. These estimates produce a likelihood of detection equal to 0.210 (i.e. 2.05/9.78).

Of course, it is possible that Assumption 5 is not satisfied with high incentives for detection also reducing the likelihood of engaging in fraud. Wang (2013) offers a solution of applying the Poirier (1980) bivariate probit model for estimating dichotomous outcomes in partial observability settings to the case of corporate fraud. We follow this approach, presenting results in Appendix III. Results are similar with a slightly higher estimate of the detection likelihood as expected. Nonetheless, we focus our attention on the probit estimate. Not only is it more conservative, but it also does not require the additional assumptions in the bivariate probit about variables that affect the likelihood of starting a fraud, but do not affect detection, about which there is little consensus.

IV.4 Related Literature

A limited literature tries to measure both undetected and detected fraud, focusing almost exclusively on financial frauds and using a variety of parametric methods and/or surveys. While our setting has the advantages of looking at all types of frauds, and the natural experiment provides advantages in empirical design, it is nonetheless instructive to compare our findings with these results from other settings and other approaches. Reassuringly, all these other approaches produce estimated of pervasiveness of fraud not far from the 13% based on our main experiment, and lie within the range we report in Table 5.

Beneish (1999) focuses on financial frauds and identifies threshold levels of probM score that balance type I and type II errors in predicting fraud. Using data from 1982-1992 he flagged 13% of firms as potentially fraudulent, and in an updated study using the same cutoff Beneish, Lee and Nichols (2012) flag on average 18.8% of firms as potentially earnings manipulators in the 1996-2004 period.

Wang, Winton and Yu (2010) examine financial frauds among the 3297 IPOs from 1995-2005. Their main goal is to show that fraud is procyclical, yet, their bivariate probit model produces predicted probabilities of engaging in fraud of 10-15%, very much in line with our estimates.[14]

Prior to Lie (2005), no options backdating had been detected. Bebchuk, Grinstein and Peyer (2010) look back over this period and identify the percentage of publicly-traded firms from 1996-2005 in which CEOs or directors were 'lucky' directors in that they received option grants on day of the month the stock price was the lowest price. By their estimate 12.4% of firms have such lucky CEOs.

Dichev, Graham, Harvey and Rajgopal (2013) survey 169 CFOs of public companies. A few of their questions focus on misrepresentations (they do not focus on fraud). Their survey suggests that 18.4% of firms manage earnings, meaning within-GAAP manipulation that misrepresents performance. Zakolyukina (2014) uses a structural model to explore detected and undetected GAAP manipulation and reports that 73% of CEOs manipulate their financials, with this result dominated by undetected manipulation (she reports a detection likelihood of only 0.06).

V. How Expensive Is Corporate Fraud?

To assess the economic relevance of corporate fraud it is not sufficient to estimate how frequently fraud is occurring, we also need to have an estimate of the deadweight loss produced when it occurs. In doing so there are three conceptual problems. First, we need to separate the company loss due to fraud from the legal penalties imposed on the fraudulent company, which are largely transfers. Second, we need to separate a company's loss in value due to fraud from the effect of the revelation of bad information about fundamentals, which frauds generally try to cover up. Third, we need to estimate the cost of undetected fraud.

[14] We infer this from Figure 1, predicted probability of fraud, and summary statistics on the distribution of industry EPS growth available in the internet appendix.

V.I Costs of Detected Frauds

The first two problems have been addressed by Karpoff, Lee and Martin (2008) (henceforth KLM). To estimate the cost of detected fraud KLM use a sample of firms subjected to SEC or DOJ enforcement actions. They calculate the sum of abnormal returns surrounding the announcements of fraud detection and punishment at 38% of the value of equity. To isolate the social cost, KLM subtract an estimate of the expected legal penalties and the capitalized value of assets written off in restatements (as a proxy of the costs arising from revelation of bad information).[15] By using this method, they estimate the deadweight loss of detected fraud at 25% of the equity value before the fraud. We are going to use this number as our base estimate.

The magnitude is not at all surprising. As noted in Karpoff and Lott (1993), corporations can gain in a variety of ways from being known as an honest actor that fulfills promises, delivers high product quality, etc. These quasi rents from reputation can be lost when a fraud is committed and the firm loses its reputation.

Yet, there are two possible concerns. First, this estimate has been derived from a sample different from the one for which our probability estimates have been obtained. Second, the announcement returns are appropriate for the enforcement actions, but not for a sample like DMZ where most of the revelations are due to media and employees, i.e., sources other than enforcement authorities where information is more likely to trickle out over time.

To address these concerns, we produce a within-sample fraud cost estimate using a slight modification of Berger and Ofek's (1995) multiples approach and considering the period from fraud initiation until its detection. This approach potentially produces a more noisy estimate than one based on announcement returns, with the long fraud duration of 1.67 years providing more opportunity for idiosyncratic shocks, and legal limits on fraud duration potentially lowering cost estimates.

[15] This measure of lost reputation cost is different from the social cost in several respects. It ignores the cost imposed on employees and customers and also ignores that competitors can capture part of the value lost.

As a starting point, we define $\widehat{V}_i$, the counterfactual enterprise value of firm *i* if there had been no fraud, as

$$\widehat{V}_i = \widehat{m}_i Y_i ,$$

where Y_i is firm's i level of performance (e.g. EBITDA) after the fraud has been revealed (and the financial accounts restated) and $\widehat{m}_i$ is firm's *i* counterfactual enterprise multiple after the fraud is revealed. To compute the counterfactual multiple we assume that -- had the fraud not occurred -- firm's *i* multiple would have changed in line with that of the industry over the fraud period, i.e.:

$$\hat{m}_{it} = m_{is} \frac{M_{jt}}{M_{js}},$$

where j is the industry firm *i* belongs to, an upper case *M* indicates an industry multiple, and *s* is the time before the fraud started and *t* the time after it had been fully revealed. The actual enterprise value is calculated after the fraud has been revealed and restatements have been introduced.[16]

The percentage loss in enterprise value caused by fraud is then computed as:

Deadweight Loss = Counterfactual Enterprise Value – (Actual Enterprise Value + Legal Cost.) [17]

This value is then standardized by the equity value the month before the announcement of the fraud. As legal costs we use the 3.7% of equity value, estimated by KLM.[18] We repeat this exercise for three different enterprise multiples: EBITDA, revenue, and fixed assets. For these results we focus on the results for the median firm.

Table 7 reports in Panel A the summary statistics for the key inputs in these calculations and in Panel B the resulting estimates of the deadweight loss from detected fraud. Focusing on the whole sample in the left-hand side of Panel B, we find detected fraud's deadweight loss ranges from 25% to 44% of

[16] This assumption might appear unpalatable because fraud is often committed to cover up underperformance. Note, however, that we account for underperformance by using the ex-post realized growth rate. So our assumption is simply that – absent fraud—the valuation would not have dropped more than what the decline in profitability justified.
[17] This will overstate the social cost if there is a substantial competitive effect with one firms' loss offset by a competitors' gain.
[18] This is likely an over correction. First, a large fraction of these fines are (and are expected to be) covered by insurance. The portion that is covered will not produce a drop in value as they will not burden the firm. Second, the KLM sample includes a lot of smaller firms, and the legal costs with larger firms is likely smaller.

equity value depending on which valuation multiple is used, with an average of 37%. If we rely on the EBITDA multiple (the most used) the median firm's deadweight loss of fraud is 25% of equity value, identical to the KLM estimates.

V.II Costs of Not-Detected Frauds

When it comes to non-detected frauds, there are two concerns. First, some of the deadweight loss of fraud had to do with the decline in reputation that follows the revelation of the fraud. Can we say that if the fraud goes undetected it has no cost? Second, since it is more difficult to hide bigger frauds, it is possible that the size of the frauds that go undetected is intrinsically smaller.

If we maintain the rational expectation assumption, fraud creates a deadweight loss even when it is not discovered, as long as investors are aware it exists. The only difference is that this deadweight loss is not borne by the fraudulent firm, but by all firms. Knowing that there are some fraudster firms, but not knowing who they are, investors will apply a lemon discount to all firms. The lemon discount borne by all firms in the marketplace is equal to the difference in value between the honest firms and the fraudulent ones multiplied by the probability a firm is fraudulent. Thus, if we multiply the loss that fraudulent firms would have incurred if discovered by the fraction of such firms in the sample, we obtain exactly the deadweight cost paid by all firms collectively because some of them are lemons.

To address the possibility that costs are systematically smaller for non-detected frauds we turn to the literature. Beneish, Lee and Nichols (2012) compare the annual buy-and-hold returns of firms that are flagged as probable earnings manipulators (by having a probM score above a cutoff) with firms that are not flagged as potential manipulators. They report a difference in annual future returns between the two portfolios of 11 percent. This result holds after controlling for a four factor model. We regard this lower annual return for potential manipulators as a reasonable, albeit conservative, estimate for the costs of undetected fraud.[19]

[19] Beneish, Nichols and Lee (2012) report that less than 0.5% of the firm years in their sample are associated with fraud as captured in an AAER. The negative return is likely conservative as the estimate is based on a one year buy

Consistent with theory, this cost estimate for firms with non-detected frauds is less than the cost associated with detected fraud, but is certainly not zero. This is consistent with a reputational cost being imposed if there are indications of a fraud in progress, even if there is no formal accusation and restatement involved.

