# Corporate Scandals and Household Stock Market Participation

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October 29, 2013

**Abstract**

We show that after the revelation of corporate frauds in a state, the equity holdings of households in that state decrease significantly both in the extensive and the intensive margins. Using an exogenous shock to fraud detection and exogenous variation in households' lifetime experiences of corporate fraud, we are able to establish that the impact of fraud revelation in local companies on household stock market participation is causal. Even households that did not hold stocks in the fraudulent firms decrease their equity holdings and all households decrease their holdings in fraudulent firms as well as non-fraudulent firms. The decrease in households' demand for equity due to corporate fraud generates significant economic costs for firms headquartered in the same state as the fraudulent firms, which experience an increase in the cost of capital and a decrease in valuation and in the number of shareholders.

**JEL classification:** G30, D12, D14
**Keywords:** corporate securities fraud, corporate scandal, household stock market participation, local bias

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## 1. Introduction

Corporate scandals have large negative effects on the value of the firms that are discovered having committed fraud (Karpoff, Lee, and Martin 2008). Besides being concerned about the direct losses inflicted to shareholders by frauds, regulators also worry about the indirect effects on market participants' willingness to participate in financial markets, which may generate even larger losses by increasing the cost of capital for other firms. However, evidence of the externalities generated by corporate frauds is quite limited.

This paper aims to fill this gap by exploring the effect of corporate scandals on the demand for equity and households' willingness to (directly or indirectly) participate in the stock market. To generate cross-sectional and time-series variation in households' exposure to corporate scandals, we note that households are likely to be more exposed to corporate frauds affecting firms headquartered in the state where they live. This is the case not only because households tend to hold the stocks of local firms[1] and are likely to experience losses in their stock portfolios when these firms are revealed having committed frauds, but also because coverage of local news or personal interaction with neighbors increases their exposure to these episodes.

We ask whether corporate scandals in a state reduce equity holdings and household stock market participation in that state, controlling for nationwide macroeconomic conditions and capturing asynchronous local shocks with a host of household level and state level controls. We find unambiguous evidence that household participation decreases both on the extensive and intensive margins following corporate scandals in the state where the household resides. Older

[1] Households' portfolios are known to have a pronounced local bias (Grinblatt and Keloharju 2001; Ivkovic and Weisbenner 2005; Seashole and Zhu 2010).

households and the households that participate in the stock market in the period preceding the frauds are more affected by these episodes.

Using brokerage data on a sample of retail investors across the US (from Barber and Odean (2000)), we further show that after revelation of fraud in a state *all* households, not only the ones holding the stocks of fraudulent firms, decrease their equity holdings. This suggests that the decrease in household stock market participation is not driven by financial losses associated with holdings in fraudulent stocks. Moreover, households decrease their stock holdings in fraudulent *as well as* non-fraudulent firms, suggesting that fraud revelation may cause a negative spillover for non-fraudulent firms.

Concerns may arise that our findings are driven by state level omitted factors that drive both the revelation of corporate fraud and household stock market participation. For instance, the revelation of corporate fraud generally occurs at the beginning of economic downturns that may independently drive households' decision to reduce their equity holdings (Wang, Winton and Yu, 2010). To establish a causal effect of corporate scandals on local households' stock market participation, we use two different strategies.

The first strategy utilizes an exogenous shock to fraud detection due to the sudden demise of the large auditing firm, Arthur Andersen, in 2002. All Arthur Andersen's clients were forced to change auditors. Since new auditors have incentives to "clean the house", the firms that switched auditor due to Arthur Andersen's demise had higher probability to be revealed as having committed fraud (Dyck, Morse and Zingales, 2013). This led to an exogenous increase in the probability of fraud revelation that differs across states, depending on the fraction of firms in the state that were Arthur Andersen's clients right before Arthur Andersen's demise. We can thus use the fraction of firms in a state that were Arthur Andersen's clients right before Arthur

Andersen's demise as an instrument for fraud revelation in that state in the period following the shock. We find that the exogenous variation in fraud revelation due to differences in the presence of Arthur Andersen's clients across states leads to a decrease in household stock market participation.

