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HOW PERVASIVE IS CORPORATE FRAUD?

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ABSTRACT

We estimate what percentage of firms engage in fraud and the economic cost of fraud. Our estimates are based on detected frauds, and frauds that we infer are started but are not caught. To identify the 'iceberg' of undetected fraud we take advantage of an exogenous shock to the incentives for fraud detection: Arthur Andersen's demise, which forces companies to change auditors. By assuming that the new auditor will clean house, and examining the change in fraud detection by new auditors, we infer that the probability of a company engaging in a fraud in any given year is 14.5%. We validate the magnitude of this estimate using alternative methods. We estimate that on average corporate fraud costs investors 22 percent of enterprise value in fraud-committing firms and 3 percent of enterprise value across all firms.

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Until recently, the United States was deemed the corporate governance standard towards which other countries aspired. The major wave of corporate scandals that emerged at the beginning of the millennium deeply shook this confidence. How was it possible for companies like HealthSouth to falsify its financial statements for 11 years without notice, or WorldCom to transform 3.8 billion of expenses into capital investments, or Enron to allow managers to enrich themselves while hiding billions of liabilities? Do these examples just reflect a few rotten apples, or are they instead the tip of the proverbial iceberg?

The answer to this question is not just intrinsically interesting, but it is extremely important. If we knew the frequency and cost of frauds this would help investors and boards to tailor resources to mitigate the scope of the problem. It would also provide a fact base for reforms, such as the legislative reforms in Sarbanes Oxley and Dodd-Frank. If there are just a few rotten apples, large-scale intervention might be a waste of energy and resources. As the old saying goes, "If it ain't broke, don't fix it". But if these examples are the tip of the iceberg, then further interventions to fix the problems might be warranted.

Frauds we are interested in have a few key characteristics. As is clear in the examples above, frauds involve misrepresentations, concealment, or nondisclosure. They also need to be important rather than minor. This is obviously more difficult to define. It is captured in part by the accountants' term 'materiality', or that "knowledge of the matter would be likely to influence the user of the financial or other statements under consideration." They could involve lying about the past, with financial misrepresentations, or lying about the future by concealing or not disclosing the status of projects designed to deliver future growth. Less important for us is whether those who are involved have an intent to deceive, a criteria at the heart of most legal cases.

Prior research provides some indicators that could be used to size up the pervasiveness of fraud, but this research has at least two important limitations. The first problem is that the major databases that are used to explore fraud have features leading to meaningful biases. Karpoff, Koester, Lee and Martin (2012) (hereafter KKLM) focus on this issue, identifying the four datasources that researchers have relied upon, the major papers that use each of the databases, and the limitations of each database. At the risk of oversimplification, two central limitations are over-restrictiveness, or over-inclusion.

Focusing only on firms where there is a Securities and Exchange Commission Accounting and Auditing Release (AAER), for example, is likely to lead to a sample that is restricted and excludes some cases that we see as frauds. [1] AAER firms are serious cases brought by the SEC after investigation. Befitting their seriousness, AAER firms are more likely than firms in all other databases to be associated with legal charges of financial fraud. But this comes at the cost of omitting other cases that likely are material. The SEC after all has a limited budget, and they don't have incentives to go after all frauds, rather those that are visible and less costly to detect. Moreover, by definition enforcement actions involve accountants,[2] and focus on financial misrepresentations. Non-disclosure of the status of projects delivering future growth, for example, has clear value consequences, but would not be captured by AAERs.

Alternatively, focusing on firms where there has been a financial restatement, using either the sample produced by the General Accounting Office, or the sample from Audit Analytics, is likely over inclusive of cases of financial misrepresentation in including many cases that are unlikely to be material. Hennes et al. (2008), for example categorize 73.6% of GAO restatements as unintentional misapplications of GAAP accounting.

A second limitation with existing studies for our purposes is that they do not focus on the iceberg of undetected fraud. Sizing up the full extent of the fraud problem in US corporations requires an effort to identify frauds being committed in corporations that remain undetected. It is likely that researchers have avoided this, in part as a result of a need for strong identifying assumptions to go from observed data to infer the extent of unobserved frauds. A notable exception to this is Wang (2011) who tackles this problem but does not produce an estimate of the scope of detected and undetected fraud.

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[1] Examples that have focused on Accounting, Auditing and Enforcement Releases, include Dechow, Sloan and Sweeney (1996), Miller (2006), and

[2] See AAER-1, 1982 SEC LEXIS 2565, May 17, 1982 and discussion in KKLM.

In this paper we provide an answer to the question of the pervasiveness of fraud. We use the dataset of frauds from Dyck, Morse and Zingales (2010), (hereafter DMZ) who developed a comprehensive sample of frauds from Security Class Action cases. Their frauds include those involving financial misrepresentations, but importantly are not limited to those. The Securities Class Action data, when combined with DMZ extra steps, performs well according to the criteria of KKLM. This sample relies on the fact that the security class action system provides strong incentives (for attorneys and shareholders) to file suit whenever a fraud that is likely to have a material impact is revealed. As a result the sample is unlikely to suffer from problems of over restrictiveness. For large companies, it is highly unlikely that detected frauds exist without a corresponding class action suit. Of more concern is that the sample will be over inclusive. DMZ apply rigorous filters to eliminate likely frivolous suits. [3]

With this dataset we tackle the question of unobservable fraud. We appeal to basic probability rules for guidance of going from observed data of the joint event of engaging in fraud and being caught, to our actual variable of interest, the probability of engaging in fraud regardless of whether they are caught or not. The idea is really quite simple. What is observed is the probability that managers engage in a fraud and that they get caught, Pr(*engage*,*caught*). The unconditional probability of engaging in a fraud which is what we are ultimately interested in is Pr(*engage*). This is the product of the detection likelihood Pr(*caught*|*engage*) and the observed probability of engaging and getting caught. Thus, if we knew the detection likelihood we could easily calculate the probability of engaging in fraud. Our identification strategy exploits circumstances in which the likelihood of being caught increases to close to one. By comparing the differences in detection in this special circumstance to normal circumstances, we produce an estimate of the iceberg (i.e., the normal detection likelihood). From there, it is a short step to estimate the unconditional pervasiveness of engaging in fraud.

[3] As with all databases, the SCAC also has limitations that KKLM discuss. These include the fact that the SCAC database omits 9.4% of cases that prompted SEC enforcement and had security class action filings. This makes the results we arrive at more conservative. The SCAC omission rate is the lowest across the four databases.

Our primary test takes advantage of the natural experiment created by the demise of Arthur Andersen that forced all firms that previously had Andersen as their auditor to seek another auditor. This forced auditor turnover enhances the incentives of new auditors to be active. When we restrict our attention to firms that had Arthur Andersen as an auditor and were forced to change auditors, we find that the incidence of fraud detection by auditors goes up by a multiple of close to four. This gives a sense of how much undetected fraud exists more generally, with the iceberg being 3 times bigger under the water than above the water. Taking this estimate, and applying it with some additional assumptions, we arrive at our best estimate that 14.5% of large publicly traded corporations engage in fraud. We also use this experiment to produce a very conservative lower bound estimate, which we find to be 5.6%.

To validate these results, we introduce two additional tests and compare our results with others in the literature. The first of these validation tests applies a similar thought experiment across a range of fraud detectors. For example, conditional on a fraud being committed, we conjecture that the probability a fraud is revealed is a positive function of the number of analysts following a company. This conjecture is supported by the data. Hence, in companies that have more analysts following them, if a fraud is perpetrated, it is more likely to emerge. There are a number of such variations in our data (time series and cross sectional) that heighten incentives for fraud detection. Looking at all of these, and using a partial observability probit model that allows us to simultaneously account for the fact that the presence of fraud detectors might also influence the likelihood of starting a fraud, we produce another estimate of the unconditional probability of engaging in fraud. These results lie within the range suggested by the Arthur Andersen experiment. Our third test employs a different approach, looking at survey results on whether corporate actors were asked to engage in illegal activity, in a setting where there is little incentive not to reveal the truth, producing an estimate of 14.8 percent.

In a final section of the paper we take the results one step further and offer an estimate of the social costs of fraud. Even in circumstances where there is no prior information leakage, the change in market value on the day the fraud comes to public attention is not a comprehensive measure of the social costs of fraud. Frauds are often committed to cover up negative news, which would have been revealed to the market earlier in absence of fraud. In other words, the stock price, and thus the stock drop, at the time of revelation may be too large relative to fundamentals. The amount of damages alleged in legal suits is also not a good measure of such costs, because many of the dollar losses are transfers rather than social losses.

We construct a new measure of the cost of fraud, which we define to be equal to the difference between the enterprise value after the fraud is revealed and what the enterprise value of the company would have been in the absence of fraud. We construct this hypothetical value by making projections from the pre-fraud period, assuming the trajectory would have followed that of other firms in the same industry. Using this approach, we estimate that the median loss is 21.7 percent of the enterprise value of our fraud companies, using firms' enterprise value prior to the beginning of fraud as the benchmark.

We also take advantage of a conceptually similar approach to measure the cost of fraud by Karpoff, Lee and Martin (2008) that is well suited to situations with financial frauds, which leads to an estimate of the social costs of fraud of 21.8% of enterprise value.

Putting the estimate of the extent of fraud with this estimate of the cost per firm of fraud, we produce an estimate of the social cost of fraud for these firms as a percentage of their enterprise value. This price tag is 2.5 to 3.1% of enterprise value of all large corporations.

The rest of the paper proceeds as follows. Section I describes the data and provides a baseline conservative estimate of fraud pervasiveness based on frauds that are caught. Section II describes our main identification methodology and information about the relevance of the Arthur Andersen demise for our estimation procedure. Section III provides results on fraud pervasiveness from the Arthur Andersen experiment. Section IV introduces and describes two tests that validate these estimates, based on a larger sample of fraud detectors and survey evidence, as well as related literature. Section V provides costs estimates and we conclude in section VI.