As with detected frauds, we also construct an alternative estimate based on data within our sample. Recall that our main concern is the probability of detection is directly correlated with the magnitude of the fraud and hence with the magnitude of the value loss due to fraud. In this case, our estimate above would overestimate the magnitude of the value loss from all fraud. To address this concern we use the AA experiment. Since the forced turnover was unexpected, some fraud that "normally" would have never emerged did emerge. Thus, we can compare the costs of those normally undetected frauds with the costs of the full sample of detected frauds. This estimate is anti-conservative for undetected frauds in that their revelation may magnify their cost.

Table 7 panel B provides the result. As we report in the right-hand side of Panel B, the cost of the frauds brought to light by the AA experiment ranges from 21% to 39% of equity value depending on which valuation multiple is used, with an average of 27.8% of equity value. Again, as expected, these costs are lower than the fraud costs in the full sample of 36.7%. The within-sample estimate is higher than the undetected fraud costs provided by Beneish, Lee and Nichols (2012).

V.III – Overall Costs of Detected and Undetected Fraud

We are now in the position to report the deadweight cost of fraud, which is a residual loss after all the contractual and regulatory interventions to mitigate agency costs.[20] This cost depends on our main contribution of an estimate of detection likelihood, along with estimates of the detected fraud rate (with

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and hold return and undetected frauds are likely to have a longer duration (duration of detected frauds in our sample is 1.67 years).

[20] These calculations do not include potential benefits companies' managers can get from fraud. In not counting managerial private benefits, we are consistent the literature, as the value of such benefits can be dissipated through rent seeking.

estimates that vary over time and fraud sample), and the costs of fraud (with estimates that vary between literature estimates and in-sample estimates). We illustrate the range of costs in Table 8.

In our main experiment result, we estimate an average of 13% of firms engaged in fraud, composed of 4% detected fraud and 9% not-detected fraud. The cost for these fraud firms, using cost estimates from the literature, is 15.2% of equity value. (i.e. 15.2%=(0.04/0.13)*0.25+(0.09/0.13)*0.11). The cost using within-sample fraud costs, is 30.6% of equity value. Looking at all large traded firms, the deadweight cost of frauds is thus between 2.0% to 4.0% of equity value per year (i.e. pervasiveness (column 1) * deadweight loss for fraud firms (column 3)). Expressed differently, the annual cost of fraud among large US corporations is between $181 and $364 billion.[21]

Panels B and C provide cost estimates under alternative assumptions. This allows a researcher, for example, to limit their attention to sub-samples (such as financial fraud), or to use more conservative assumptions for detection likelihood or the estimates based on the regressions reported in Table 6. By all metrics, costs are substantial.

VI. Applications to Cost-Benefit Analysis of Regulation

The D.C. Circuit Court has struck down certain the Securities and Exchange Commission proposed rule for proxy access because the SEC failed to conduct a proper cost-benefit analysis of the new rule (Business Roundtable v. SEC, 647 F.3d 1144, 1150 (D.C. Cir. 2011)). While the same principles do not apply to Congress, there is an increasing trend to request a cost-benefit analysis for any kind of regulation. The possibility of conducting such an analysis depends crucially upon the existence of reliable estimates of the potential benefits of regulation. Since one of the goals of regulation is to reduce fraud, our estimates of the cost and pervasiveness of fraud can be used for such an analysis. We sketch out two possible applications.

[21] The total equity value of U.S. publicly traded firms with more than $750 million in assets is $15.3 trillion in 2004, and the average fraud lasts 1.67 years.

VI.1 A Cost Benefit Analysis of SOX

The easy part of any cost benefit analysis is the estimation of costs. Hochberg, Sapienza and Vissing-Jorgensen (2009) exploit survey data collected by Finance Executives International (FEI) to arrive to an estimate of $3.8 million of compliance costs per firm, with costs increasing in the issuer's size.[22] To calculate compliance costs in our sample of firms above $750 million in capitalization, we multiply the compliance costs by the number of firms, producing an estimate of total compliance costs of $6.8 billion per year.

With the annual cost of fraud in the range of $181-$364 billion, the benefits of SOX would exceed the costs if SOX reduced the probability of starting a fraud by between 2% and 4%. In Table 6 (model 3), the point estimate of the effect of SOX on the probability of fraud detection is 1.3%, i.e. a 33% increase in the probability of detection.[23] If we regard as plausible that a 33% increase in detection might lead to at least a 2-4% decrease in the amount of fraud initiated, then SOX is justified on a cost benefit analysis.

VI.2 A Cost Benefit Analysis of Mandatory Auditor Rotation

The initial version of SOX mandated a periodic rotation of auditing firms, as it takes place in many countries. There are clear costs in mandatory rotation. The General Accounting Office (GAO, 2003) estimates that mandatory rotation could lead to an increase of audits costs of between 43% and 128% in the initial year. The GAO (GAO, 2008) also puts the average audit fee for large companies at 0.08% of sales. Setting the GAO estimate of increase in audit fees at the midpoint (85.5%) and assuming a ratio of assets to sales equal 1, we can estimate that mandatory rotation would cost our universe of firms above $750 million in assets a total of $14 billion (0.855*0.0008*21 trillion).

[22] See Hochberg, Sapienza and Vissing-Jorgensen (2009), Table 11 on page 571 for the costs by size categories that we use in our estimates.
[23] This estimate assumes that all the increase in fraud detection is due to SOX and not to an increase in monitoring after a crisis. Ideally, we would be able to estimate separately the two effects.

What are the benefits to justify these costs? If we think of the demise of Arthur Andersen as an unexpected mandatory rotation, our analysis provides an estimate. Table 3 Panel B shows that a forced rotation increases in the probability of detection of an accounting fraud from 0.9% to 2.8%.

Since we estimated the real cost of fraud at 15% of the equity value, forced auditor turnover has the potential of reducing the cost of fraud by $46 billion (0.02*0.15*15.3 trillion). We say "potential" because the cost is saved only if the fraud is prevented and we do not have good estimates of the deterrence effect. Thus, we can conclude that a one-off auditor rotation imposed to all firms above $750 million in assets would have a cost of $14 billion and a potential benefit of $46 billion.

Any form of mandatory rotation, however, will not mandate it every year. Most likely, it will be every 6 or 9 years. Thus, the annual cost of mandatory rotation would be divided by 6 or 9. It is less clear how the benefit will be affected. If the threat of rotation in 9 years is able to prevent a fraud today, then the annual benefit of rotation will remain constant, making mandatory rotation even more appealing. If, as it is likely, the benefit of preventing fraud decays if the rotation takes place far in the future, then the annual benefit might be smaller. The worst case scenario is that prevention works just the year before mandatory rotation. In that case the annual benefit will be divided by 6 or 9 as well. Yet, the conclusion is the same: the estimated potential benefits exceed the estimated costs.

VII. Conclusion

In this paper we try to quantify the pervasiveness of corporate fraud in the United States and to assess its costs. The major problem in any such a study is how to estimate the amount of undetected fraud. We follow different approaches to infer the unconditional probability that a fraud is committed, regardless of whether it is subsequently caught. Irrespective of the method used, we find that only 1 in 3 frauds is detected. Using this estimate we conclude that there is fraud in about one firm of every 8 (13%).

Having established the incidence of fraud, we try to estimate how much frauds cost investors. We estimate the cost of non-detected frauds to be lower than the costs of detected fraud, but still substantial. With US traded firms with more than $750 million in assets collectively having an equity value of more than $15 trillion, this implies an annual cost of fraud among large US corporations ranging from $180 to $360 billion.

Finally, we present two illustrations of how our estimates can be used to do cost-benefit analysis of regulation. First, we compare the benefits of the reduced incidence of fraud after the introduction of SOX with the compliance cost associated with it. Our estimates show that it would take a very minimal deterrence effect of SOX on fraud (2-4%) to justify its introduction. We also compare the benefits of mandatory auditors' rotation in terms of fraud discovery with the estimated additional costs it imposes on auditors and firms. We find that a one-off mandatory rotation would have a benefit of $46 billion and a cost of $14 billion. These are just illustrative examples. More research is needed to settle these questions beyond any reasonable doubt. These examples, though, show the wide potential applications of our estimates.

Appendix I: Dyck, Morse, and Zingales (2010) Filters to Eliminate Frivolous Fraud

First, DMZ(2010) restrict attention to alleged frauds in the period of 1996 -2004, specifically excluding the period prior to passage of the Private Securities Litigation Reform Act of 1995 (PSLRA) that was motivated by a desire to reduce frivolous suits and among other things, made discovery rights contingent on evidence. Second, they restrict attention to large U.S. publicly-traded firms, which have sufficient assets and insurance to motivate law firms to initiate lawsuits and do not carry the complications of cross-border jurisdictional concerns. In particular, they restrict attention to U.S. firms with at least $750 million in assets in the year prior to the end of the class period (as firms may reduce dramatically in size surrounding the revelation of fraud). Third, they exclude all cases where the judicial review process leads to their dismissal.[24] Fourth, for those class actions that have settled, they only include those firms where the settlement is at least $3 million, a level of payment previous studies

[24] They retain cases where the reason for dropping the suit is bankruptcy for in this instance the cases could still have had merit but as a result of the bankruptcy status, plaintiff lawyers no longer have a strong incentive to pursue them.

suggested to divide frivolous suits from meritorious ones.[25] Fifth, they exclude those security frauds that Stanford classifies as non-standard, including mutual funds, analyst, and IPO allocation frauds.[26] The final filter removes a handful of firms that settle for amounts of $3 million or greater, but where the fraud, upon their reading, seems to have settled to avoid the negative publicity.[27]

[25] Grundfest (1995), Choi (2007) and Choi, Nelson, and Pritchard (2009) suggest a dollar value for settlement as an indicator of whether a suit is frivolous or has merit. Grundfest establishes a regularity that suits which settle below a $2.5 -$1.5 million threshold are on average frivolous. The range on average reflects the cost to the law firm for its effort in filing. A firm settling for less than $1.5 million is most almost certainly just paying lawyers fees to avoid negative court exposure. To be sure, we employ $3 million as our cutoff.