The second identification strategy utilizes variation in households' life-time experience about corporate scandals. Even households living in the same state at a particular point in time can have different corporate fraud experiences because they may have moved across states and have different experiences depending on their age. In these specifications, we are able to absorb any state level shocks by including interactions of state and year fixed effects and continue to find that the variation in households' fraud experiences has a negative impact on household stock market participation. Overall, our identification strategies enable us to establish a causal impact of corporate fraud on household stock market participation.

Finally, we examine the spillover effect of corporate fraud on other firms headquartered in the same state that have not been revealed having committed fraud. We find that local firms that are not involved in the scandals experience a significant decline in the number of shareholders, particularly the number of retail shareholders. These firms also experience higher cost of capital and lower valuations, presumably as a result of lower risk sharing (Merton, 1987) or of the need to attract distant shareholders after the drop in local demand (Hong, Kubik and Stein, 2008). We show that these results are unlikely to be driven by an increase in the probability that other firms in the state have also committed fraud or by other uncontrolled changes in state economic conditions.

This paper is related to several strands of literature. First, a strand of literature focuses on the incidence and consequences of corporate frauds. Prior studies have documented significant

costs to fraudulent firms upon the revelation of fraud (Karpoff et al., 2008; Dyck, Morse and Zingales, 2013). Other studies document that misreporting firms experience significant increases in the cost of equity (e.g., Hribar and Jenkins (2004), Kravet and Shevlin (2010)) and the cost of debt (Graham, Li, and Qiu (2008)). While most papers study the consequences for the firms committing fraud, Goldman, Peyer and Stefanescu (2012) and Gleason, Jenkins, and Johnson (2008) explore contagion effects among industry peer firms. These papers highlight that the valuations of peer firms are affected negatively by corporate scandals and argue that investors reassess the financial statement information provided by firms similar to the ones committing frauds. Instead of focusing on the informational spillover, we show that there is a distinct and potentially more pernicious spillover effect through the demand for equity of local households.

Second, the paper is related to the literature on household stock market participation (see Guiso and Sodini, 2013, for a recent review). Guiso, Sapienza, and Zingales (2008) show that households' subjective probability of being cheated helps explain the decision not to participate in the stock market. Malmendier and Nagel (2011a and b) highlight the effects of macroeconomic experiences on the expectations of different cohorts of households about stock market returns and inflation and, consequently, on their decisions to hold stocks and other financial assets. Malmendier and Nagel (2011a and b) find that since households form their expectations weighing past experiences, the effect of past experiences decreases over time and are weaker for older households. We highlight the additional effect of corporate governance scandals, controlling for households' macroeconomic experiences. It appears that corporate scandals have more lasting effects on households' equity holdings, as we estimate that the effect of corporate scandals does not significantly decrease over time and is stronger for older households, who have a higher probability of holding stocks in the absence of frauds.

The rest of the article is organized as follows. Section 2 describes the data sources and the main variables of interest. Section 3 discusses the baseline results of the effect of corporate frauds on local households' stock market participation and our identification strategies. Section 4 examines the spillover effect of fraud on local companies. Section 5 concludes.

## 2. Data Sources and Main Variables of Interest

### *2.1 Households Survey Data*

We obtain information on households' equity holdings, wealth, income, state of residence, and other demographic characteristics from the Panel Study of Income Dynamics (PSID), a longitudinal survey of representative U.S. individuals and their families, compiled by the University of Michigan.[2] The database provides the state of residence for all households in the survey. This is crucial for our study and enables us to exploit how the variation in corporate fraud revelation across states and over time affect households' decisions to participate in the stock market.

The information on household financial wealth and equity holdings in particular is available at five-year intervals starting in 1984, and then every other year from 1999 to 2009. For this reason, the sample period for our household data is from 1984 to 2009. When we construct the control variables, we consider only the characteristics of PSID household heads and neglect other members of the household.