I. Data on Caught Fraud Incidence

To establish a baseline of the pervasiveness of corporate fraud in U.S. publicly-traded firms, we start with the DMZ sample of caught frauds. DMZ identify frauds as firms subject to securities class action lawsuits, as compiled in the from the Stanford Securities Class Action Clearinghouse (SCAC). DMZ argue that this sample is close to the population of caught fraud for large (over $750 million in assets) publicly-traded companies because of the incentive structure for law firms. Class action law firms have automated the mechanism of filing class action suits such that specialist attorneys start searching for a cause to file a suit every time a large negative shock to share prices occurs in large corporation. Since stock prices drop following revelation of most serious corporate frauds, it is highly unlikely that a corporate fraud of any magnitude would emerge without a subsequent class action suit being filed (Coffee, 1986).

The biggest potential problem with using class action data is not that it misses important frauds, but rather that it might be over-inclusive in including frivolous allegations. DMZ use a filtering process, summarized in the appendix of this paper, to remove this concern. The gist of the screening is to restrict attention to data after 1996, after a law change made the courts became more stringent about evidence for certification. They then limit the data to cases that are not dismissed, presuming this filters out some frivolous cases. And they limit non-dismissed cases by excluding cases with low settlements, based on guidance from the legal literature as to what settlement amounts constitute nominal payments to make the suit go away.

It is worth noting that while we use the term frauds they are better thought of as 'alleged frauds'. Security class action cases are almost always settled (to protect executives from personal liability), and settlements almost always involve no admittance of wrongdoing. For simplicity, in the rest of the paper we nonetheless use the term fraud, and do not append the adjective "alleged".

In total, the sample includes 212 frauds detected in the 1996-2004 period.[4] These frauds include all of the high profile frauds such as Enron, Worldcom, Adelphia and Healthsouth, as well as many others. The class action database provides start and end dates for the frauds.[5] The frauds in the sample have an average duration (from the class action suit period) of approximately 1 year and 7 months (590 days). To gauge the pervasiveness of fraud, we also have to identify the possible population of firms that could have produced frauds. The relevant population for our purposes is, like our fraud sample, the set of U.S. publicly-traded companies with $750 million in assets. In Compustat, 2,976 companies on average per year meet this criterion. Appendix Table 1 reports the characteristics of these samples.

With this information on the start dates of frauds that are caught, the information on their duration, and on the underlying population of firms, we can calculate the percentage of firms that are engaging in frauds that are caught for every year in our sample period. Figure 1 illustrates the incidence of caught frauds. We plot the percentage of large U.S. publicly traded companies that start fraud in each year (the grey bars) and the percentage of firms engaging in fraud (the black bars). This evidence suggests a non-trivial level of fraud taking place, with an average of 1.3 percent of firms starting fraud each year and 3.3 percent of firms being engaged in fraud. We think it is particularly important to use an average of fraud over a time period that involves booms and busts as this period does, as there appear to be time patterns in fraud activity.[6] Note the significant time series variation in these numbers, with the incidence of firms starting fraud peaking in 2000 (2.4 percent of large corporations), and the fraction of firms engaging in fraud peaking in 2001 (5.9 percent of large corporations).

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[4] We drop 4 frauds from the DMZ sample because they are not over the $750 million threshold at the beginning of the fraud.

[5] Because these dates can be, and often are, revised as suits progress, we use the most recent definition of the suit window from the legal filings. This definition of duration may be conservative in that the statute of limitations on class actions under Section 10(b) of the Exchange Act dictates that cases must be brought within one year after discovery of the alleged violation, and no more than three years after the violation occurred. This limit was loosened in 2002 as Sarbanes-Oxley legislation changed this to 2 years after discovery, and no more than 5 years after the violation occurred.

[6] Galbraith () was one of the first to conjecture such a relationship. This idea has been explored more recently in theoretical models in Povel, Singh and Winton (2007) and an empirical application in Wang, Winton and Yu (2010).

Rather than solely using this data, we make modifications to reflect a clear bias. Figure 2 introduces a correction for the fact that there are some additional frauds that will be caught after we ended our sample collection in 2004, that were taking place during our sample period. To extrapolate these missing frauds we use the distribution of fraud duration for those cases which begin prior to the year 2000 to forecast how many cases are yet to be caught for frauds starting through 2003. Using the duration distribution, we then roll the distribution forward to forecast how many additional cases that began after 1999 will yet be caught. This correction raises our estimate of the overall incidence of firms starting fraud to 1.4 percent per year and the overall fraction of firms engaging in fraud in any year based on the 96-2004 period to 4.0 percent of firms. Again, we focus on the average over a period with cycles and boom as the data show significant time series variation with a much higher incidence of frauds starting prior to the demise of Enron and Arthur Andersen in 2001 and the passage of SOX in 2002.

II. Methodology & Statistics: Arthur Andersen Natural Experiment

The figures provide an incomplete picture, as they ignore the fact that some frauds are never caught. Without exploring the likelihood of fraud taking place without being caught, we do not know if these observed estimates are the whole iceberg, or just the tip of the iceberg.

II.1. Experiment Overview

Our identification strategy for inferring unobserved fraud relies on a basic probability rule. What we observe is the joint event of firm engaging in fraud and being caught, **Pr(*engage, 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(*engage*). By the law of conditional probability, the unconditional probability of engaging in a fraud can be written as:

$$\Pr(engage) = \frac{\mathbf{Pr(\boldsymbol{engage, caught})}}{\Pr(caught \mid engage)} \tag{0}$$

Thus, if we knew the detection likelihood, the probability that a fraud is caught given that it is ongoing, Pr(*caught*|*engage*), we could easily calculate the Pr(*engage*). In the circumstance where

Pr(*caught*|*engage*) is equal to one, then the unobserved Pr(*engage*) would simply be equal to the observed **Pr(*engage, caught*).**

Our strategy identifies and exploits circumstances in which the likelihood of being caught by a particular fraud detector increases to close to one. In fact we assume the Pr(*caught*|*engage*) is one, providing a conservative bias to our estimates. We compare the caught fraud rate in this 'full detection' circumstance to the caught fraud rate in the normal circumstance to estimate the detection rate of fraud in normal times. In particular, our main experiment uses the sudden demise of the auditor Arthur Andersen (AA) as a situation in which the likelihood of being caught for financial fraud approaches one.

II.2. Arthur Andersen Clients

Although the experiment conditions are fairly intuitive, we want to be precise as to the assumptions we need. We begin with a baseline assumption about AA clients.

$$Assumption\,1: \frac{\Pr(engage^{*})}{\Pr(engage^{*}|AA)} = \boldsymbol{1} \tag{1}$$

Assumption 1 says that financial fraud was equally likely in AA and non-AA firms prior to 2001. We distinguish financial frauds detectable by an auditor from all frauds by using an asterisk superscript. 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 coming into the demise of 2001-2002, and detection is thereafter.

Despite conditioning from the press and perhaps out human nature inclination to dis-believe Assumption 1, evidence from prior authors finds no evidence that AA was any less stringent of and auditor than other auditors. In a matched sample, Agrawal & Chada (2005) find that the existence of Arthur Andersen as the auditor does not associate with firms having more restatements. Likewise, controlling for client size, region, time and industry, Eisenberg & Macey (2004) find that Arthur Andersen clients did not perform any better or worse than other firms.

Our sample of only large U.S. corporations is different from the aforementioned studies, and we wanted to verify this assertion. We construct an additional test of whether pre-indictment (1998-2000)

Arthur Andersen clients differ from non-AA clients using the earnings manipulation score "ProbM-Score" of Beneish (1999) and Beneish and Nichols (2007). The idea in these papers is that specific indices made from financial statements can be indicative of fraud taking place or conditions for fraud to take place. The components in the ProbM Score are: days sales in receivables, gross margin, asset quality index, sales growth index, depreciation index, SGA index, leverage, and the ratio of accruals to assets. Beneish motivates how each of these subindices captures an aspect of manipulation, and, thus, we refer the interested reader to Beneish (1999) for a description of each variable. To construct the ProbM Score, we download the appropriate financial statement variables from Compustat, construct all the components directly following the data definitions in Beneish (1999), and use Beneish's estimated coefficients to construct the ProbM Score. Appendix II details the equation calculation.

We report the result of this analysis in Table 1. The table first provides univariate differences between AA clients and all non-AA clients that meet our size criteria or between AA clients and all non-AA clients that use one of the other Big Five audit firms, a perhaps more appropriate reference group. Panel A shows no significant differences between AA and non-AA clients across all sub-components of the prob-M score and for the prob-M score variable itself.

Panel B introduces the possibility that AA and non-AA clients differ on other dimensions. Indeed, AA firms are (trivially) smaller, have more debt, higher sales to assets and higher profitability measured using EBITDA to sales ratio. Thus, we implement multivariate tests to control for covariates. Panel C reports regressions of the prob-M score on an indicator variable indicating AA and a series of control variables. Across all specifications, being an AA client has no significant impact on the prob M score. In column 1, we use an OLS specification and include all firms. In column 2, we restrict our attention to the sample with a top-5 auditor. Columns 3 and 4 repeat the analysis using a median specification. Finally, the last row of Panel A reports that AA clients are no more likely to engage in fraud which is caught prior to 2001 compared to other auditors. We conclude that, as was found in prior studies, Arthur Andersen clients are not statistically or economically different from other auditor's clients prior to 2002.

II.3. Catching Assumptions

Our main identifying assumption is that post-AA, the probability of detecting an ongoing financial misreporting fraud increases to one for former AA clients.

$$Assumption\ \mathbf{2}:\quad \Pr\left(caught \mid engage^{*}, AA\right) = \mathbf{1} \tag{2}$$

The time frame covers frauds starting prior to or during 2002, and catching done in 2002 or later.