[26] Stanford Class Action Database distinguishes these suits for the reason that all have in common that the host firm did not engage in wrongdoing. IPO allocation cases focus on distribution of shares by underwriters. Mutual fund cases focus on timing and late trading by funds, not by the firm in question. Analyst cases focus on false provision of favorable coverage.

[27] The rule they apply is to remove cases in which the firm's poor ex post realization could not have been known to the firm at the time when the firm or its executives issued a positive outlook statement for which they are later sued.

Appendix II: Calculation of Beneish's Probability of Manipulation Score (ProbM Score)

The components in the ProbM Score include days sales in receivables, gross margin, asset quality index, sales growth index, depreciation index, SGA index, leverage, and the ratio of accruals to assets. (Please refer to Beneish (1999) for motivation of how each of these subindices captures an aspect of manipulation.) To construct the ProbM Score, we use Compustat, data to construct the variable components following Beneish (1999) and apply his estimated coefficients.

The probability of manipulation, ProbM Score, of Beneish (1999) is calculated as follows:

$$\begin{aligned}\text{ProbM} \;=\; & -4.84 \;+\; 0.92 * \text{DSR} \;+\; 0.528 * \text{GMI} \;+\; 0.404 * \text{AQI} \;+\; 0.892 * \text{SGI} \;+\; 0.115 * \text{DEPI} \\ & + 0.172 * \text{SGAI} \;+\; 4.679 * \text{ACCRUALS} \;-\; 0.327 * \text{LEVI}\end{aligned}$$

The variable codes are defined as follows:

DSR = Days Sales in Receivables

GMI = Gross Margin Index

AQI = Asset Quality Index

SGI = Sales Growth Index

DEPI = Depreciation Index

SGAI = Sales, General and Administrative expenses Index

ACCRUALS - Total Accruals to total assets

LEVI = Leverage Index

Appendix III – Partial Observability Bivariate Probit Estimates of Detection Likelihood

Let E_{it} be the incentive for firm i to engage in fraud at time t. Fraud is committed if E_{it} is positive:

$$\begin{aligned} E_{it} &= X_{it}^{E}\Gamma_{E} + \mu_{it} \\ engage_{it} &= 1 \; if \, E_{it} > 0. \end{aligned} \tag{11}$$

E_{it} is a function of observables X_{it}^{E}, which includes the high incentives and opportunities indicators and the variables in Table 6: $X_{it}^{E}\,' = \left[I_{it}^{HiIncentOpp} \quad \mathrm{x}_{it}^{E} \right]$.

Identification in Poirier's model comes from two pieces. First, Poirier assumes that $\left(\mu_{it}, \nu_{it}\right)$ are distributed bivariate standard normal. Second, identification depends on the ability to come up with variables which affect either firms' incentives to engage in fraud or detectors' ability to uncover fraud, but not both. Under the assumption that some of the X_{it}^{E} and some of the X_{it}^{D} are excluded from each other's set, the parameters in Γ_{E} and Γ_{D} can be identified using a bivariate probit model:

$$Pr\left(engage_{it}, caught_{it} \,\middle|\, X_{it}^{E}, X_{it}^{D}\right) = \Phi\left(X_{it}^{E}\Gamma_{E},\; X_{it}^{D}\Gamma_{D}\right), \tag{12}$$

where $\Omega(\cdot,\cdot)$ denotes joint cumulative standard normal distribution over the two arguments.

The abnormal ROA and abnormal stock return included in Table 6 affect the likelihood of detection, but not the likelihood of engaging in fraud as they would not be known at the time when a firm started to engage in fraud. In Appendix Table 3 we also include two types of variables that we assume influence the likelihood of engaging in frauds but not detection. First, we include compensation-related measures as prior papers have found option grants to be an important predictor of fraud (e.g. Burns and Kedia (2006), Efendi, Srivastava and Swanson (2007)). We use Execucomp's valuation of all of the options held by top executives. We also measure the incentives provided by their most recent pay package to focus on future as opposed to current performance as captured in percentage of restricted stock grants divided by total compensation. Second, we include measures of market conditions under the assumption that the starting of frauds may be procyclical. Wang, Winton and Yu (2010) review theoretical bases for procyclicality in fraud and provide some evidence that positive sentiment (captured by IPO conditions) has positive, albeit non-linear, association with the likelihood to start committing fraud. To capture time trends in market conditions, we use the sentiment index of Baker and Wurgler (2006) and this index squared.

Appendix Table 3 reports the results of the bivariate probit. Column 1 provides the coefficient estimates and marginal effects for the start equation. Our focus is on column 2 that reports the coefficient estimates for the detection equation. As in the probit of Table 6, the indicator variables are generally positive (5 of the 6 indicators). In 5 of the 6 indicator variables, the signs of the indicator variables flip between the detection and the start equations, consistent with variables that increase detection reducing the likelihood of starting.

With these estimates, we now recompute the detection likelihood reported in the bottom of the table. Comparing the detection rate in the non-high state and in the high state we arrive at a detection likelihood of 0.153. Thus, the estimate of the unconditional likelihood of fraud is 26.4% (4.04%/0.153). As predicted, relaxing assumption 5 does lower the detection likelihood, and raise the predicted unconditional estimate of fraud.

Appendix IV – Process for Calculating Implied Value Loss not Attributable to Changes in Fundamentals

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We follow Berger and Ofek's (1995) multiples approach with modification to exploit firm-specific information. Assume that a fraud begins right after time *s* and ends before time *t*. The pre-fraud enterprise multiple, specific to firm *i*, which resides in industry *j*, is:

$$m_{ijs} = \frac{Long\ Term\ Debt_{is} + \ Market\ Equity_{is}}{Y_{is}},$$

where we consider several valuation bases, $Y \in \{EBITDA, revenue, \ fixed\ assets\}$. Likewise, we define a pre-fraud industry multiple, M_{js}, as the revenue-weighted average multiple for SIC 2-digit industries, indexed by *j*. We exclude the fraud firm in this calculation. We do the same procedure at time *t*, the year ending *after* the fraud revelation date to get M_{jt}. We use the change in the industry multiple as the benchmark for how the firm's multiple would have evolved over the time period if it was just impacted by factors affecting the industry; i.e.:

$$\hat{m}_{ijt} = m_{ijs} \frac{M_{jt}}{M_{js}}.$$

The idea is to compare the fraud firm's value of debt and equity at time *t* with the debt and equity which would be projected by the firm's pre-fraud multiple adjusted to a growth or decline rate in its industry benchmark multiples. The estimated "but-for" or counterfactual valuation is thus the EBITDA, sales, or fixed assets implied enterprise value at time *t*, calculated as:

$$Counterfactual\ Enterprise\ Value = \hat{m}_{ijt} Y_{it},$$

$$for\ Y_{it} \in \{revenue_{it}, \ fixed\ assets_{it}, EBITDA_{it}\}.$$

The next step is to compare the counterfactual with the actual enterprise value post fraud revelation to produce a dollar loss per firm arising from the fraud. To ensure comparability across firms we also express this dollar loss relative to the pre-fraud enterprise value to define the fraud loss as a percentage of enterprise value.

$$Cost\ Caught\ Fraud_{it} = Counterfacutal\ Enterprise\ Value_{it} - \left(Long\ Term\ Debt_{\text{it}} + \ Equity_{\text{it}}\right).$$

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FIGURE 1: Frequency of Observed Fraud According to Various Definitions