Our main proxy for household equity market participation, "*Equity Participation"*, is an indicator variable that equals one if the household holds any stocks in publicly held corporations,

[2] PSID was started in 1968 and conducted at an annual frequency until 1997; the frequency became biennial from 1997 onward. In the same year, the original core sample was reduced from roughly 8,500 households in 1996 to approximately 6,300 in 1997.

mutual funds, or investment trusts in a given year. For the first two survey rounds, 1984 and 1989, the questionnaire asks the household to include stocks in employer-based pensions or individual retirement accounts (IRAs). Since the 1994 survey, the same question has been changed to exclude stocks in pensions or IRAs. A separate question asks whether the household has any money in private annuities or IRAs. For the purpose of our study, we wish to focus on households' (direct and indirect) stock investment outside their pension accounts or IRAs because this reflects a more active decision to participate in the stock market. Thus, *Equity Participation* excludes stocks in pension accounts or IRAs except for the 1984 and 1989 surveys. The results are very similar when we exclude these two survey years. Results are also similar when we use an alternative indicator variable "*Equity Participation (IRA)*", which includes stocks in pension accounts or IRAs in all survey years.

We also gauge the extent of equity participation using three different measures. The survey asks households how much they would receive if they sold all the (non-IRA) stock investment and paid off anything they owed on that investment. Based on the answer to this question, we create the variable "*Equity Value*", which reflects the estimated net dollar value of the households' equity investment in a given year. Similarly, based on the answer to the survey question about how much money the household put in stocks during the last year, we create the variable "*Net Equity Purchase*", which indicates the net value of *new* equity investment in that year. Since the latter two variables are highly skewed, we use their logarithm in the empirical analysis.[3] Lastly, we create the variable "*Equity-Wealth Ratio*", which is the value of equity investment as a fraction of the household's total wealth in a given year.

[3] We add one dollar to the equity value before the logarithmic transformation because some households have zero dollars in equity.

We also extract from PSID the following household characteristics that have been shown to be important in explaining households' decision to hold stocks: annual family income, family wealth, number of family members, and, for the household's head, age, years of schooling, and marital status. We exclude from family wealth the value of (non-IRA) equity to avoid any mechanical relation between wealth and equity value.

Panel A of Table 1 reports the summary statistics of the household variables. On average, about 22% of the households participate in the stock market during our sample period. If we include stocks held in pension accounts or IRAs, then the participation rate increases to about 30%, which is comparable to the findings in other studies. The value of household equity investment is highly skewed, with the average being $24 thousand and the median being $0. The net new equity investment is on average $7.7 thousand, and the average equity-wealth ratio is 4.3%; 55% of the household heads surveyed are married; 71% are male, their average age is 45, and they have on average 12.7 years of schooling. The average family in our sample consists of about 3 people, with a family income of $54 thousand per year, and net financial wealth (excluding equity investment) of $131 thousand.

*2.2. Individual Trading Data*

A limitation of the household data is that we do not observe which stocks households hold. To be able to evaluate whether households that did not hold fraudulent firms are also affected and whether households reduce their equity holdings in non-fraudulent firms, we use information from a large discount brokerage firm on the investments of 78,000 households from January 1991 to December 1996. Barber and Odean (2000) provide a detailed description of this data source. Here it is important to note that as documented by Korniotis and Kumar (2013), the distribution of households across states is very similar between the retail investor sample and the

Census data. Thus, even if the brokerage data are less representative of the US population and provide a shorter sample than PSID, they allow us to examine the mechanisms through which fraud revelation affects households.

Using the brokerage data, we define the following variables to capture changes in the intensity of equity market participation. Our main variable of interest is the change in a household's equity holdings between the end of year *t* and *t+1* relative to the household's equity holdings at *t*. We evaluate all positions and their changes using prices at the beginning of the period. Specifically, for household *i* in year *t* with holdings in *k* different stocks, this variable is defined as: $\Delta holding^{i}_{t+1} = \sum_{k} p^{k}_{t}(holding^{ik}_{t+1} - holding^{ik}_{t}) / \sum_{k}(p^{k}_{t} holding^{ik}_{t})$, where $p^{k}_{t}$ is the price for stock *k* at the end of year *t*.