The intuition of the assumption is as follows. In the fall of 2001, Enron's collapse triggered accusations about Arthur Andersen. AA was indicted in March 2002, and convicted in June, 2002. Over the period 2001-2002, all of Arthur Andersen's clients had to change their external auditor. Because new auditors do not want to face litigation risk or reputation risk for actions (or non-actions) taken by prior auditors, new auditors have a strong incentive to "clean house". Cleaning house implies that new auditors address any potentially misleading financial reporting, ranging from gross errors to overly aggressive financial reporting. An important advantage of this is that the line of causality is clear: the turnover leads to the fraud revelation, rather than the fraud leading to the turnover. A significant literature concerning litigation risk finds evidence that more conservative accounting reporting emerged in former AA clients (see Cahan and Zhang (2006), Krishnan (2007)).

A couple of points are worth noting. First, Assumption 2 is conservative. Auditors may not be privy to information as to the impropriety, thus limiting their capacity to find all fraud. If they do not, we will underestimate the total number of frauds.

Second, strictly speaking this experiment speaks to the detection probability only for financial frauds, which auditors have the position and incentive to detect and reveal. The other main type of securities fraud involves management's failure to reveal information about the future that is pertinent to shareholder valuation, but is not reflected in current financial statements. A typical example might be that management does not reveal that their R&D is failing to advance a technology or biological development.

Since our goal is to speak generally to all securities fraud, we take two approaches to generalizing from financial securities fraud to all securities fraud. First, we can set up what we have thus far by putting assumptions 1 and 2 together and write down a ratio equation of the conditional probability equation for AA firms and all firms:

$$\frac{\Pr\left(caught \mid engage^{*}\right)}{\Pr\left(caught \mid engage^{*}, AA\right)} \frac{\Pr\left(engage^{*}\right)}{\Pr\left(engage^{*} \mid AA\right)} = \frac{\boldsymbol{Pr\left(engage^{*}, caught\right)}}{\boldsymbol{Pr\left(engage^{*}, caught \mid AA\right)}}. \qquad (1)$$

Substituting in (1) and (2), equation (4) below is an estimator for the probability of detection, given that a firm has engaged in financial misreporting fraud:

$$\hat{\Pr}\left(caught \mid engage^{*}\right) = \frac{\boldsymbol{Pr\left(engage^{*}, caught\right)}}{\boldsymbol{Pr\left(engage^{*}, caught \mid AA\right)}}. \qquad (4)$$

To go from financial fraud to all securities fraud, we first take a lower bound approach, which makes no further assertions, but rather just assumes that all failure to disclose, future-type frauds are caught. In this case, denoting future-oriented fraud with superscript *future*:

$$\begin{aligned}&\text{Lower Bound}: \hat{\Pr}\left(caught \mid engage\right) = \\ &\quad \boldsymbol{Pr\left(engage^{future}, caught\right)} + \boldsymbol{Pr\left(engage^{*}, caught\right)} \hat{\Pr}\left(caught \mid engage^{*}\right).\end{aligned} \qquad (5)$$

To the extent that some future-type frauds are icebergs under the water, this estimate will be too large, and thus conservative.

Our best estimate instead makes one final assertion, that financial frauds are no more hidden than other types of fraud:

$$\Pr\left(caught \mid engage^{future}\right) = \Pr\left(caught \mid engage^{*}\right) \qquad (6)$$

And thus,

$$\text{Best Estimate}: \quad \hat{\Pr}\left(caught \mid engage\right) = \hat{\Pr}\left(caught \mid engage^{*}\right). \qquad (7)$$

We have no data to support or refute this assumption. We feel that it is likely a conservative assumption, in that the detection likelihood if anything may be higher for financial frauds. Insiders need to produce financial statements, and likely misrepresentations can be identified by comparing this with past history and competitor information, as is common in the accounting literature. It is more challenging for fraud detectors in cases of concealment or non-disclosure as ‘they don’t know what they don’t know,’ and such types of frauds are more likely to lie at the heart of frauds involving lying about the future.

III. Results: Arthur Andersen Natural Experiment

The inputs into our main experiment calculations are as follows. We take all large corporations who existed in 2002 and had an auditor identified in Compustat in either 2001 or 2002. We code the firm as an AA client if the auditor was AA in either 2002 or 2001. We put no restriction on survival post 2002. To capture the staggering of the change in auditors and to allow the new auditors time to process all of the new client accounts, we measure a firm as having fraud if the fraud started prior to and including 2002, and the fraud was revealed in 2002 or later. We categorize a fraud as being a financial fraud if it restated financials after the revelation of the fraud.

We provide calculations in Table 2. Firms with Arthur Andersen as their auditor in 2001 or 2002 had a 1.379 percent chance of having an auditor reveal existing fraud after the Arthur Andersen demise. This is compared to all firms in this same time period, which only had a 0.378 percent chance of having an auditor reveal fraud. (Note that in this period, auditors reveal 11 percent of the fraud cases in DMZ.) Comparing these two numbers, the detection likelihood for financial frauds is just 0.275. For every 1 fraud that is caught, another 3 lie hidden under the water. Under the assumption that financial frauds are no more hidden than other frauds, 0.275 is our Best Estimate for fraud detection probability, generally across all securities fraud.

This estimate is conservative to the extent that the probability of revelation conditional on having been an AA client is less than one. Research suggests this is likely. The incentive to critically review past audit decisions and ‘clean house’ is dulled by the fact that the same individuals continued to audit former AA firms as other auditing firms hired AA former auditors and they brought their clients with them (Blouin, Grein and Rountree (2005)).

The step to translate this number into the overall probability of a firm committing fraud is straightforward, with one caveat. Figure 2 shows that across the business cycle represented in the years

1996-2004, 4.0 percent of fraud are engaged in fraud each year that will eventually be caught. To the extent that this time period is representative, we can combine 4.0 percent of firms engaging in to-be-caught frauds with the 27.5 percent detection probability to conclude that 14.5 percent of large U.S. corporations are engaging in fraud at any point.

Our best estimate relied on assumption on the hiddenness of future-type fraud being the same as financial fraud. If we instead assume that all future-type frauds are caught as in equation (5), we can provide a very conservative lower bound estimate. In the DMZ data, 35.3 percent of the engaging-in-fraud observation years are for firms that eventually will restate financials, our measure of a fraud being financial. Combining these numbers, Table 2 shows that the Lower Bound estimate for the detection probability is 0.531 (= 0.353 *1 + 0.647 * 0.275). Our lower bound estimate suggests that just over half of frauds are caught, and that 7.5 percent of large U.S. corporations are engaging in fraud at any point.

IV. Validation Methods to Identify Frauds that are not Detected

The Arthur Andersen experiment provides a natural experiment setting to identify hidden auditor-detectible frauds because it provides a clear circumstance in which the likelihood of being caught increases to close to one, and the experiment involves a significant number of firms across a variety of industries. To explore the validity of our findings based on the demise of AA, this section introduces two additional approaches to estimate fraud and compare the results with those from the Arthur Andersen experiment.

IV.1. Validation by Incentives and Opportunities Estimation

Auditors are important fraud detectors, but focus on financial frauds and only account for eleven percent of detections in DMZ. A village of detectors contributes to fraud revelation, led by analysts, employees, media, short sellers, and non-financial regulators. This evidence and related literature suggests at least 6 ‘experiments’ we can exploit that while weaker than the AA natural experiment, as each offer an example of heightened incentives from which we might be able to estimate what percentage more frauds would be detected if incentives were high.

For the first two 'experiments', we can take advantage of cross sectional differences in both media and analyst coverage. If media or analyst coverage of a firm is high, fraud detection is more likely as journalists and analysts are more likely to understand the firm dynamics and patterns in other firms. Yu (2008) finds that firms with more analyst coverage engage in less earnings management. Dyck, Volchkova and Zingales (2008) document the impact of media coverage on uncovering governance abuses. Third, we can take advantage of cross sectional differences in incentives to detect fraud by shortsellers. Incentives depend on the ease to assemble short positions, using institutional ownership as a proxy for shortability this suggests more detection in firms with more institutional ownership. Fourth, employees may reveal fraud more if a monetary payoff results, producing a cross sectional prediction as such heightened incentives only exist in certain industries. Similarly, DMZ find industry regulators are fraud detectors, again providing a cross sectional prediction. Sixth, the passage of SOX lead to greater attention and oversight presumably raising detection for all detectors post passage.

The idea behind our strategy in this section is thus the same as in the Arthur Andersen experiment. We look for situations to assert that the probability of detection of an ongoing fraud approaches one. We do not have such a natural experiment, so we try to make one synthetically by seeing how the probability of detection increases when firms potentially committing fraud score high on all variables capturing the heightened incentive or opportunity for detection.

In our empirical approach we in essence then, seek to estimate a probit as a function of a set of indicators for high incentive and opportunities variables. With this probit, we can then calculate an inference as to what a true natural experiment would reveal by adding up the estimate impact on the probability of detection by setting all of those indicators to unity. In performing this analysis we recognize we need to take extra steps not required in the AA experiment where the type of auditor (AA or not) made no difference in the likelihood of manipulating statements in the first place. We cannot rely on that assumption here. It is highly probable that the presence of more analysts, for example, will affect the likelihood of engaging in fraud in the first place, in fact reducing the likelihood of engaging in fraud. This is also true for the other fraud detectors. Thus, to accurately measure the impact of the number of analysts on detection we need to simultaneously account for its likely impact on engaging in fraud in the first place.

Wang (2010) earlier recognized this point and offered a solution, suggesting the possibility of employing the Poirier (1980) bivariate probit model for estimating dichotomous outcomes in partial observability settings to the case of corporate fraud. Our empirical methodology follows directly from Wang (2010).

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:

$$\begin{aligned} D_{it} &= X_{it}^{D}\Gamma_{D} + \nu_{it} \\ caught_{it} &= 1 \; if \; D_{it} > 0. \end{aligned} \tag{2}$$

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. Also in X_{it}^{D} are general characteristic $\left[\mathrm{x}_{it}^{D}\right]$ that would lead to detection in all firms (e.g., size and stock performance). We denote these two groups: $X_{it}^{D}{}' = \left[I_{it}^{HiIncentOpp} \quad \mathrm{x}_{it}^{D}\right]$.