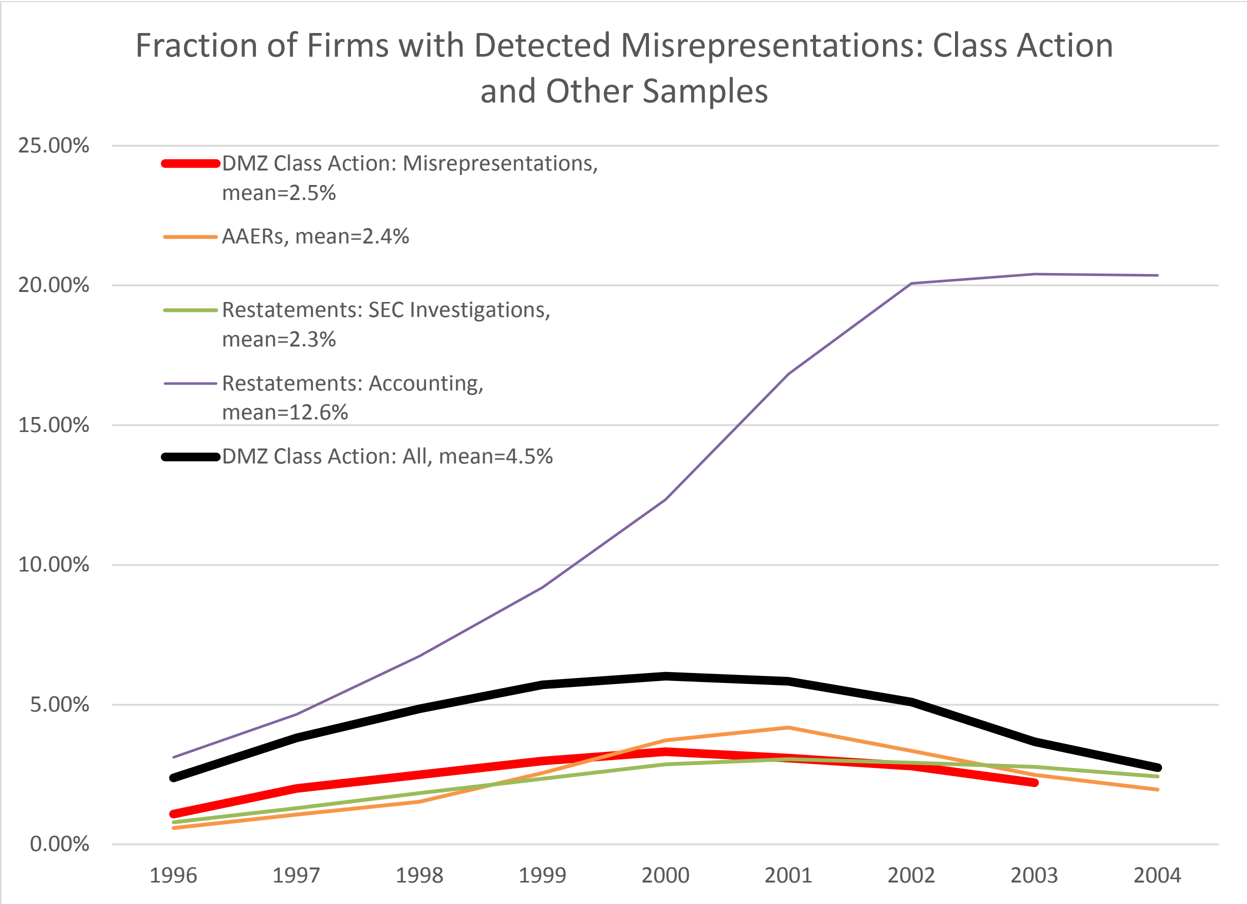

Table 1: Statistics: Arthur Andersen Clients (AA) and Non-AA Clients, 1998-2000

The sample is all Compustat firms with $750 million in assets in the 1998-2000 period. AA (Arthur Andersen) clients are those with AA as their auditor in 2001 or 2002. Non-AA firms are all other firms meeting the size cutoff, except in the farthest right columns, where we remove non-Big 5 auditor clients from the non-AA sample for comparability. In Panel A, the p-values are ttests for the mean comparisons and ranksum tests for the medians. Leverage is long term debt divided by assets. In Panel B, we present the industry distribution of firms and present Pearson distribution equivalence tests.

Panel A: Firm Characteristics

	AA Firms Observations = 496		All Non-AA Observations = 2,067				Big 5 Non-AA Observations = 1,626			
	Mean	Median	Mean	p-value	Median	p-value	Mean	p-value	Median	p-value
Assets	7,003	2,011	10,503	0.0704	2,036	0.8913	10,133	0.0710	2,117	0.8913
Sales	3,566	1,480	4,597	0.0674	1,377	0.9420	4,719	0.0416	1,457	0.9420
EBITDA	559.9	196	704.7	0.1691	185	0.5064	721.4	0.1016	200	0.5064
Leverage	0.336	0.308	0.276	0.0000	0.252	0.0069	0.276	0.0000	0.256	0.0000

Panel B: Industry Distribution

	AA Firms Observations = 496 Percent of Distribution (*100)	All Non-AA Observations = 2,067 Percent of Distribution (*100)	Big 5 Non-AA Observations = 1,626 Percent of Distribution (*100)
Agriculture	0.40	0.19	0.18
Banks & Insurance	11.49	20.90	19.50
Chemicals	3.23	4.40	4.49
Communication & Transport	17.74	11.42	10.58
Computers	4.64	9.48	9.04
Durable Manufacturing	10.28	12.72	13.65
Food & Tobacco	1.61	2.71	2.46
Lumber, Furniture, Printing	4.23	4.74	4.80
Mining & Construction	2.42	1.84	1.85
Pharmaceuticals	1.41	2.85	3.08
Refining Extractive	4.03	2.23	2.28
Retail & Wholesale	6.45	8.56	8.55
Services & Healthcare	11.09	7.40	7.32
Textile & Apparel	1.01	1.11	1.11
Utilities	19.96	9.43	11.13
	100	100	100
Pearson's Chi-Square test of Distribution Equivalence of non-AA Samples to AA Sample			
Statistic		215.3	267.9
P-value		0.000	0.000

Table 2: Were Arthur Andersen Clients More Fradulent in 1998-2000 than other Corporations?

Panel A: Univariate tests: Probability of manipulation scores (ProbM Score) are from Beneish (1999), Fraud score (Fscore) are from Dechow, Ge, Larson and Sloan (2011), the occurance of a class action securities fraud suit (Class Action Fraud) are from Dyck, Morse, and Zingales (2010), AAERS are from the CFRM at Berekeley-Haas, SEC investigated restatements are from AuditAnalytics, and accounting restatements are from AuditAnalytics. Panel A reports cross sectional t-test of differences in mean fraud rates, comparing 1998-2000 large corporations who had AA as auditor to all others, or to those with another Big 5 auditor.

	All Firms		AA Firms		All Non-AA		P-value	Big 5 Non-AA		P-value
	Mean	Obs.	Mean	Obs.	Mean	Obs.	(versus AA)	Mean	Obs.	(versus AA)
ProbM Score	-1.583	2,342	-1.614	463	-1.575	1,879	0.701	-1.621	1,482	0.944
Fscore	5.186	2,142	4.093	429	5.459	1,713	0.318	4.823	1,363	0.565
Fraud	0.0306	2,545	0.0283	494	0.0312	2,051	0.740	0.0323	1,612	0.662
AAERs	0.0188	2,553	0.0182	494	0.0189	2,059	0.916	0.0185	1,619	0.964
Restatements: SEC Investigations	0.0137	2,551	0.0161	496	0.0131	2,055	0.608	0.0142	1,615	0.760
Restatements: Accounting	0.0565	2,514	0.0719	487	0.0528	2,027	0.102	0.0539	1,597	0.137

Panels B & C: Multivariate Tests: The dependent variables are measures of fraud, as in panel A. Industry fixed effects, when included, are at the 2 digit SIC level. Estimation is either by ordinary least squares (OLS) or by quantile median regressions, as designated. Pseudo R-squared are reported for quantile estimations. ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively. Standard errors in brackets are clustered at the firm level.

Panel B: Unobservable Measures of Fraud Dependent Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent var.:			Accounting Manipulation (ProbM)						Fraud Score (Fscore)			
Non-AA sample:		All non-AA		Big 5	Big 5	Big 5		All non-AA		Big 5	Big 5	Big 5
Estimation:	OLS	OLS	Quantile	OLS	OLS	Quantile	OLS	OLS	Quantile	OLS	OLS	Quantile
Arthur Andersen	-0.0039	-0.0207	0.0215	0.0110	-0.0096	0.0217	-0.3340	-0.1580	0.0296	-0.1360	-0.0591	0.0361
	[0.0499]	[0.0532]	[0.0220]	[0.0512]	[0.0541]	[0.0224]	[0.531]	[0.594]	[0.0295]	[0.535]	[0.614]	[0.0304]
Log Assets	-0.0703***	-0.0560***	-0.012	-0.0761***	-0.0633***	-0.0109	-0.391	-0.176	0.0459***	-0.317	-0.0826	0.0494***
	[0.0179]	[0.0199]	[0.00775]	[0.0188]	[0.0205]	[0.00842]	[0.253]	[0.274]	[0.0106]	[0.270]	[0.271]	[0.0117]
Sales / Assets	-0.155***	-0.217***	-0.0702***	-0.142***	-0.188***	-0.0645***	-1.127***	-1.565***	0.0508**	-0.901***	-1.306***	0.0428*
	[0.0367]	[0.0549]	[0.0168]	[0.0405]	[0.0599]	[0.0180]	[0.269]	[0.402]	[0.0234]	[0.270]	[0.401]	[0.0253]
EBITDA / Sales	-2.509***	-2.440***	-0.907***	-2.387***	-2.320***	-0.864***	-23.43***	-22.74***	-1.248***	-19.27***	-18.11***	-1.317***
	[0.425]	[0.531]	[0.114]	[0.464]	[0.589]	[0.122]	[4.230]	[5.013]	[0.151]	[4.144]	[4.730]	[0.164]
LT Debt/Assets	-0.589***	-0.606***	-0.216***	-0.512***	-0.506***	-0.231***	-7.108***	-6.081***	-0.0544	-5.575***	-4.215***	-0.0615
	[0.123]	[0.131]	[0.0465]	[0.135]	[0.140]	[0.0517]	[1.522]	[1.432]	[0.0637]	[1.538]	[1.359]	[0.0713]
Fixed Effects:												
Industry	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Year	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry*Year	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Observations	5,285	5,285	5,285	4,433	4,433	4,433	4,817	4,817	4,817	4,069	4,069	4,069
R2/ Pseudo R2	0.041	0.100	0.049	0.038	0.106	0.053	0.031	0.087	0.090	0.025	0.089	0.105

Table 2 (Cont)