We define two analogous measures of changes in equity holdings for each household distinguishing between change in holdings of firms that have been revealed having committed fraud during the last 12 months and firms that do not (we describe below the definition of fraudulent firm).

Finally, for each household, we define a dummy variable capturing whether the household held stocks of any firm that has been revealed having committed fraud during the last 12 months, the return of the household's portfolio during the previous year, and obtain a number of household characteristics, including number of household members, marital status, and age of the household's head.

*2.3 Corporate Securities Frauds*

Our detected corporate securities fraud sample comes from the Federal Securities Regulation (FSR) database compiled by Jonathan Karpoff, Scott Lee, and Gerald Martin (see, e.g., Karpoff et al., 2012). FSR contains 1,099 hand-collected securities fraud cases in which the

Securities and Exchange Commission (SEC) and/or Department of Justice (DOJ) brought enforcement action from 1978 to 2011. This is the most comprehensive database for federal securities enforcement actions. From this database, we select cases that involve (1) US issuers as defendants, (2) enforcement action against a securities fraud under either the Securities Act of 1933 or the Securities Exchange Act of 1934, and (3) common stock as the primary security registered by the firm with the SEC.[4] This selection process leads to 711 cases involving 702 US companies. FSR provides information about the announcement dates of all key litigation events related to each case.[5] We use the earliest date at which a fraud is revealed to define the fraud revelation year of a case.

To evaluate which households are likely to have been more exposed to frauds, it is crucial for us to know the state in which the alleged firms' headquarters are. We obtain headquarters locations from COMPUSTAT, Compact Disclosure, which records headquarters' changes, and hand-collect any missing information.[6] Panel B of Table 1 reports the distribution of frauds across states and over time. Only 5 out of 51 states have no federal securities fraud litigation during our sample period. There is substantial variation in fraud revelation across states and over time. We exploit precisely this variation across states over time to identify the effects of corporate fraud.

Larger states with more company headquarters, such as California, New York, and Texas, have more fraud cases. For this reason, for each state and year, we compute the fraud revelation

[4] The database also includes enforcement actions against non-fraud related violations committed by corporate issuers (e.g., bribery in foreign countries, obstruction of justice) and violations committed by non-corporate parties (e.g., investment advisors, investment companies). We exclude these cases in our analysis.

[5] These event dates include the trigger event date (the date of the first public announcement of an activity that reveals the fraud), the inquiry date (the date of the first announcement of an informal information request by regulators), the investigation date (the date of the first announcement of formal investigation), the class action filing date (the date of the first filing of the related private securities class action lawsuit), the regulatory action beginning date (the filing date of the first regulatory proceeding), the restatement date (the date of the first announcement of financial restatement), the wells date (the first date of the announcement of a Wells Notice or settlement agreement).

[6] We thank Angie Low for kindly sharing the headquarters data with us.

intensity as the number of revealed frauds divided by the total number of publicly traded companies in the state. Since from PSID we have information on households' equity holdings every five years from 1984 to 1999 and every other year starting from 1999, we cumulate the fraud revelation intensity in recent years in each state. Specifically, "*Fraud in State* $_{jt}$" is the sum of the yearly fraud revelation intensity in the past four years in state *j* in year *t*. This is our main measure of fraud revelation, in which we treat each fraud case equally. When we use the brokerage data and explore the effect of fraud on households' changes in equity holdings at the yearly frequency, we measure fraud intensity over the previous year (*Yearly Fraud in State* $_{jt}$).