We also define an engage equation, defining E_{it} to be the incentive for firm *i* to engage in fraud at time *t*. Fraud is committed if E_{it} is positive:

$$\begin{aligned} E_{it} &= X_{it}^{E}\Gamma_{E} + \mu_{it} \\ engage_{it} &= 1 \; if \; E_{it} > 0. \end{aligned} \tag{3}$$

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

Identification in Poirier's model comes from two pieces. First, Poirier assumes that (μ_{it}, ν_{it}) are distributed bivariate standard normal. Second, identification depends on our 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:

$$Pr\left(engage_{it}, caught_{it} \middle| X_{it}^{E}, X_{it}^{D}\right) = \Phi\left(X_{it}^{E}\Gamma_E,\ X_{it}^{D}\Gamma_D\right), \tag{4}$$

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

Table 3 lists all of the variables we use in this estimation. Panel A lists factors we hypothesize affect both the likelihood of engagement and detection, Panel B lists those variables we hypothesize affect primarily the likelihood of engaging in fraud, Panel C lists variables that primarily affect the likelihood of detection.

As noted above, a great number of these variables we assume can both affect detection and likely in the other direction the probability of starting fraud. These variables include indicator variables for high media and analyst coverage, an indicator variable for ease of shorting the stock, a dummy for regulated firms and qui-tam industries and finally a post-sox indicator. Recognizing how they affect detection, those committing fraud might very well be deterred from starting frauds in the first place predicting the opposite sign in the engagement equation. Each of these, entered alone we expect would load positively in a detection equation[7] and possibly negatively in a starting equation, but in multivariable regressions the predictions are less clear depending on the strength of different effects.

To these core variables we also add a number or additional control variables that capture important cross sectional variation across firms and could influence detection and engagement. These include company size, the stock return, ROA, leverage, R&D, and the VIX. For example, a disappointing stock

[7] In a previous version of the paper we conduct this analysis, and find as predicted that detection is higher across these splits.

return likely encourages engaging in fraud and likely leads to more detection behavior. We follow Wang (2006) and include R&D as a measure of the opacity of firm fundamentals, under the assumption that it is easier to engage in fraud if financials are more opaque and it is harder to detect under the same circumstances.

Panel B lists variables that we hypothesize primarily influence fraud by influencing the likelihood of engaging in frauds. We focus here on the monetary incentives for top management, in particular the role of options and more generally compensation based on future rather than current returns. We first produce a measure of the stock of pay in options by calculating all of the options held by top executives. We next 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. We hypothesize that managers may want to take on more risk with nonlinear payoffs of compensation being performance based. At the same time it is possible that managers may be less willing to commite fraud if they have more exercisable options exposed to the consequences of fraud.

Finally, Panel C lists variables that enter only into the ex post detectability of the fraud already being engaged. The helpful insight here is that fraud has a time dimension, and unanticipated information and events may happen after fraud has already started that change the setting for detection. We focus first on abnormal performance measured by abnormal stock returns using a CAPM model for returns, and then abnormal ROA. Detectors may pay more attention to a firm if the firm is performing abnormally relative to the industry and market, measured by abnormal leverage. We also think there is a macro impact to detection provided by volatility, using abnormal VIX to capture this.

IV.1.2. Incentives & Opportunities Estimate: Results

Table 4 presents estimates, with our focus on the bivariate probit in model 5 and the estimate coefficients in column 6. Column 1 and 2 show the probit for starting with column 2 including the Hi incentives and opportunity indicator variables. We find evidence that recognition of higher detection incentives has some effect on starting, with a negative coefficient on Hi Media, a negative coefficient on Regulated, and a negative and significant coefficient on post Sox. Qui Tam industry in this specification surprisingly loads positively, although this reverses when we get to the bivariate probit. Amongst the control variables, performance measured by ROA and Stock returns predicts starting fraud, consistent with a situation of good times providing expected cover for such activities. Similarly, VIX increases the likelihood of engaging in fraud. More important are the variables identified in italics that are in the start equation, as they are hypothesized to effect starting but not detection. Here we find a significant positive impact of incentive pay on the likelihood of starting a fraud, as predicted, and an insignificant (and negative) effect on log options held.

In models 3 and 4 we turn to detection probit. We find that four of our Hi indicator variables, including HI analysts, HI shorability, Qui Tam industry and Post SOX produce a positive and significant impact on detection, as anticipated, with Regulated and Media producing insignificant impacts once in this multivariable setting. Amongst the control variables, as expected increases in leverage and deteriorations in ROA significant increase detection, and Stock returns also work in the same direction although insignificant. VIX is seen to also increase detection.. For the abnormal performance variables the signs are as expected, with abnormal leverage increasing detection, and abnormal performance measured either as ROA or stock returns reducing detection, but all of these variables are insignificant.

Most importantly, we are able to account for the possible different effects on start and detection in model 5 using the bivariate probit. Here the results are easiest to interpret. 5 of 6 of the HI incentives variables produce positive coefficients, 4 of which are significant, in the detection equation. At the same time, we find 5 of 6 produce negative effects on starting, 4 of which are significant. One interpretation of these results is that simultaneously examining starting and detection improves our precision of the role of these factors in starting frauds. Only the regulated dummy produces opposite results to predictions, albeit insignificant.

The true value to this exercise is that it allows us to produce an estimate as in the AA experiment of the relative size of the iceberg below and above the water surface. We can infer this from the marginal effects in column 6 which show what would be the effect if we were able to turn on all 6 of the HI incentives we identified. Summing the marginal effects, we find a predicted marginal increase in detection likelihood of 3.49 %. With the average duration of fraud in our sample of 1 year and 8 months this implies the unconditional probability of starting a fraud is

IV.2. Validation by Survey

A potential concern with the validation method in the last section is that it relies on the same sample of fraud detections as our main Arthur Andersen natural experiment data. Thus, as a second validation, we conducted a survey with University of Chicago MBAs to assess the frequency of illegal behavior in corporate America. In particular, all first year campus Chicago MBAs are required to attend a program called LEAD, which tries to develop soft skills. In the academic year 2004-2005 LEAD program, we inserted an anonymous survey on illegal and unethical behavior students encountered in their previous jobs. The question asked: "*In your job you are asked to do something that is illegal. Example: Your boss asks you to lie in reporting sales*." We then asked them to provide a short description of the illegal act they were asked to do. We also asked in what industry they were working in and what function they were performing at the time.

This method has its own pluses and minuses. On the plus side, this method is the least likely to be affected by the uncaught fraud selection bias. Given that the students have left their previous employers and operate in an academic environment under guarantee of anonymity, it is unlikely that they will omit reporting any fraud they encountered. On the negative side, we might omit major frauds that are concentrated in the headquarters. Given the low level position most MBAs covered before they joined the program, they are unlikely to be privy of major fraud consummated in the corporate headquarters. In addition, it is likely that these frauds are not material, although if they are similarly widespread across the organizations in which they were employed they might end up being so for the organization.

With these caveats in mind, Table 6 Panel A reports the percentage of MBAs who responded they faced a legal dilemma. On average 14.8 percent of the students were asked to do something illegal in their previous employment, almost identical to our main Arthur Andersen estimate. The actions they were requested to perform vary from falsifying sales numbers to reclassifying a job as redundant to get rid of an employee with very high health-related expenses. In all the cases, however, they appear as truly illegal activities, hence there is no sign of misclassification there.

Surprisingly, the incidence of illegal activities does not seem to differ across industries. The only exception is consumer goods, where the incidence is only 7 percent, less than half the sample average. One possible explanation is that manufacturers of consumer products are more sensitive to their public image, because this has a larger impact on sales. This conjecture is supported by the fact that also the incidence of unethical requests is lower than average (27 percent vs. 37 percent) in the consumer industry. Contrary to expectations, the financial service industry does not experience a higher incidence of illegal activity. The same pattern is present if we divide the incidence by function performed by the student in his/her previous employment. Contrary to expectations, investment bankers are not more likely to be asked to undertake something illegal nor are accountants. Illegal activity is very homogenously diffused across the board.

Aside from intrinsic interest in these data, in summary, the survey result that 14.8 percent of MBAs experienced fraud in action suggests that our main estimate that 14.5 percent of firms are engaging in fraud at any point in time is quite valid.[8]

IV.3 Related Literature

There is a voluminous literature on corporate governance and fraud.[9] As Karpoff, Lee and Martin (2008) argue, there are almost no papers that produce estimates of the probability of detection, given that firms are engaging in fraud. Accordingly, very little of this literature speaks to the overall incidence of fraud including detected and undetected frauds.

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[8] Entering MBAs typically work a couple of years before returning to school. We ignore that we are cumulating these entering students' experiences over a few work years rather than speaking to in any given year, as the rest of the paper does. Since we doubt that new college graduates are given any responsibility for a year or two and since this test already biases against finding fraud since lower-tier employees are less likely to be knowing participants, we feel the accumulation of experiences in the previous job is not an issue of any magnitude.

[9] Much of the fraud related literature has been published in accounting journals, with important papers cited in KKLM .

Two notable exceptions are Wang (2011) and Wang, Winton and Yu (2010). Neither paper reports as a significant result the predicted probability of engaging in fraud, but this can be inferred from results reported in Wang, Winton and Yu (2010). This paper examines frauds by firms that go through IPOs. They start with 3297 IPOs from 1995-2005, use data on detected frauds (they define frauds as firms that had an AAER and/or where there was a securities class action that was not dismissed, exceeded the $2mn threshold and related to financial reporting), and then generate predicted probabilities of engaging in fraud by using a bivariate probit model. Their predicted probabilities are in line with our estimates, ranging from 10-15%.[10]

The literature on options backdating also provides another estimate of engaging in fraud. This setting has a number of strengths. In a sense this is an experiment in which detection rates go to 1 as researchers *ex post* look at the data to identify firms whose actions are most consistent with backdating. It is complementary to our tests, as our sample includes no back dating cases as they developed after we completed our data collection. It also has some limitations, as researchers' identification of likely backdaters does not mean that these situations would satisfy our definition of fraud, as they may not be material.