Panel C: Observable Measures of Fraud Dependent Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent var.:	Class Action Fraud				AAERs			
Non-AA sample:	--	--	Big 5	Big 5	--	--	Big 5	Big 5
Estimation:	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Arthur Andersen	-0.0064	-0.0045	-0.0073	-0.0061	-0.0004	0.0003	-0.0003	0.0005
	[0.00694]	[0.00726]	[0.00718]	[0.00761]	[0.00492]	[0.00493]	[0.00509]	[0.00511]
Log Assets	0.00699**	0.0102***	0.00634	0.00946**	0.00288	0.00356*	0.00295	0.00384*
	[0.00350]	[0.00378]	[0.00392]	[0.00410]	[0.00185]	[0.00193]	[0.00211]	[0.00209]
Sales / Assets	0.0108*	0.00747	0.011	0.00755	0.00961**	0.00495	0.0103**	0.00663
	[0.00629]	[0.00824]	[0.00722]	[0.00935]	[0.00419]	[0.00537]	[0.00476]	[0.00625]
EBITDA / Sales	-0.0812*	-0.106*	-0.0796	-0.101	-0.0137	-0.00247	-0.0126	0.000752
	[0.0484]	[0.0557]	[0.0543]	[0.0627]	[0.0176]	[0.0212]	[0.0194]	[0.0233]
LT Debt/Assets	-0.0106	-0.00714	-0.0235	-0.0216	0.0111	0.0138	0.014	0.0184
	[0.0137]	[0.0150]	[0.0150]	[0.0165]	[0.00964]	[0.0113]	[0.0111]	[0.0123]
Fixed Effects:								
Industry	N	Y	N	Y	N	Y	N	Y
Year	Y	Y	Y	Y	Y	Y	Y	Y
Industry*Year	N	Y	N	Y	N	Y	N	Y
Observations	5,844	5,844	4,916	4,916	5,867	5,867	4,933	4,933
R-squared	0.007	0.046	0.008	0.054	0.006	0.029	0.007	0.034

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Dependent var.:	Restatements: SEC Investigations				Restatements: Accounting			
Non-AA sample:	--	--	Big 5	Big 5	--	--	Big 5	Big 5
Estimation:	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Arthur Andersen	0.0034	0.0033	0.0026	0.0023	0.0062	0.0093	0.0050	0.0089
	[0.00455]	[0.00412]	[0.00468]	[0.00429]	[0.00843]	[0.00845]	[0.00870]	[0.00861]
Log Assets	0.000697	0.0022	0.000489	0.00194	-0.0113***	-0.00959***	-0.0114***	-0.00971***
	[0.00161]	[0.00159]	[0.00181]	[0.00171]	[0.00254]	[0.00264]	[0.00294]	[0.00305]
Sales / Assets	-0.00205	-0.00638**	-0.00209	-0.00574*	-0.00436	-0.0136**	-0.00675	-0.0191***
	[0.00186]	[0.00295]	[0.00216]	[0.00345]	[0.00423]	[0.00583]	[0.00455]	[0.00594]
EBITDA / Sales	-0.0113	-0.0278*	-0.00733	-0.0229	-0.0962**	-0.127***	-0.0818**	-0.108**
	[0.0133]	[0.0156]	[0.0146]	[0.0172]	[0.0378]	[0.0424]	[0.0413]	[0.0455]
LT Debt/Assets	0.00152	0.00292	0.00366	0.00472	0.0028	-0.00486	0.00364	0.00152
	[0.00631]	[0.00798]	[0.00689]	[0.00898]	[0.0147]	[0.0167]	[0.0168]	[0.0189]
Fixed Effects:								
Industry	N	Y	N	Y	N	Y	N	Y
Year	Y	Y	Y	Y	Y	Y	Y	Y
Industry*Year	N	Y	N	Y	N	Y	N	Y
Observations	5,867	5,867	4,931	4,931	5,751	5,751	4,843	4,843
R-squared	0.001	0.021	0.001	0.021	0.011	0.043	0.011	0.05

Table 3: Detection Likelihood Estimates from the Arthur Andersen Experiment

The table presents estimates of the detection likelihood using the AA experiment. The sample is U.S. publicly traded corporations with more than $750 million in assets. A firm is identified as being an Arthur Andersen (AA) client if it was audited by AA anytime during 2000-2002. In all cases, the frauds considered are those starting prior to 2002. Auditor-detected frauds are frauds in the DMZ (2010) sample of class action frauds which were detected by an auditor either by an auditor resignation or by the auditor issuing a qualified opinion and either the firm or analysts revealing the fraud. The sample 'Restatements: Accounting' are from AuditAnalytics and refer to restatements triggered by accounting mis-application. The sample 'Restatements: SEC Investigations' are restatements from AuditAnalytics accompanied by an SEC investigation. AAERs are the SEC investigation releases used in Dechow, Ge, Larson and Sloan (2011). The 'Financial Misrepresentations' sample is from DMZ and includes all class action frauds where the alleged mis-doing involved financial misrepresentations. The sample 'All Class Actions' is from DMZ and includes any securities class action fraud. In column (1), the detection likelihood comes from the ratio of columns (b) and (a) and refers to the detection likelihood of frauds being detected in 2001-2002 period. In column (2), the detection likelihood is calculated on a sample that include frauds detected in the 2001-2003 period. In column (3), a placebo, we consider frauds detected in 2004-2006. Columns (a) and (b) present the joint probabilities of having started a fraud and being caught. The p-values test differences of these calculations in t-tests. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level.

	(a)	(b)	(c)	(1)	(2)	(3)
Panel A: Main Experiment Result						
	Pr(FF, caught): Observed Financial Fraud		p-value of difference	*Pr(caught | FF)*: Detection Likelihood 2001-2002	*Pr(caught | FF)*: Detection Likelihood 2001-2003	
	AA	non-AA				
Auditor-Detected Frauds	0.0282	0.0088	0.0010	0.311***	0.291***	
Panel B: Robustness in Different Samples of Financial Fraud						
	Pr(FF, caught): Observed Financial Fraud		p-value of difference	*Pr(caught | FF)*: Detection Likelihood 2001-2002	*Pr(caught | FF)*: Detection Likelihood 2001-2003	*Pr(caught | FF)*: Detection Likelihood 2004-2006 (placebo)
	AA	non-AA				
Restatements: Accounting	0.1385	0.0457	0.0000	0.330***	0.410***	1.053
Restatements: SEC Investigations	0.0282	0.0083	0.0006	0.295***	0.573*	0.946
AAERs	0.0333	0.0185	0.0581	0.554*	0.661	0.901
Financial Misrepresentations	0.0308	0.0143	0.0201	0.465**	0.476**	
Panel C: Robustness to Sampling of All Frauds						
	Pr(F, caught): Observed Fraud		p-value of difference	*Pr(caught | F)*: Detection Likelihood 2001-2002	*Pr(caught | F)*: Detection Likelihood 2001-2003	
	AA	non-AA				
All Class Actions	0.0538	0.0277	0.0067	0.515***	0.525***	

Table 4: Detection Likelihood Robustness to Industry and Region Subsampling

The table presents estimates of the detection likelihood using the AA experiment, from Table 3 column (2), by industry and region subsampling. The overall sample is U.S. publicly traded corporations with more than $750 million in assets. Industries and regions are excluded, as noted in the rows. A firm is identified as being an Arthur Andersen (AA) client if it was audited by AA anytime during 2000-2002. This table considers frauds caught during 2001-2003. Auditor-detected frauds are frauds in the DMZ (2010) sample of class action frauds which were detected by an auditor either by an auditor resignation or by the auditor issuing a qualified opinion and either the firm or analysts revealing the fraud. Restatements are from AuditAnalytics and refer to restatements triggered by accounting mis-application. In both panels, for each fraud sample the first two columns are the joint probabilities of having started a fraud and being caught and the third column is the detection likelihood that comes from the ratio of columns (1) and (2) for that fraud sample. The p-values test differences of these calculations in t-tests. *, **, and *** indicate statistical significance at the 10%, 5% and 1% level.

Panel A: Robustness in Excluding Industries

	Auditor-Detected Frauds			Restatements: Accounting		
	Pr(FF, caught): Observed Financial Fraud		*Pr(caught | F)*: Detection Likelihood	*Pr(FF, caught)*: Observed Financial Fraud		*Pr(caught | F)*: Detection Likelihood
	AA	non-AA		AA	non-AA	
Full Sample (Table 3, column 2)	0.0333	0.0097	0.291***	0.1667	0.0684	0.410***
Excluding Less Represented Industries						
Banks & Insurance	0.0319	0.0135	0.424**	0.1710	0.0754	0.441***
Computers	0.0292	0.0080	0.274***	0.1671	0.0645	0.386***
Retail &Wholesale	0.0329	0.0095	0.278***	0.1671	0.0697	0.417***
Excluding More Represented Industries						
Refining & Extractive	0.0349	0.0099	0.284***	0.1667	0.0685	0.411***
Communication &Transport	0.0242	0.0091	0.374**	0.1576	0.0689	0.438***
Utilities	0.0363	0.0096	0.263***	0.1518	0.0674	0.444***
Services & Health	0.0373	0.0093	0.249***	0.1667	0.0657	0.394***