We also construct three alternative measures of fraud revelation in which we treat some fraud cases as potentially having a larger impact on households than other cases. First, frauds committed in companies that are relatively more important in a state, either because the state is small and has few companies or because the company is relatively large in the state, may have a larger impact on households not only because the revelation of fraud in these firms may receive more local media coverage, but also because households' portfolios, having a local bias, are more likely to be exposed to these frauds. Local households may also be more exposed to frauds affecting an important firm in the state through the product market or personal interactions with the firms' employees. To capture this, we weigh each fraud case by *1+ market share*, where the market share is the alleged issuer's total book assets, divided by the total book assets of all publicly traded firms in the same state as the firm committing fraud. For example, if a fraudulent firm's market share is 20%, then we count this case as 1.2 cases when we compute the fraud revelation intensity. This augmentation allows larger fraudulent firms to have a bigger effect in our analysis. "*Fraud in State 2*" is the cumulative market-share-augmented fraud revelation intensity in the past four years in a state.

Second, we measure the severity of a fraud case by the cumulative market reaction across the seven key fraud-revelation events associated with the case. Karpoff et al. (2008) call this the "market penalty" or the "reputation cost" of fraud. The market reaction to each event is measured as the alleged firm's market-adjusted announcement day stock return. Since the market reaction to fraud revelation is usually negative, our augmenting factor is (1- cumulative market reaction), which means that each fraud case is counted as (1- cumulative market reaction) cases. For example, if the cumulative market reaction of a fraud case is -0.50, then this case is counted as 1.5 cases. "*Fraud in State 3*" is the cumulative market-penalty-augmented fraud revelation intensity in the past four years in a state.

Lastly, frauds in a local company with high retail ownership are expected to have a larger impact on local households than frauds in firms with high institutional ownership. The FSR database provides information about the institutional ownership of alleged companies. We compute the percentage retail ownership as (1-institutional ownership). Thus, our last augmenting factor is (1+ retail ownership), which means that cases involving firms with larger retail ownership are counted as more influential. "*Fraud in State 4*" is the cumulative retail-ownership-augmented fraud revelation intensity in the past four years in a state.

Panel C of Table 1 reports the summary statistics of the four cumulative fraud revelation intensity measures (in percentage points). On average, more than two companies are revealed as fraudulent in a four-year window in a state, which is about 1% of the total number of public firms in a state. The standard deviation is 2.4%. The alternative measures (*Fraud in State 2-4*) have higher mean and higher standard deviation by construction.

*2.4 State Level Variables*

Frauds are more likely to occur during macroeconomic and industry booms and to be discovered during busts (e.g., Povel, Singh, and Winton 2007; Wang, Winton, and Yu 2010). By exploring whether corporate scandals in a state reduce equity holdings and stock market participation for households in that state, we are able to fully control for aggregate macroeconomic conditions with year fixed effects. Nevertheless, the concern remains that fraud revelation in local companies is more likely when state-level business conditions deteriorate. Poor local business conditions in turn may affect local households' incentives to hold the stocks of local businesses because they may affect expectations of future stock returns or family income. Therefore, omitting state economic conditions could lead to spurious correlation between fraud revelation in a state and households' stock market participation.

To address this concern, we collect data on state economic conditions from the Bureau of Economic Analysis (www.bea.gov/regional/index.htm). For each state and year, we obtain the GDP growth rate, employment growth rate, state population growth rate, state total as well as per capita personal income growth rates. We then compute the average of these variables for the past four years. All these state economic condition measures are highly correlated. Thus, in our analysis we mainly use the "*State GDP Growth*" as a control. The results are however robust to the inclusion of the other controls or if we use only the last year of the state level controls instead of their average in the past four years.

We also control for the stock market performance of local companies, computed as the annual value-weighted return of all public traded companies headquartered in the state. We then compute the buy-and-hold state stock market return over the past four years, and call it "*State Stock Return*".

Panel D of Table 1 reports the summary statistics for the number of public firms in the state, *State GDP Growth* and *State Stock Return*. The average state has 167 public firms in a given year, and enjoys a 6.7% GDP growth rate per annum. The average four-year buy-and-hold state stock market return is 38%.