Bebchuk, Grinstein and Peyer (2010) look in depth at the options backdating scandal first brought to attention by Lie (2005). They attempt to uncover the percentage of publicly-traded firms from 1996-2005 in which CEOs or directors were 'lucky' directors in that they received option grants on the lowest price day of the month, filtering out those that could have taken place simply due to luck. These lucky option grants increased the value of that grant by 20% and CEO pay in that year by 10%. By their estimate 12.4% of firms have such lucky CEOs and 7% of firms have lucky directors, with the percentage of "lucky grants" of 14.5% prior to SOX and 8.4% after. Note that this reveals a big iceberg, as prior to the research of Lie (2005) none of this was revealed. Note further that the 12.4% is very close to our estimate from the AA experiment.

[10] 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. How Expensive is Corporate Fraud?

In section II we show that 4.0 percent of large publicly traded firms are eventually revealed to be engaged in fraud. The AA experiment provides a best estimate of the detection likelihood of 27.5%, leading to an estimate that in 14.5 percent of firms, or one in seven, insiders are engaging in fraud. Two validation experiments and related literature provide similar estimates. Is this level of fraud detection and pervasiveness a point of concern? To address that question, we need to go further and provide an assessment of the economic costs associated with frauds.

V.1 Prior Estimates of the Cost of Fraud

Prior research provides various measures of the cost of financial fraud. One method is to take an event study approach and carefully measure the decline in equity (and sometimes debt) capitalization at the moment of fraud revelation (e.g. Feroz, Park and Pastena (1991), Palmrose, Richardson and Scholz (2004), Grande and Lewis (2009)). This is a good measure under two conditions. First, it must be the first indication to the market of the fraud. If there was prior partial leakage, then the event alone would be an underestimate of the actual costs as Gande and Lewis (2009) show. Second, and perhaps more challenging, the measured decline must be solely attributable to the fraud, rather than to the revelation of bad information about fundamentals which the fraud tried to cover up.

A paper that addresses this second issue is Karpoff, Lee and Martin (2008). They use a sample of firms from 1978 - 2002 subject to SEC enforcement actions and put a price tag on fraud. To assess the extraordinary loss they first collect the abnormal returns from a trigger event that brings the fraud to light and add the abnormal returns from *subsequent* public disclosure of enforcement events. To capture the value loss arising from deterioration in fundamentals (they call this the readjustment effect) they attach a market value to the book value of assets written off in subsequent financial restatements. The outcome of this analysis is that they estimate that the mean (median) fraud losses not attributable to the readjustment are 29% (28%) of equity value. Assuming a 25% D/V ratio that is the median in our fraud sample, and that the value of debt is unaffected by the fraud, their study suggests fraud is associated with destruction of 22% of the firms' enterprise value.

V.2. An Alternative Method for Calculating the Cost of Detected Fraud

We take a conceptually similar approach with two modifications. Reflecting Gande and Lewis's (2009) finding of a partial anticipation effect, we allow for the possibility that some information about the fraud leaks even *before* the trigger event that defines the end of the class period for securities class purposes and disregard market responses after the trigger event. We use an industry-adjusted multiples approach to capture an estimate of the value loss to a deterioration in fundamentals, motivated in part by the fact that our cases include not only cases of financial fraud (where restatements are available) but also non-financial frauds (where there are no restatements we can appeal to).

We start by expressing the value of the fraud firm in the pre-fraud period using a performance multiple (e.g. of EV/EBITDA). We then produce an estimate of expected multiple expansion by a typical firm in the industry over the fraud period. We then apply this industry-adjusted multiple to the performance data for the firm after the fraud has been revealed and reflected in the firm financials producing an estimate of 'non fraud implied enterprise value.' The difference between this estimate and the actual enterprise value at this point in time provides an estimate of the value loss for shareholders from fraud. Appendix IV provides more detail of the calculations involved.

To check the validity of the process consider a fraud firm manipulating the financials and reporting inflated earnings numbers for a while that return to normal earnings after the fraud is revealed. For simplicity, assume the firm is the typical industry firm as measured using multiples before the fraud and that there is no multiple change over the fraud period. In this case, the predicted hypothetical multiple is the same as the starting multiple which equals the industry multiple. Note that the time-limited manipulation has no effect on the implied value, but it very well could on the observed multiple that is likely to be lower. This will be the case if investors, consumers, suppliers and others change the terms under which they interact with the firm as a result of the fraud. Karpoff and Lott (1993) label this the lost reputation effect, and Karpoff, Lee and Martin (2008) see this as the biggest source of costs with corporate fraud. Note also that this approach accounts for industry trends that have nothing to do with the fraud. If, for example, during the fraud period industry fundamentals goes down by 10%, and the fraud firm's fundamentals go down by the same percentage, we would not want to attribute this decline as a cost to the fraud, and this process ensures that we do not.

V.3. From Costs of Detected Fraud to Social Costs of Fraud

To go from this number to an estimate of social costs of fraud in firms with detected fraud and undetected fraud a few more refinements are required. First, one reason the enterprise value may be lower after the fraud is that there are fines and other penalties and the stock price could capture these expected fees that the firm will pay. These are not social costs, as someone else receives them (e.g. the government, plaintiff law firms). They should be excluded in considering social costs for fraud firms, and certainly if we consider social costs for non-detected fraud firms where we do not expect these costs to ever be paid. KLM find that the mean punishment fines and settlements, both official and private, are 3.7 percent of equity value. [11]

Second, we seek to apply our findings of the costs of fraud from firms with detected fraud to the iceberg firms with undetected frauds by making some additional assumptions. There are good reasons to believe that there will be reputational costs in firms with undetected frauds as well. If a firm for example commits a fraud by creating a false perception that developments are going well when they are going poorly this will eventually be learned by employees, buyers and suppliers to the firm reducing their reputation even if there is no official fraud. Gande and Lewis's (2009) results that show partial anticipation of the fraud also shows that the market reflects such information even in the absence of an official fraud statement.

V.4. Disclaimers on our Method for Calculating the Cost of Fraud

Before presenting results, we also want to note that our approach also has limitations. One limitation that may make our estimate overly large is that it does not correct fully for information about

[11] We recognize that using this estimate is conservative. Insurers, and/or other firm stakeholders such as accounting firms or even directors pay part of these fines, reducing the cost borne by firm shareholders.

fundamentals that might be confounded with revelation about the fraud. As noted above, we attempt to correct for this using changes in industry over time, but there could very well be extra fundamental information about the firm that does not relate to the industry (e.g. in a pharmaceutical firm information about a specific clinical trial) and does not relate to the fraud

V.5. Results: The Cost of Fraud

Tables 5 and 6 present the results from this analysis. Table 5 Panel A provides summary statistics for our sample, showing data pre fraud and post fraud. Means always exceed medians showing positive skewness in size, and post fraud measures show the decline in enterprise value, while there is growth in assets and fixed assets over the fraud period. Panel B shows more clearly the value destruction over the fraud period. The median firm actual declines substantially for example from 12.88 to 9.85 from pre to post fraud, while the median industry firm was relatively unaffected over the period. The sixth column in panel B provides the firm counterfactual using our methodology, that is how the median fraud firm would have done over the fraud period if it had simply followed industry changes in multiples. For example the median fraud firm started with an EBITDA multiple of 12.88, and by our measure would have had a multiple at the end of the fraud period of 12.96. We find instead that the post fraud multiple is 9.85. We focus on the EV/EBITDA multiple as we view this as most easily comparable across firms. We provide results for sales multiples, and asset multiples for robustness. Unfortunately, we do lose some data points with EBITDA multiples arising from losing firms through bankruptcy and negative earnings.

In Table 6 we provide our key findings. Panel A presents the implied costs of fraud in dollars, while panel B expresses these as a percentage of enterprise value. We find a substantial cost to fraud of 23.3% of enterprise value. Subtracting off the cost for fines and other penalties, we arrive at an estimated cost of 21.7 % of enterprise value. Note that using sales or assets produces substantially larger estimates at 27 to 43% of enterprise value.

Finally, we are ready to assess the overall costs associated with fraud for large firms. As a first estimate, we can apply this approximation of average costs in our fraud sample to the population of publicly-traded firms with more than $750 million in assets. With 14.5% of firms estimated to engage in fraud, and those frauds in turn costing 21.7% of enterprise value, this suggests 3.15% of enterprise value is lost to fraud (i.e. 0.145*21.7=3.15).

A possible concern with this estimate is we are applying the same fraud cost to detected and undetected frauds, whereas the costs with each might be very different. Fortunately, we have the data to tackle this question directly coming from the AA experiment. There we were able to identify firms that ended up with detected fraud that we hypothesize would not have been detected absent the demise of AA. We therefore look at their costs and see how they compare with that of the rest of the firms in our sample. We provide this data in Panels C and D of Tables, providing summary statistics on the pre and post fraud actuals and counterfacutals for the sample of firms with AA as Auditor in 2000 or 2001, and then showing the costs of fraud in dollars and as a percentage of enterprise value. As expected, these costs are lower, but they remain substantial at 15.6% of enterprise value. Thus, we can put a lower estimate on the costs of fraud of 2.5% (4%*.217+10.5%*.156=2.5%) .

VI. Conclusion

In this paper we set out to answer the question of the pervasiveness of corporate fraud in the United States and to assess its costs. Using a dataset of corporate frauds in large corporations that impact shareholder value and are caught from DMZ, we take the next step to estimate the iceberg of undetected frauds to infer the unconditional probability that a fraud is committed whether or not it is subsequently caught. Our identification comes from observing situations in which the incentives for fraud detection are high. In particular, capitalizing on the natural experiment provided by the demise of Arthur Andersen, we estimate that approximately 14.5 percent of firms are engaging in fraud, based on the increased probability of a fraud being revealed following the forced turnover of external auditors (with a lower bound estimate of 5.6%). We check for validity of our results using an incentives and opportunities estimation framework and by surveying incoming MBAs, providing broadly similar results.