Panel B: Robustness in Excluding Regions and States

	Auditor-Detected Frauds			Restatements: Accounting		
	Pr(FF, caught): Observed Financial Fraud		*Pr(caught | F)*: Detection Likelihood	*Pr(FF, caught)*: Observed Financial Fraud		*Pr(caught | F)*: Detection Likelihood
	AA	non-AA		AA	non-AA	
Full Sample (Table 3)	0.0333	0.0097	0.291***	0.1667	0.0684	0.410***
Excluding Regions:						
Northeast	0.0276	0.0095	0.345***	0.1748	0.0691	0.395***
Southeast	0.0336	0.0102	0.304***	0.1779	0.0726	0.408***
Southwest	0.0380	0.0098	0.258***	0.1637	0.0645	0.394***
West	0.0311	0.0074	0.238***	0.1695	0.0613	0.362***
Mountain	0.0350	0.0095	0.272***	0.1590	0.0681	0.428***
Midwest	0.0381	0.0103	0.271***	0.1557	0.0705	0.453***
Foreign	0.0323	0.0107	0.330***	0.1613	0.0695	0.431***
Excluding Large States:						
Texas	0.0365	0.0096	0.264***	0.1573	0.0654	0.416***
California	0.0303	0.0082	0.270***	0.1708	0.0609	0.357***
New York	0.0296	0.0091	0.307***	0.1667	0.0680	0.408***

Table 5: Fraud Pervasiveness Estimates

This table presents estimates of fraud pervasiveness in publicly traded US firms with more than $750million in assets, along with data used to generate these estimates. The first four detection likelihood estimates in column 1 are from the AA experiment and are described more fully in Table 3. The final detection likelihood estimate in column (1) is from Table 6. The detected fraud rates in columns (2) and (4) are based on data from figure 1. Column (2) uses the 1996-2004 period, excluding the 2000 and 2001 years associated with the technology crisis. Column (4) uses the full 1996-2004 period. Fraud Pervasiveness estimates in column 3 are defined as the detected fraud rate from column (2) divided by the detection likelihood from column (1). Fraud Pervasiveness estimates in column 5 are defined as the detected fraud rate from column (4) divided by the detection likelihood from column (1).

	(1)	(2)	(3)	(4)	(5)
	Detection Likelihood	*Detected Fraud Rate excluding 2000-2001*	*Fraud Pervasiveness*	*Detected Fraud Rate, 1996-2004*	*Fraud Pervasiveness using 1996-2004 Sample*
Panel A: Main Experiment Result					
DMZ Class Action, All-fraud sample	0.311	4.04%	13.0%	4.46%	14.3%
Panel B: Robustness in Different Samples of Financial Fraud					
DMZ Class Action, Financial fraud sub-sample	0.311	2.26%	7.3%	2.50%	8.0%
Restatements: Accounting	0.330	12.08%	36.6%	12.63%	38.3%
Panel C: Robustness to Alternative Detection Likelihood Estimates					
DMZ Sample: Assumption 3A that all NFF are caught	0.554	4.04%	7.3%	4.46%	8.1%
DMZ Sample: Probit estimates (Table 6)	0.210	4.04%	19.3%	4.46%	21.3%

Table 6: Detection Likelihood Estimates from Probit Estimation

The dependent variable is an indicator that takes the value 1 if the firm is detected as a fraud in that year. The fraud sample is securities class action fraud for firms with more than $750 million in assets from DMZ (2010). The table reports marginal effects of a probit estimation. In columns (3) and (4), the table includes indicator variables to capture the presence or absence of fraud detectors found to have a significant impact on detection in DMZ (2010). These high attention to detection variables include: high analyst attention relative to industry, high media attention, high shortability (institutional ownership) relative to industry, qui tam applicable industry, regulated industry, and PostSOX. Covariates include firm size (log of assets), accounting performance (ROA), abnormal accounting performance, stock market performance (stock return), abnormal stock performance, R&D intensity (R&D/assets), leverage, firm market share, the Herfindahl index at the SIC2 industry level, VIX and the one year percentage change innovation to VIX. In column (2) and (4) the table also includes the Beneish Prob(M) measure, which is an indicator of potential financial statement manipulation based on firm-specific variables from financial statements (defined more fully in Appendix II)). Appendix Table A3 provides defitions of the variables used in this probit estimation. The final row of the table presents the detection likelihood estimate based on these probit estimation estimations. Robust standard errors are in brackets. ***, **, and ** denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)
	Marginal Effects from Probit (Fraud Detected=1)			
Log Assets (lag)	0.00286**	0.00239**	0.0018	0.00105
	[0.00114]	[0.00112]	[0.00124]	[0.00122]
Firm Marketshare	0.00002	0.000025	-0.00003	-0.000025
	[6.69e-05]	[6.52e-05]	[6.81e-05]	[6.56e-05]
Industry Herfindahl Index	0.0181	0.0140	0.0166	0.0143
	[0.0192]	[0.0195]	[0.0206]	[0.0206]
R&D/Assets (lag)	0.149***	0.142***	0.0566	0.0502
	[0.0334]	[0.0340]	[0.0382]	[0.0371]
Leverage (lag)	0.0290***	0.0276***	0.0231***	0.0211***
	[0.00651]	[0.00645]	[0.00650]	[0.00642]
ROA (lag)	-0.0156	-0.0105	-0.0312*	-0.0283
	[0.0208]	[0.0215]	[0.0187]	[0.0183]
Abnormal ROA (lag)	-0.000007	0.000007	-0.000091	-0.000083
	[7.62e-05]	[8.01e-05]	[7.90e-05]	[8.61e-05]
Stock Return (lag)	-0.00266	-0.000207	-0.00053	0.00139
	[0.00407]	[0.00319]	[0.00341]	[0.00212]
Abnormal Stock Return (lag)	-0.000495	-0.0099	-0.00696	-0.0141
	[0.0132]	[0.0136]	[0.0128]	[0.0129]
VIX	0.00247***	0.0028***	0.00209***	0.0024***
	[0.00050]	[0.00050]	[0.000433]	[0.00043]
Innovation to VIX	-0.00074	-0.00093**	0.00021	-0.000090
	[0.000459]	[0.00047]	[0.000492]	[0.00048]
Beneish Prob(Manipulation)		0.00183		0.00176
		[0.00147]		[0.00122]
<u>High Attention to Detection Variables</u>				
Hi Analysts (lag)			0.00935***	0.0107***
			[0.00291]	[0.00292]
Hi Media (lag)			-0.00244	-0.00116
			[0.00336]	[0.00325]
Hi Shortability (lag)			0.00601**	0.00630**
			[0.00298]	[0.00292]
Qui Tam Industry			0.0324***	0.0302***
			[0.0106]	[0.0101]
Regulated			-0.00252	-0.00203
			[0.00399]	[0.00402]
PostSOX			0.0142***	0.0128***
			[0.00454]	[0.00435]
Observations	7699	7354	7699	7354
Pseudo R-squared	0.0463	0.0472	0.0805	0.0855
LR/Wald Chi-Square	63.82	58.27	127.6	132.5
<u>Detection Likelihood Calculation</u>: Predicted Prob(Detect | As Observed) / Predicted Prob (Detect | As Observed except All High Variables =1)			0.219	0.210

Table 7: In-Sample Costs of Fraud Estimates

The table presents summary statistics and costs estimates for fraud based on the DMZ (2010) sample. The statistics are reported only for fraud firms of DMZ's original sample of 216 firms which have statistics for pre and post periods. The pre and post columns represent the same set of firms, with the counts depending on availability of financial items. Panel A reports the valuation of equity, long-term debt and enterprise value as well as the financial statement line items which enter the multiples analysis. The numbers are in millions of USD. Deadweight Loss from Fraud are computed by comparing actual firm equity values post fraud with a firm counterfactual equity value that uses a multiples approach (described in Appendix IV) and assumes firm value would have grown over the fraud period as a typical firm in the industry, defining industry as the same 2-digit SIC. Reported values exclude the estimated cost of legal penalties using the estimate from Karpoff, Lee and Martin (2008). In Panel B, the AA sub-sample is defined as those firms that had AA as an auditor prior to 2001, that began their fraud pre-2001 and had their fraud end in 2001-2003.

Panel A: Statistics

	Pre-Fraud				Post-Fraud			
	Median	Mean	StDev	Frequency	Median	Mean	StDev	Frequency
Market Capitalization	4,451	14,782	29,945	199	1,969	9,643	19,401	199
Long Term Debt	791	2,774	7,420	205	856	5,948	31,667	205
Enterprise Value	6,396	17,778	32,335	191	5,256	19,105	46,544	191
EBITDA	473	1,317	2,071	160	407	1,701	3,432	160
Sales	2,468	6,971	9,275	191	3,378	8,211	11,433	191
Assets	3,461	14,099	33,800	191	4,176	22,792	78,145	191

Panel B: Cost of Detected Fraud as a Percentage of Equity Value

	All Firms				AA Sub-sample			
	Median	25th Percentile	75th Percentile	Frequency	Median	25th Percentile	75th Percentile	Frequency
EBITDA Multiple	**24.5%**	-8.6%	73.9%	160	**21%**	-19.6%	76.8%	25
Sales Muliple	**41.6%**	-3.6%	130.8%	191	**39%**	-26.7%	130.3%	29
Assets Multiple	**44.1%**	3.2%	112.9%	191	**23%**	-8.9%	104.0%	29
Average	***36.7%***				***27.8%***			

Table 8: Deadweight Cost from Fraud Estimates

The table provides estimates of the loss associated with fraud in columns 3-5, with columns 1-2 indicating the data used in the calculation. Column 1 reports the fraud pervasiveness estimate from Table 5. Column 2 reports the basis for the fraud cost estimates in columns 3-5. 'Literature' indicates costs for detected fraud come from Karpoff, Lee and Martin (2008), and costs for non-detected frauds from Beneish, Lee and Nichols (2012). 'In-sample' fraud costs use the DMZ data for detected fraud costs and the AA subsample for non-detected fraud costs from Table 7. Column 3 focuses only on fraud firms and reports the average loss arising from fraud as a percentage of equity value. Column 4 focuses on all firms and reports the fraud loss as a percentage of equity value for all traded firms with more than $750 million in assets. Column 5 reports the annual deadweight loss in dollars. This is constructed by multiplying column 4 by the value of traded equity in 2004 for publicly traded firms with more than $750 million in assets of $15.3 trillion and then dividing this by the average 1.67 year fraud duration.