## 3. Fraud Revelation and Household Stock Market Participation

### *3.1 Empirical Model*

We relate measures of household stock market participation to our proxies for the extent of frauds revealed in the state over the previous four years using the following empirical model:

$$Participation_{ijt} = \beta \times FraudInState_{jt} + \gamma X_{ijt} + h_i + s_j + \zeta_t + \varepsilon_{ijt}, \qquad (1)$$

where participation can be *Equity Participation*, *log(Equity Value)*, *log(Net Equity Purchase)*, or *Equity-Wealth Ratio*. We capture changes in macroeconomic conditions using year fixed effects ($\xi_t$), average differences across states using state fixed effects ($s_j$), and household-specific time-invariant factors by household fixed effects. The matrix $X_{ijt}$ includes a host of time-varying controls that vary across states or across households.

Given the large number of fixed effects we include in most of our specifications, we estimate all equations by ordinary least squares even when they involve a limited dependent variable. Since the decision to hold stocks for a given household is likely to be correlated across time, we cluster standard errors at the household level. The results we present hereafter, however, remain highly statistically significant if we cluster standard errors by states or by time.

### *3.2 Baseline Results*

Panel A of Table 2 relates the household decision whether to hold stocks to our proxies for fraud revelation and a number of control variables. In all columns but column 1, we include

households fixed effects. In the specifications with household fixed effects, we exclude *Years in School* because the value of this variable is largely invariant over time for a given household head. In all specifications, an increase in the fraud revealed in the past four years in the state is associated with a decrease in the probability that the household participates in the stock market. The effect is consistent across the different proxies for fraud, and is statistically and economically significant.

For example, the parameter estimates in column 2 imply that an increase in the state level fraud revelation intensity by 2 percentage points (from the 25$^{th}$ percentile to the 75$^{th}$ percentile) decreases the probability that a household participates in the stock market by 0.72 percentage points. Since approximately 20% of the households participate in the stock market, this implies a 3.6% decrease in the probability of household participation.

In columns 3 and 4, we take into consideration that our measure *Equity Participation* is not fully consistent across all survey years because it includes stockholdings in IRAs only in the first two survey rounds. In column 3, we exclude the first two rounds of the survey in which IRAs are included; in column 4 we use *Equity Participation (IRA)*, which includes stockholdings in IRAs consistently across all survey years. In the latter specification, we also include an indicator variable that equals 1 if the household has an IRA. Our results remain invariant both qualitatively and quantitatively.

Estimates in columns 5 to 7 are largely invariant when we use the alternative measures of fraud exposure. Hence, there seems to be weak evidence that some fraud cases have a larger impact on local households.

Panel B shows that revealed frauds in local corporations negatively affect not only the extensive margin of household stock market participation, but also the intensive margin.

Following periods of high fraud revelation in a state, the amount of household wealth held in stocks, as captured by the logarithm of the value of equity investment, decreases. This result holds true when we exclude the years in which we cannot distinguish whether stocks are held in IRAs in column 2 or when we consistently include the value of stocks held in IRAs in column 3. A concern with the interpretation of this result is that frauds may have a direct negative effect on the dollar amount of equity of a household because typically the shareholders of companies that are discovered to be fraudulent experience large losses. However, we also find that, after periods of high fraud revelation in local companies, households purchase less equity and the proportion of equity investment in the household's total wealth decreases.

These results consistently indicate that fraud revelation in local companies is negatively related to households' equity market participation. This effect is unlikely to be driven by negative income and wealth shocks because we control for the household's wealth and income at the time of the survey. Also, in unreported specifications, we show that fraud revelation in a state has no effect on the income and wealth of households in that state.

The effect of fraud revelation on household stock market participation is also unlikely to be driven by state-level economic shocks, which we attempt to control for. However, the correlation between household equity market participation and fraud revelation in the state could still be driven by some unobservable state level factors. In what follows, we propose two alternative methodologies to address this concern.

*3.3 Identification through an Exogenous Shock to Fraud Revelation*

In this subsection, we address the concern that our results may be driven by unobservable state-wide events, which we were unable to control for, by exploiting an exogenous increase in fraud revelation that is unlikely to be correlated with the state economic conditions.