Having established the incidence of fraud, we then explore the social cost of fraud capturing costs borne by investors over and above the losses they incurred from the deterioration in firm fundamentals that often is the spur for the fraud in the first place. We introduce a new methodology that produces an estimate that the median cost of fraud in our sample is 20.4% of the pre fraud enterprise value. We also use pre-existing estimates from other researchers that arrive at broadly similar results. Finally, we put these two findings together to come up with an estimate of the cost of fraud, which we find to be 2.5-3.1% of enterprise value.

We think our findings have relevance both for investors in firms and for policy. The AA experiment suggests that the detection rate in normal times is just 27.5% of what it is during extraordinary times, suggesting significant scope to increase the engagement of fraud detectors. Consistent with a gap between what could be and what is, we also find significant differences across other fraud detectors when we compare situations with high and low incentives for fraud detection. This evidence establishes that existing fraud detectors can be more active. The social cost calculations we arrive at, along with that of prior researchers, also establishes that these frauds have substantial costs over and above hiding weaknesses in firm fundamentals.

What the evidence does not speak to directly is whether it is cost effective for investors and policy makers to take steps to increase on a permanent basis the detection activity. It is surely true that there are costs with heightened detection that we do not measure as well as benefits. It is also undoubtedly true that policy interventions, to the extent they subject all firms to the same treatment, create additional costs.

Appendix I: Data Appendix

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

First, they restrict attention to alleged frauds that ended 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.[12] 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 suggested to divide frivolous suits from meritorious ones.[13] Fifth, they exclude those security frauds that Stanford classifies as non-standard, including mutual funds, analyst, and IPO allocation frauds.[14] 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.[15]

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

The probability of manipulation, ProbM Score, of Beneish (1999) and Beneish and Nichols (2007) 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

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[12] 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.

[13] Grundfest (1995), Choi (2004) and Choi, Nelson, and Pritchard (2005) 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.

[14] 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.

[15] 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.

SGAI = Sales, General and Administrative expenses Index
ACCRUALS - Total Accruals to total assets
LEVI = Leverage Index

For a complete description of the motivation for each item as an indicator of potential for manipulation and for the compustat codes leading to the calculation of the indices, please see the papers referenced above. We followed their compustat definitions exactly to construct the ProbM Score yearly for the large corporations in our sample. According to Beneish (1999), a score greater than -2.22 indicates a strong likelihood of a firm being a manipulator

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

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Specifically, 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:

$$m_{ijs} = \frac{Long\ Term\ Debt_{is} + \ Market\ Equity_{is}}{Y_{is}}, \tag{4}$$

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 3-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.:

$$\hat{m}_{ijt} = m_{ijs} \frac{M_{jt}}{M_{js}}. \tag{5}$$

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:

$$Counterfactual\ Enterprise\ Value = \hat{m}_{ijt} Y_{it}, \quad for\ Y_{it} \in \{revenue_{it},\ fixed\ assets_{it}, EBITDA_{it}\}. \tag{6}$$

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.

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

(6)

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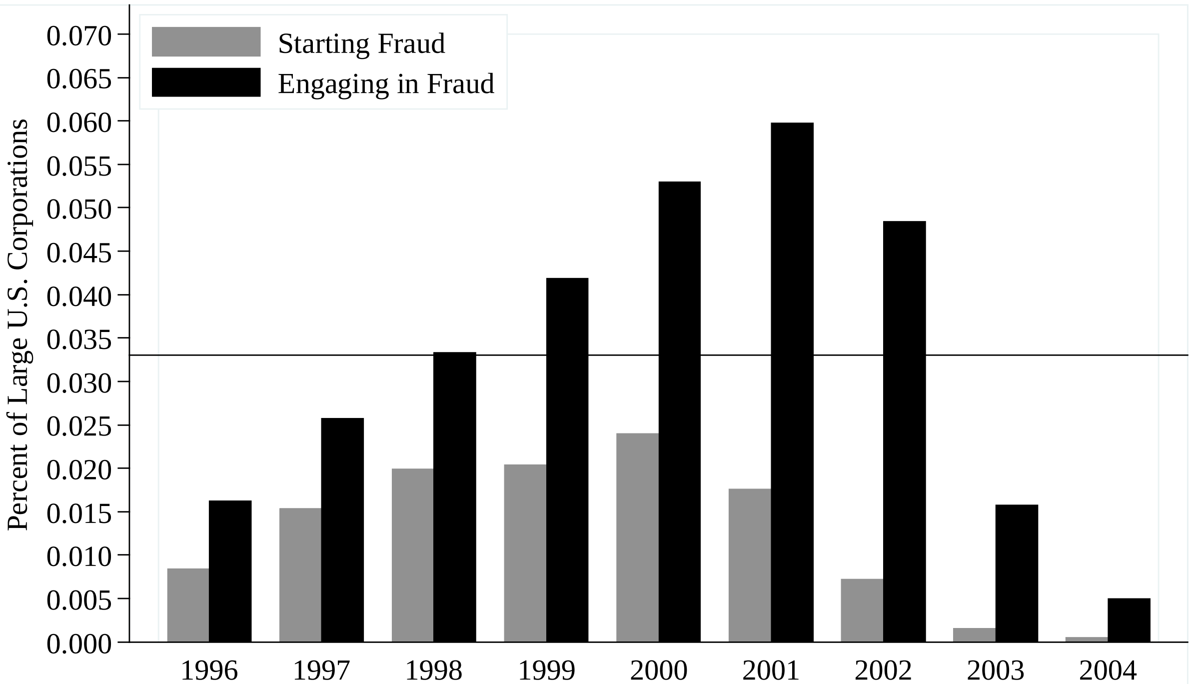

Figure 1: Percentage of Large Corporations Starting and Engaging in Fraud

The grey and black bars respectively report the percent of firms starting and engaging in fraud. The reference line at 0.033 is the overall mean percent of firms engaging in fraud.

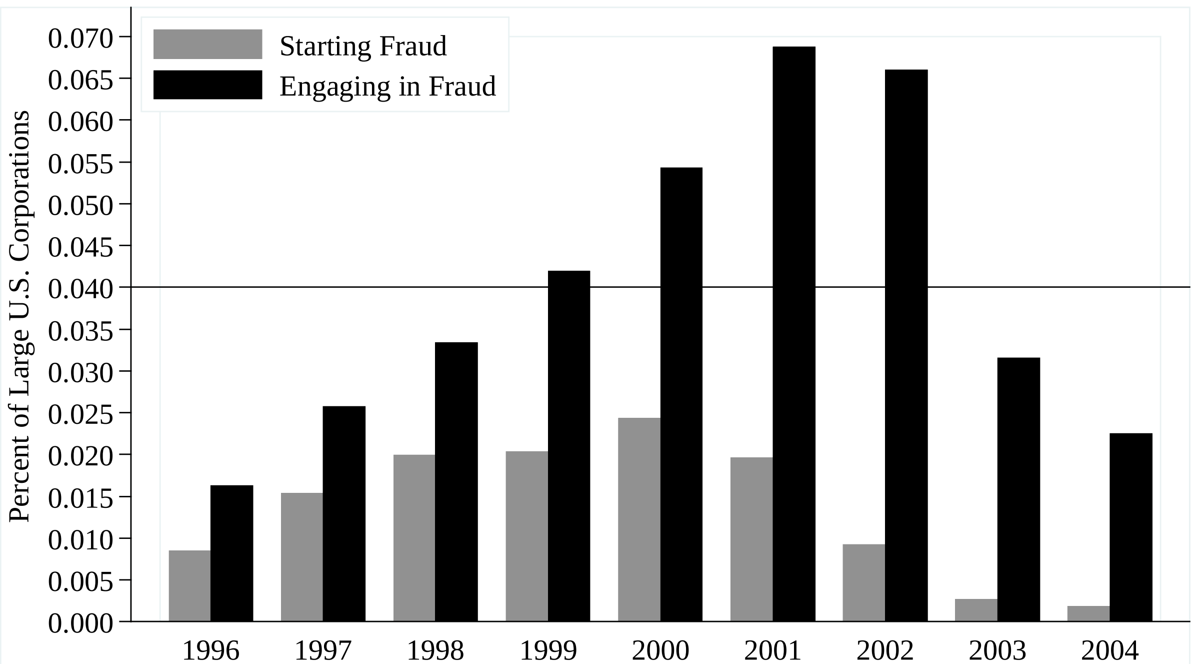

Figure 2: Adjusted Percentage of Large Corporations Starting and Engaging in Fraud

The grey and black bars respectively report the percent of firms starting and engaging in fraud. The reference line at 0.04 is the overall mean percent of firms engaging in fraud. The figure adjusts for the empirical distribution of frauds which in expectation will be caught.

Table 1: Did Arthur Andersen have Clients More Likely to Commit Fraud?

All of the statistics below are for 1998-2000. The columns divide the sample of all Compustat firms with more than $750 in assets into Arther Andersen (AA) clients and otherwise. In the last three columns, otherwise is all non-AA clients which have a Big 5 auditor. Presented are the means and counts, as well as p-values for ttests that the non-AA clients differ from AA clients on each of the statistics. In Panel A, the variables examined are the ProbM Score (probability of manipulation) of Beneish (1999), followed by the eight financial statement components Beneish identifies as making up the scoring of manipulation. The penultimate variables is whether the auditor issued a qualified opinion (which is very rare) or issued a nonqualified opinion with and explanation. The final variable is whether AA firms started fraud more (an indicator for fraud propensity) prior to 2000. In Panel B, the variables are characteristics of the clients; namely, the log of total assets and the ratios of long term debt ot total assets, sales to assets, and EBITDA to sales. Panel C presents OLS and quantile (median) estimations as to whether AA cleints differ on the ProbM Score of Beneish (the dependent variable). ***, **, and * denote significance at the 1%, 5%, and 10% levels respectively. standard errors are in brackets.