	(1)	(2)	(3)	(4)	(5)
	Fraud Pervasiveness	Source of Deadweight Loss Estimates	Fraud firms: Deadweight loss as % of equity value	All traded firms: Deadweight loss as % of equity value	All traded firms: Deadweight loss per year (in billions)
Panel A: Main Experiment Result (Table 5, Panel A)					
	13.0%	Literature	15.2%	2.0%	$181
	13.0%	In-sample	30.6%	4.0%	$364
Panel B: Financial Fraud Only (Table 5, Panel B)					
	7.3%	Literature	15.2%	1.1%	$101
	7.3%	In-sample	30.6%	2.2%	$204
Panel C: Robustness - Alternative Detection Likelihood Estimates					
Conservative Assumption 3A that all NFF are caught	7.3%	Literature	18.8%	1.4%	$126
Conservative Assumption 3A that all NFF are caught	7.3%	In-sample	32.7%	2.4%	$219
Probit estimates (Table 6)	19.3%	Literature	13.7%	2.6%	$242
Probit estimates (Table 6)	19.3%	In-sample	29.7%	5.7%	$525

Appendix Table A1: Propensity Score Matched, Collapsed Tests: Were AA Clients Committing More Fraud?

The sample is a cross section of firms with $750 million in assets and a Big 5 auditor during 1998-2000. Estimation in Panel A is limited to only Texas headquartered firms. Panel B replicates panel A for the entire United States. Estimation is a propensity-matched estimation. For each AA client firms, we find a non-AA match in the same two digit SIC code, matched on the propensity of assets, sales/assets, EBITDA/sales and leverage to predict being an AA client. Once matched, the dependent variables are measures of uncaught fraud (columns 1-4) or caught fraud (columns 5-12) as follows: the probability of manipulation score (prob-M score) of Beneish (1999) (columns 1-2), fraud score (Fscore) of Dechow, et al (2011) (columns 3-4), the occurance of a class action securities fraud suit (Class Action Fraud) (columns 5-6), AAERS (columns 7-8), SEC investigated restatements from AuditAnalytics (columns 9-10), and the broader set of AuditAnalytics accounting restatements (columns 11-12). All regressions include industry fixed effects (SIC2) and year fixed effects. ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively. Robust (for OLS) standard errors are in brackets.

Panel A: Texas Only Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Manipulation (ProbM)		Fraud Score (Fscore)		Class Action Fraud		AAERs		Restatements: SEC		Restatements: Acctng.	
Arthur Andersen	-0.4030	-0.5570	3.9370	4.4250	0.0009	-0.0068	0.0208	0.0114	0.0417	0.0309	0.0208	-0.0145
	[0.852]	[0.932]	[2.658]	[3.261]	[0.0422]	[0.0386]	[0.0544]	[0.0619]	[0.0292]	[0.0225]	[0.108]	[0.116]
Log Assets		-0.1140		-0.4540		0.0346		0.0580*		0.0268		0.0890
		[0.233]		[0.696]		[0.0430]		[0.0347]		[0.0292]		[0.0638]
Sales / Assets		0.4690		2.0560		0.0141		-0.0131		0.0008		0.0391
		[0.311]		[2.884]		[0.0378]		[0.0136]		[0.00357]		[0.0654]
EBITDA / Sales		0.8100		-47.60*		-0.3730		-0.4600		-0.0419		0.5140
		[4.142]		[24.31]		[0.375]		[0.281]		[0.269]		[0.675]
LT Debt/Assets		3.0470		-13.730		-0.0436		-0.0655		0.0939		0.3800
		[3.454]		[11.94]		[0.246]		[0.185]		[0.129]		[0.265]
Observations	78	78	74	74	80	80	81	81	80	80	81	81
R-squared	0.0070	0.0570	0.0240	0.1140	0.0000	0.0520	0.0020	0.0870	0.0210	0.0530	0.0010	0.0690

Panel B: United States Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Manipulation (ProbM)		Fraud Score (Fscore)		Class Action Fraud		AAERs		Restatements: SEC		Restatements: Acctng.	
Arthur Andersen	0.0298	0.0547	-0.1990	0.0610	-0.0088	-0.0102	-0.0082	-0.0095	0.0019	0.0020	0.0185	0.0183
	[0.160]	[0.157]	[1.508]	[1.521]	[0.0126]	[0.0128]	[0.0108]	[0.0110]	[0.00843]	[0.00843]	[0.0169]	[0.0169]
Log Assets		-0.108*		-0.817*		0.0148*		0.0126*		0.0121**		-0.0062
		[0.0599]		[0.428]		[0.00819]		[0.00664]		[0.00588]		[0.00824]
Sales / Assets		-0.0812		-0.7650		0.0337*		0.0172*		-0.0025		0.0004
		[0.132]		[0.836]		[0.0177]		[0.00984]		[0.00356]		[0.00992]
EBITDA / Sales		-5.870***		-57.33***		-0.238**		0.0507		-0.140*		-0.1460
		[1.175]		[20.14]		[0.116]		[0.0919]		[0.0844]		[0.108]
LT Debt/Assets		0.1480		-10.03*		-0.0179		0.0293		0.0216		0.0614
		[0.516]		[5.445]		[0.0307]		[0.0351]		[0.0299]		[0.0512]
Observations	796	796	736	736	847	847	853	853	853	853	839	839
R-squared	0.000	0.080	0.000	0.056	0.001	0.028	0.001	0.014	0.000	0.022	0.001	0.009

Appendix Table A2: Industry and Georgraphy of Fraud Samples

The sample is U.S. publicly traded corporations with more than $750 million in assets. Auditor-detected frauds are frauds in the DMZ (2010) sample of class action frauds which were detected by an auditor either by an auditor resignation or by the auditor issuing a qualified opinion and either the firm or analysts revealing the fraud. The 'Financial Misrepresentations' sample is from DMZ and includes all class action frauds where the alleged mis-doing involved financial misrepresentations. The sample 'Restatements: Accounting' are from AuditAnalytics and refer to restatements triggered by accounting mis-application. The sample 'Restatements: SEC Investigations' are restatements from AuditAnalytics accompanied by an SEC investigation. AAERs are the SEC investigation releases used in Dechow, Ge, Larson and Sloan (2011). The sample 'All Class Actions' is from DMZ and includes any securities class action fraud. In all cases, the frauds considered are those starting prior to 2002 and being detected in 2001-2003 period. Panel A presents cross-sectional correlations. Panel B and C report fraud incidence by industry or geography and the percent of frauds in the sample within the industry or geography.

Panel A: Correlations among Fraud Measures

	Auditor Detected	Misrepre-sentation	AAER	Restate: SEC	Restate: Accting	All Fraud
Auditor Detected	1.000					
Misrepresentations	0.545	1.000				
AAERs	0.169	0.250	1.000			
Restatements: SEC	0.164	0.254	0.374	1.000		
Restatements: Accounting	0.175	0.200	0.218	0.311	1.000	
All Frauds	0.594	0.730	0.311	0.318	0.265	1.000

Sample: 2704 firms (1 observation per firm) over the period 2001-2003

*Note: All Correlations are statisically significant at < 1% level

Panel B: Fraud Incidence within an Industry and Industry Distribution of the Fraud Samples

Industry	Auditor Detected	Misrepre-sentation	AAER	Restate: SEC	Restate: Accting	All Fraud	Auditor Detect	Misrepre-sentation	AAER	Restate: SEC	Restate: Accting	All Fraud
	Incidence within Industry Firms						Percent of Fraud Cases					
Agriculture	--	--	--	--	0.200	--	--	--	--	--	0.5%	--
Banks Insurance	0.005	0.011	0.017	0.009	0.059	0.026	11%	16%	19%	15%	22%	20%
Chemicals	--	--	0.010	0.020	0.070	0.010	0%	0%	1%	4%	3%	1%
CommunicationTransport	0.032	0.024	0.032	0.024	0.100	0.064	22%	11%	11%	13%	11%	15%
Computers	0.035	0.050	0.050	0.045	0.110	0.090	19%	18%	13%	19%	10%	17%
Durable Manufacturing	0.010	0.017	0.028	0.014	0.056	0.031	8%	9%	11%	9%	7%	8%
Food Tobacco	0.017	0.017	0.017	0.017	0.103	0.017	3%	2%	1%	2%	3%	1%
Lumber Furniture Printing	--	0.010	0.010	0.010	0.060	0.020	--	2%	1%	2%	3%	2%
Mining Construction	--	0.022	--	--	0.133	0.022	--	2%	--	0%	3%	1%
Pharmaceuticals	0.029	0.029	0.072	0.014	0.087	0.116	6%	4%	7%	2%	3%	7%
Refining & Extractive	--	0.015	0.015	0.015	0.092	0.031	--	2%	1%	2%	3%	2%
Retail & Wholesale	0.017	0.021	0.038	0.017	0.050	0.029	11%	9%	12%	9%	6%	7%
Services & Health	0.012	0.030	0.053	0.053	0.124	0.053	6%	9%	12%	19%	10%	8%
Textile Apparel	0.037	0.037	--	--	0.074	0.037	3%	2%	--	--	1%	1%
Utilities	0.015	0.030	0.030	0.007	0.125	0.041	11%	15%	11%	4%	16%	10%
Total	0.013	0.020	0.028	0.017	0.081	0.040	100%	100%	100%	100%	100%	100%