Panel A: Univariate Tests for Difference of AA Firms in Manipulation, Fraud, and Auditor Opinion

	AA Firms		All Non-AA			Big 5 Non-AA		
	Mean	Obs.	Mean	Obs.	P-value	Mean	Obs.	P-value
ProbM Score	-2.232	912	-2.230	4,557	0.979	-2.249	3,335	0.704
Days Sales Receivables	1.048	1,008	1.095	4,821	0.597	1.111	3,538	0.544
Gross Margin Index	0.848	1,037	1.007	4,938	0.157	1.011	3,641	0.197
Asset Quality Index	2.452	1,038	2.875	4,943	0.675	2.847	3,643	0.690
Sales Growth Index	1.333	1,037	1.283	4,939	0.240	1.283	3,642	0.295
Depreciation Index	194.6	1,035	286.6	4,940	0.718	247.6	3,641	0.832
SG&A Expense	1.068	940	1.048	4,672	0.694	1.057	3,436	0.854
Accruals/Assets	1.046	1,035	1.112	4,941	0.220	1.126	3,641	0.198
Leverage Index	-0.051	1,095	-0.046	5,224	0.137	-0.047	3,842	0.195
Fraud Started	0.017	1,097	0.016	5,231	0.836	0.017	3,847	0.877

Panel B: Comparison of Size, Use of Debt, and Profitability for pre-2000 AA and non-AA Firms

	AA Firms		All Non-AA			Big 5 Non-AA		
	Mean	Obs.	Mean	Obs.	P-value	Mean	Obs.	P-value
Log Assets	8.078	1,097	8.152	5,227	0.0904*	8.190	3,844	0.013**
LT Debt / Assets	0.324	1,093	0.241	5,226	0.000***	0.248	3,843	0.000***
Sales / Assets	0.811	1,097	0.717	5,225	0.000***	0.754	3,843	0.026**
EBITDA / Sales	0.102	1,095	0.092	5,225	0.001***	0.096	3,843	0.074*

Panel C: Multivariate Tests for Difference of AA Firms in Manipulation (ProbM : dependent variable).

	(1)	(2)	(3)	(4)
	OLS	OLS	Medians	Medians
Arthur Andersem	0.035	0.024	0.015	0.015
	[0.031]	[0.032]	[0.012]	[0.012]
Log Assets	-0.069***	-0.084***	-0.023***	-0.026***
	[0.009]	[0.010]	[0.003]	[0.004]
Sales / Assets	-0.094***	-0.100***	-0.040***	-0.038***
	[0.017]	[0.018]	[0.006]	[0.007]
EBITDA / Sales	-1.418***	-1.434***	-0.787***	-0.815***
	[0.139]	[0.152]	[0.052]	[0.058]
LT Debt / Assets	-0.240***	-0.318***	-0.111***	-0.155***
	[0.060]	[0.068]	[0.022]	[0.026]
Constant	-1.427***	-1.267***	-2.166***	-2.121***
	[0.081]	[0.094]	[0.030]	[0.036]
Observations	11,033	8,672	11,033	8,672
R-squared	0.022	0.025	0.015	0.015
Sample:	All 1994-2000	Big 5 1994-2000	All 1994-2000	Big 5 1994-2000

Table 2 – Pervasiveness of Fraud Based on AA Natural Experiment

The table presents the calculations described step-by-step in Section III of the text. Data for the calculations are from DMZ and Compustat.

Observed inputs:

Pr (engage, caught) =	4.0%	Observed frauds are occurring in 4% of large corporations. (See figure 1.)
Pr (engage*, caught) =	0.38%	Percent of U.S. large corporations engaging in financial fraud that is detected after AA demise.
Pr (engage*, caught|AA) =	1.38%	Percent of AA clients large corporations engaging in financial fraud that is detected after AA demise.

Estimates	*Estimate: Detection likelihood*		*Estimate: Unconditional likelihood of fraud*	
Financial Firms Estimate:	Pr (caught | engage*) =	0.38% / 1.38% = 0.275		
Best Estimate Assumption:	Pr (caught | engage) =	Pr (caught | engage*)		
	Financial fraud are no more hidden that future-type fraud.			
Best Estimate:	Pr (caught | engage) =	27.5% of started frauds are caught	Pr (engage) =	4.0% / 27.5% = 0.145: 14.5% of firms are committing fraud
Lower Bound Assumption:	Pr(caught | engage^{future}) = 1 All future-type fraud are caught. Thus, Pr (caught | engage) = Pr(engage^{future}) + Pr(engage*) Pr (caught | engage*)			
Lower Bound Estimate:	Pr (caught | engage) =	0.353+0.647*0.275: 53.1% of started frauds are caught	Pr (engage) =	4.0% / 51.3% = 0.075: 7.5% of firms are committing fraud

Table 3: Variables in the Engaging and Detecting Bi-Probit for Incentives and Opportunities Estimation

The table presents the variables, their description, and the source of the data for the Incentives and Opportunities Estimation. The variables are divided into three categories – those included in both the engaging in fraud and detecting fraud equation, those included only in the engaging in fraud equation, and those included in only the detecting fraud equation. High Incentives and Opportunities indicator variables are a subset of the variables which could potentially affect both engaging and detecting fraud.

Variable	Description	Source
Panel A: Variables in Both Engaging and Detecting Equations		
High Incentives and Opportunities Indicator Variables		
Analyst Coverage Indicator	A dummy variable that takes the value of 1 for companies with higher than the median value of analyst coverage in companies with more than $750 million in assets.	I/B/E/S
Media Coverage Indicator	A dummy variable that takes the value of 1 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
Shortability Indicator	A dummy variable that takes the value 1 for companies with a greater than median level of institutional shareholding in the prior year.	Compact-D
Regulated Firm Indicator	A dummy variable that take the value of 1 if the firm is in the following categories: 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) and others
Fortune Best 100 Indicator	A dummy variable that takes the value of 1 if the company is a *Fortune Best 100* firm.	Fortune magazine
Qui-Tam Industry Indicator	A dummy variable that takes the value of 1 if the industry is one in which qui tam lawsuits are possible. Included are healthcare and defense contractor industries.	Civil Division, Department of Justice
Post Sox Indicator	A dummy variable that takes the value of 1 if the time period is post-SOX.	Legislation date
Main Variables in Both Engaging and Detecting Equations		
Company Size	Log of total book assets	Compustat
Stock Return	Total return on stock	CRSP
R&D	R&D expenditures / total assets	Compustat
Panel B: Variables in Engaging Equation Only		
In-Money Exercisable Options	The sum of the in-the-money exercisable options for all executives.	Execucomp
Option & Restricted Stock Grants	The average of the ratio of restricted stock grants divided by total compensation across executives for a firm-year.	Execucomp

Panel C: Variables in Detecting Equation Only		
Abnormal ROA	Residual from regression with *i* denoting company; *j* industry; and *t* time: $ROA_{ijt} = \alpha_0 + \alpha_1 ROA_{ijt-1} + \alpha_2 \overline{ROA_{jt}} + \epsilon_{ROA,ijt}$, where $\overline{ROA_{jt}}$ denotes the industry average. This estimation removes serial correlation and the industry effect.	Compustat
Abnormal Stock Return	Residual from CAPM regression: $r_{it} = r_{ft} + \beta_i(r_{mt} - r_{ft}) + \epsilon_{r,it}$. r_{mt}, r_{it}, and r_{ft} denote the market return, the firm return, and the risk free rate, all in quarter *t*.	CRSP
Abnormal Settlements	Residual from regression with *j* denoting industry, and *t* time: $S_{jt} = \gamma_0 + \gamma_1 S_{j,t-1} + \epsilon_S$. S_{ji} is the sum of settlement dollars including insurance payouts of an industry j in year t.	DMZ
Sarbenes-Oxley Shock	Equals one if the start date is pre-SOX and the period of the potential detection is post-SOX.	Legislation date

Table 4: Partial Observability, Bi-variate Probit Estimates

Estimates in coulmns 1 to 4 are marginal effects from probit models. The dependent variable in columns 1-2 is an indicator for a fraud being started in firm-year data. The dependent variable in columns 3-4 is an indicator of fraud detection. Columns 5 reports coefficients from the partial observability, bivariate probit model of Poirier. Column 6 reports the marginal effect on the probability of catching a fraud condition on it being started. The independent variables are as defined in Table 3. Standard errors are in brackets. ***, **, and ** denote significance at the 1%, 5%, and 10% confidence intervals, respectively.

	(1)	(3)	(2)	(4)	(5)	(6)
	Probit (Start=1) MFX	Probit (Start=1) MFX	Probit (Caught=1) MFX	Probit (Caught=1) MFX	BiProbit (Caught, Start) Estimates	MFX Con-ditional: P(Caught | Start)
Start Equation						
Log Options Held	-0.0002	-0.0005			-0.0347***	
	[0.0005]	[0.0004]			[0.0123]	
Incentive Pay % (lag)	0.0349***	0.0310***			0.286**	
	[0.0069]	[0.0065]			[0.124]	
Hi Analysts		0.0033			-0.135**	-0.0051
		[0.0032]			[0.0653]	
Hi Media		-0.0028			-0.0688	0.0420
		[0.0033]			[0.113]	
Hi Shortability		0.0030			-0.139**	-0.0198
		[0.0029]			[0.0593]	
Qui Tam Industry		0.0141*			-2.969***	-0.0101
		[0.0074]			[0.803]	
Regulated		-0.0050			0.1730	-0.0205
		[0.0038]			[0.499]	
PostSOX		-0.0131***			-1.594***	0.0357
		[0.0027]			[0.526]	
Log Assets	0.0009	0.0016			0.129*	0.0028
	[0.0011]	[0.0011]			[0.0713]	
Log R&D	0.0002	-0.0005			0.0889	0.0040
	[0.0006]	[0.0006]			[0.0703]	
Leverage	0.0124	0.0098			-0.0560	0.0190
	[0.0093]	[0.0085]			[0.215]	
ROA (lag)	0.0326*	0.0155			-12.68***	0.0131
	[0.0189]	[0.0180]			[3.435]	
Stock Return (lag)	0.0057***	0.0048***			-1.176***	-0.0082
	[0.0014]	[0.0013]			[0.347]	
Vix	0.0021***	0.0016***			0.0051	0.1472
	[0.0005]	[0.0005]			[0.0688]	
Caught Equation						
Abnormal Leverage (lag)			0.0036	0.0060	0.0905*	
			[0.0042]	[0.0046]	[0.0519]	
Abnormal ROA (lag)			0.0000	-0.0001	0.0015	
			[0.0002]	[0.0002]	[0.00262]	
Abnormal Stock Return (lag)			-0.0069	-0.0145	-0.0517	
			[0.0108]	[0.0099]	[0.0911]	
Abnormal VIX			-0.0012***	0.0000	0.0040	
			[0.0004]	[0.0005]	[0.00514]	

(continued on next page)

Table 4: Partial Observability, Bi-variate Probit Estimates (continued)

Caught Equation (continued)						
Hi Analysts (lag)				0.0087***	0.176**	0.0271
				[0.0026]	[0.0840]	
Hi Media (lag)				-0.0043	0.0690	0.0008
				[0.0029]	[0.102]	
Hi Shortability (lag)				0.0055**	0.185**	0.0142
				[0.0026]	[0.0722]	
Qui Tam Industry				0.0215**	3.014***	0.0002
				[0.0084]	[0.832]	
Regulated				-0.0040	-0.1440	-0.0081
				[0.0034]	[0.481]	
PostSOX				0.0165***	1.521***	0.0006
				[0.0043]	[0.513]	
Log Assets (lag)			0.0024**	0.0014	-0.0796	0.0275
			[0.0009]	[0.0010]	[0.0636]	
Log R&D (lag)			0.0014***	0.0003	-0.0839	0.0108
			[0.0005]	[0.0006]	[0.0703]	
Leverage (lag)			0.0219***	0.0147**	-0.0837	0.0289
			[0.0073]	[0.0072]	[0.221]	
ROA (lag)			-0.0276*	-0.0241*	12.83***	-0.0125
			[0.0159]	[0.0146]	[3.449]	
Stock Return (lag)			-0.0027	-0.0010	1.277***	-0.0131
			[0.0027]	[0.0024]	[0.358]	
Vix			0.0029***	0.0021***	0.0005	-0.0131
			[0.0005]	[0.0004]	[0.0667]	
Observations	6259	6259	8114	7671	5973	
LR/Wald Chi-Square	74.8	105.5	83.2	133.4	42.3	

Table 5: Pervasiveness of Fraud in a Survey of MBAs

We asked MBAs entering the University of Chicago whether they faced a legal dilemma in their jobs before they joined the MBA program, where we defined “legal dilemma” as “In your job you are asked to do something that is illegal. Example: Your boss asks you to lie in reporting sales.” Panel A reports the percentage of MBAs who responded positively by industry, and Panel B reports the percentage by occupation or function in their jobs.

Panel A:

Industry	***Illegal***	**N**
Consulting	11.76%	51
Consumer goods	6.67%	15
Financial services	15.08%	126
Health/Pharmaceutical	14.29%	14
Other	18.18%	77
Total	14.84%	283

Panel B:

Function	***Illegal***	**N**
Accounting	11.11%	18
Consulting	11.54%	52
Corporate - Finace	15.00%	20
Corporate-Sales	13.33%	15
Corporate - Product Management	12.50%	8
Corporate -Other	33.33%	21
Investment Banking	16.67%	42
Investment Management	11.11%	18
Other	13.48%	89
Total	14.84%	283

Table 6: Cost of Fraud

The table presents the statistics setting up the counterfactual exercise to estimate the cost of corporate fraud. 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, but the counts vary slightly by which balance sheet items might be missing. Panels C and D further restrict the sample to the set of firms who were Arthur Andersen clients in 2000 or 2001 and subsequently were revealed to have started fraud during Arthur Andersen's watch. Panels A and C report the enterprise value of firms, and the equity and long term debt componets, as well as the financial statement line items which enter the multiples analysis. The numbers are in millions of USD. Panels B and D presents the median multiples corresponding to the data in panel A and C, respectively. The industry multiples are at the SIC 2-digit level. The counterfactual column in panels B and D is the counterfactual result we use to calculate the counterfactual enterprise value, using the pre periof firm actual multiple and the growth in the corresponding industry multiple. In Panel B, the 3rd, 7th, and 8th columns presents the pvalues from median test of, respectively, the pre-multiple≠ industry, the counterfactual ≠ industry, and the counterfactual ≠ actual post multiple. We do not report the tests for the small sample in panel D.

Panel A: Statistics

	Pre-Fraud				Post-Fraud			
	Median	Mean	StDev	Frequency	Median	Mean	StDev	Frequency
Equity Market Capitalization	5,200	16,173	31,747	188	2,248	9,890	19,514	188
Long Term Debt	801	2,939	7,825	186	1,068	6,518	33,226	186
Enterprise Value	6,812	19,123	34,150	188	4,227	16,258	41,817	189
EBITDA	467.1	1,312	2,036	161	466.6	1,710	3,399	161
Sales	2,452	6,857	9,006	184	3,499	8,418	11,536	184
Assets	3,850	14,896	34,512	186	4,229	24,362	79,297	186

Panel B: Multiples for Full Sample

	Pre-Fraud			Post-Fraud				
	Median Firm Actual	Median Industry	*Medians Test P-value*	Median Firm Actual	Median Industry	Firm Counter-factual	*Median Actual=Counter P-value*	*Median Industry= Counter P-value*
EBITDA Multiple	12.88	10.54	*0.001*	9.85	11.06	12.96	*0.007*	*0.014*
Sales Muliple	2.29	1.84	*0.048*	1.42	1.88	2.25	*0.000*	*0.023*
Assets Multiple	1.37	1.12	*0.001*	0.80	1.06	1.24	*0.000*	*0.122*

Panel C: Statistics Limited to the Sample of Firms with Arthur Andersen as Auditor in 2000 or 2001

	Pre-Fraud				Post-Fraud			
	Median	Mean	StDev	Frequency	Median	Mean	StDev	Frequency
Equity Market Capitalization	5,102	9,164	11,369	18	4,209	7,786	11,302	18
Long Term Debt	1,526	8,083	21,625	18	3,054	32,663	103,654	18
Enterprise Value	12,905	17,246	31,809	18	9,476	40,450	113,265	18
EBITDA	2,368	6,127	7,378	17	7,536	12,091	16,567	17
Sales	472	1,072	1,968	18	493	2,601	6,314	18
Assets	8,068	26,251	74,117	18	12,332	60,930	186,673	18

Panel D: Multiples Limited to the Sample of Firms with Arthur Andersen as Auditor in 2000 or 2001

	Pre-Fraud		Post-Fraud		
	Median Firm Actual	Median Industry	Median Firm Actual	Median Industry	Firm Counter-factual
EBITDA Multiple	11.40	9.23	10.89	11.95	13.88
Sales Muliple	2.68	1.82	1.83	2.44	2.24
Assets Multiple	1.18	0.90	0.77	0.82	0.95

Table 7: Cost of Fraud

The table presents the results from the counterfactual exercise to estimate the cost of corporate fraud. The sample in Panels A and B is all of 216 fraud firms which have statistics for pre and post periods. The sample for Panels C and D is the set of firms who were Arthur Andersen clients in 2000 or 2001 and subsequently were revealed to have started fraud during Arthur Andersen's watch. The observation counts are given in Table 7. Because the data have non-normal distributions, we present the costs distribution and focus on the median results. Panels A and C present the results in dollars (million), and Pancels B and C translate these figures into the percent of enterprise values, using market value of equity. Median and means are in millions of USD. Panels B and D present the same respective results as a percentage of enterprise values (market capitalization plus balance sheet long term debt). The right hand side columns labelled Adjusted Costs reflect adjustments to subtract out legal costs for the percentage of firms that are never caught (1-0.275)%, using the estimates in Karpoff, Lee and Martin (2010).

Panel A: Costs

	Distribution of Costs in $ Million			Adjusted Cost in $ Million		
Based on:	25th	**Median**	75th	25th	**Median**	75th
EBITDA Multiple	16	**1,090**	8,405	-46	**1,016**	7,670
Sales Muliple	228	**2,306**	10,998	167	**2,284**	10,761
Assets Multiple	374	**2,210**	7,262	296	**2,037**	7,065

Panel B: Costs Expressed as a Percentage of Enterprise Value

	Distribution of Costs as % EV			Adjusted Cost as % of EV		
Based on:	25th	**Median**	75th	25th	**Median**	75th
EBITDA Multiple	0.004	**0.233**	0.510	-0.011	**0.217**	0.504
Sales Muliple	0.081	**0.437**	0.628	0.062	**0.430**	0.625
Assets Multiple	0.094	**0.373**	0.614	0.078	**0.367**	0.610

Panel C: Costs Limited to the Sample of Firms with Arthur Andersen as Auditor in 2000 or 2001

	Distribution of Costs in $ Million			Adjusted Cost in $ Million		
Based on:	25th	**Median**	75th	25th	**Median**	75th
EBITDA Multiple	-2,798	**603**	13,949	-2,846	**510**	13,107
Sales Muliple	-762	**995**	9,006	-927	**987**	8,959
Assets Multiple	-33	**2,558**	8,630	-44	**2,468**	7,126

Panel D: Costs for AA Sample Expressed as a Percentage of Enterprise Value

	Distribution of Costs as % EV			Adjusted Cost as % of EV		
Based on:	25th	**Median**	75th	25th	**Median**	75th
EBITDA Multiple	-0.497	**0.167**	0.421	-0.512	**0.156**	0.418
Sales Muliple	-0.086	**0.240**	0.545	-0.107	**0.238**	0.543
Assets Multiple	-0.056	**0.262**	0.397	-0.076	**0.246**	0.393