Panel C: Fraud Incidence within a Region and Region Distribution of the Fraud Samples

Region	Auditor Detect	Misrepre-sentation	AAER	Restate: SEC	Restate: Accting	All Fraud	Auditor Detect	Misrepre-sentation	AAER	Restate: SEC	Restate: Accting	All Fraud
	Incidence within Region Firms						Percent of Fraud Cases					
Midwest	0.011	0.021	0.029	0.020	0.082	0.029	17%	24%	22%	24%	21%	17%
Mountain	0.012	0.012	0.012	0.024	0.131	0.036	3%	2%	1%	4%	5%	3%
Northeast	0.017	0.022	0.038	0.017	0.074	0.046	29%	25%	30%	22%	20%	28%
Southeast	0.011	0.019	0.023	0.015	0.063	0.030	17%	20%	16%	17%	15%	17%
Southwest	0.007	0.024	0.034	0.014	0.109	0.044	6%	14%	14%	9%	15%	14%
West	0.027	0.021	0.024	0.021	0.111	0.054	26%	14%	11%	15%	17%	19%
Foreign	0.005	0.005	0.024	0.019	0.072	0.014	3%	2%	7%	9%	7%	3%
Total	0.014	0.020	0.029	0.018	0.084	0.037	100%	100%	100%	100%	100%	100%

Appendix Table 3: Variable Definitions and Summary Statuistics for Opportunities and Inventives Analysis

Panels A and B present, respectively, the high attention to detection variables and the covariates used both in the Table 6 probit estimation and in the Appendix Table 3 Panel C bivariate probit estimation.

Variable	Description	Source	Mean	SD
Panel A: High Attention to Detection Variables				
High Analyst Coverage	An indicator for companies with higher than the median value of analyst coverage in companies with more than $750 million in assets.	I/B/E/S	0.5001	0.5000
High Media Coverage	An indicator if the firm has higher than the median value of media coverage in companies with more than $750 million in assets. We manually collect media coverage by searching the Wall Street Journal print edition and recording the number of media hits for the year 1995.	Factiva	0.5448	0.0461
High Shortability	An indicator for companies with a greater than median level of institutional shareholding.	Compact-D	0.498	0.5000
Regulated Firm	An indicator for firm industry: financials, transportation equipment, transportation, communications, electric, gas and sanitary services, drug, drug, proprietaries and druggists sundries, petroleum and petroleum products wholesalers pharmaceuticals, healthcare providers, and healthcare related firms in business services.	Industries identified in Winston (1998)	0.4594	0.4983
Qui-Tam Industry	An indicator for the firm's industry being one in which qui tam lawsuits are possible. Included are healthcare and defense contractor industries.	Civil Division, Dept. of Justice	0.0481	0.2139
PostSOX	An indicator for the time period is post-SOX.	Legislation date	0.3192	0.4662
Panel B: Covariates for Detecting Fraud				
Log Assets	Log of total book assets	Compustat	8.090	1.264
Firm Market Share	The firm market share using sales and 2-digit SIC industry definition.	Compustat	11.59	20.89
Industry HHI	The level of industry concentration, based on the sum of the squared market shares using sales (HHI) for publicly traded firms, using 2-digit SIC industries	Compustat	0.053	0.059
R&D/Assets	R&D expenditures / total assets	Compustat	0.012	0.032
Leverage	Long term Debt/ total assets	Compustat	0.233	0.202
ROA	Operational income after depreciation/Total Assets	Compustat	0.068	0.075
Abnormal ROA	Residual from regression of ROA on industry mean ROA	Compustat	0.731	5.959
Stock Return	Return on firm stock	CRSP	0.202	0.695
Abnormal Stock Return	Residual from CAPM regression	CRSP	0.008	0.130
VIX	Implied volatility of S&P 500 index options	CRSP	21.80	4.568
Innovation to VIX	1-year Percentage Change in VIX	CRSP	-0.043	3.898
Variables to Predict Staring Fraud, but not Catching Fraud, in the Bivariate Probit				
Lop Options Held	The sum of the in-the-money exercisable options for all executives.	Execucomp	7.603	3.487
Incentive Pay %	The average of the ratio of restricted stock grants divided by total compensation across executives for a firm-year.	Execucomp	0.381	0.236
Sentiment	Annual data on sentiment from Baker-Wurgler (2006). Sentiment index is first principal component of six standardized sentiment proxies (value-weighted dividend premium, IPO volume, First day returns, closed-end fund discount, equity share in new issues, log of NYSE turnover)	Jeffrey Wurgler website	0.256	0.675

Appendix Table 3 (continued)

In Panel C the dependent variable in the start equation (column 1) is an indicator for whether the firm first engages in the fraud that is later caught. The dependent variable in the caught equation (column 2) is an indicator variable whether a firm is detected for fraud in that year. Both the coefficient and marginal effects are reported. For the detection equation the variables include the full set of variables in the Table 6 probit: firm specific variables (log of assets, accounting performance (ROA), abnormal accounting performance, stock market performance (stock return); abnormal stock performance, R&D intensity (R&D/assets) and leverage); competition variables (firm market share and the Herfindahl index at the SIC2 industry level); overall market conditions variables (VIX and the innovation to VIX); the Beneish Prob(M) measure; and, indicator variables to capture the presence or absence of fraud detectors found to have a significant impact on detection in DMZ (2010)(high analyst attention relative to industry, high media attention, high shortability (institutional ownership) relative to industry, qui tam applicable industry, regulated industry, and PostSOX). For the start equation, the variables included are the same except the table excludes the abnormal ROA and abnormal stock return (that are assumed to primarily impact detection) as well as compensation measures that potentially affect the likelihood of starting a fraud (log options value and lagged options compensation relative to total compernsation). The final row of the table presents the detection likelihood estimate based on these probit estimations. Standard errors are in brackets. ***, **, and ** denote significance at the 1%, 5%, and 10% levels.

Panel C: Detection Likelihood Estimates from Bivariate Probit Estimation

	(1)		(2)	
Bivariate Probit Model:	Equation 1:Probit(Start Fraud=1).		Equation 2: Probit(Fraud Detected=1)	
	Coefficient	Marginal Effects	Coefficient	Marginal Effects
Log Assets (lag)	0.474***	0.0593***	-0.384***	-0.0495***
	[0.172]	[0.0197]	[0.144]	[0.0176]
Firm Marketshare	-0.0332***	-0.0042***	0.0316***	0.00407***
	[0.0108]	[0.0013]	[0.0107]	[0.0013]
Industry Herfindahl Index	-6.287	-0.7870	5.667	0.7296
	[6.621]	[0.8163]	[6.746]	[0.8600]
R&D/Assets (SIC3 average)	-0.0208	-0.0026	0.9010	0.116
	[9.304]	[1.1647]	[8.992]	[1.1582]
Leverage (SIC3 average)	14.36***	1.797***	-13.70***	-1.7641***
	[3.064]	[0.3121]	[3.067]	[0.3106]
ROA (lag)	-13.68***	-1.712***	13.79***	1.7755***
	[3.601]	[0.4103]	[3.653]	[0.4205]
Abnormal ROA (lag)			-0.0029	-0.00036
			[0.0213]	[0.0027]
Stock Return (lag)	0.159	0.0199	-0.0106	-0.0014
	[0.109]	[0.0138]	[0.0858]	[0.0111]
Abnormal Stock Return (lag)			0.3500	0.0451
			[0.262]	[0.0332]
VIX	-0.0502	-0.0063	0.0568	0.0073
	[0.0769]	[0.0096]	[0.0746]	[0.0095]
Innovation to VIX			0.00061	0.00008
			[0.0102]	[0.0013]
Beneish Prob(Manipulation)	0.413*	0.0517**	-0.3750	-0.0482
	[0.239]	[0.0294]	[0.240]	[0.0300]
Log Options Held	-0.0591***	-0.0074***		
	[0.0184]	[0.0021]		
Incentive Pay % (lag)	0.351**	0.0439**		
	[0.168]	[0.0205]		
Sentiment index	0.0996*	0.0125*		
	[0.0595]	[0.0072]		
High Attention to Detection Variables				
Hi Analysts (lag)	0.0304	0.0038	0.0665	0.0086
	[0.0988]	[0.0124]	[0.107]	[0.0137]
Hi Media (lag)	3.155***	0.395***	-2.982***	-0.3839***
	[0.667]	[0.0605]	[0.665]	[0.0623]
Hi Shortability (lag)	-0.093	-0.0116	0.244**	0.0314**
	[0.0763]	[0.0094]	[0.103]	[0.0129]
Qui Tam Industry	-2.906***	-0.3638***	2.818***	0.3628***
	[0.721]	[0.0777]	[0.742]	[0.0857]
Regulated	-1.009	-0.1263	0.975	0.1255
	[1.146]	[0.1423]	[1.075]	[0.1384]
PostSOX	-1.338**	-0.1675**	1.382**	0.1779**
	[0.624]	[0.0759]	[0.610]	[0.0758]
Observations	5548			
LR/Wald (36) Chi-Square	52.14	Prob > Chi-Square	0.04	
Detection Rate Calculation: Predicted Prob(Detect | As Observed) / Predicted Prob (Detect | As Observed except All High Variables =1)				0.153