Dyck, Morse and Zingales (2013) point out that the sudden demise of the large auditing firm Arthur Andersen (AA) following the Enron debacle provides an exogenous shock to fraud revelation. In October 2001, Enron announced that it had to restate its financial statements for the years 1997 to 2000. AA, as Enron's external auditor, was accused, investigated, and indicted in March 2002 and convicted in June 2002. As a consequence, over the period of 2001-2002, all AA clients had to find new external auditors. The switches happened very quickly. By August 2002, the majority of AA's clients had dismissed AA and engaged new auditors. The sudden fall of AA provides an exogenous shock to fraud revelation among AA's clients because the new auditors have incentives to "clean the house". Dyck, Morse, and Zingales (2010; 2013) report that, as a consequence of the change in auditors, the probability of fraud detection among AA clients increased by about three times in comparison to other firms during the period of 2002-2004.

AA's domestic clients were located in different states. This implies that the effect of the shock to fraud revelation should be different across states: States with a larger fraction of firms that were AA clients during 2001-2002 should have experienced more fraud revelations, which – as we argue in detail below— are exogenous. Thus, we use the fraction of public firms in a state that were AA clients and had to change auditors during 2001-2002 ("*AA Shock*") as an instrument for *Fraud in State* for the period around the shock. In particular, focusing on the period 1994-2005, we set *AA Shock* to zero for the period before 2001 (before the Enron scandal, when there was no exogenous need to change auditors) and equal to the proportion of firms in the state that were AA clients during 2001-2002 for the years after 2002.

Including the period before 2001 allows us to effectively control for other factors that affect *Fraud in State*. In practice, using our instrument, we exploit changes in fraud revelation

across states, which are due to firms that have to necessarily change auditor. This identification strategy is valid if the instruments have enough variation to explain changes in fraud in the state and if the shock has no independent effect on households' decisions to hold stocks. Below we provide evidence supporting the identifying assumptions.

Panel A of Table 3 shows that the instrument is relevant. Column 1 shows that *AA Shock* is positively and significantly related to *Fraud in State* between 1994 and 2005 (coefficient estimate = 0.052, p-value<0.001), after controlling for state and year fixed effects and state economic conditions. The estimate implies that the fraud revelation intensity in states with 27% AA clients (75% of the distribution) increases by about 1.4 percentage points (=0.052*0.27) relative to states with no AA clients in the period after 2001.

To provide evidence that our instrument satisfies the exclusion restriction, we compute pseudo AA shocks using the fraction of firms in a state that were clients of the other four "Big 5" auditing firms during 2001-2002, excluding firms that switched from AA in this period. Eisenberg and Macey (2004) and Agrawal and Chada (2005) have shown that there is no significant difference in the probability of financial restatements between AA clients and the clients of other Big 5 auditing firms before the collapse of AA. However, clients of the other big auditing firms did not need to change auditors during 2001-2002. Columns 2 to 5 in Panel A of Table 3 show that the cross-state differences in the fraction of firms that are clients of the other Big 5 auditing firms do not positively predict cross-state differences in fraud revelation intensity after 2001 indicating that the clients of Big5 firms are not special along some unobserved dimension.

To provide further evidence that our instrument is valid, we examine the correlation between *AA Shock* and the annual fraud revelation intensity in a state each year between 1999

and 2005. We find that *AA Shock* is positively and significantly correlated with the state fraud revelation intensity only in 2001 and 2002. The correlation is statistically insignificant in 1999 and 2000, before the *AA shock*; it peaks in 2002 (0.18), and then becomes both statistically and economically insignificant after 2003. The short-lived nature of the shock suggests that *AA Shock* is unlikely to capture persistent state conditions that are correlated with fraud detection.

Furthermore, Panel B of Table 3 shows that there is no direct relation between the fraction of firms that were AA clients in a state ("*AA Clients*") and household equity participation for the four survey years before 2001. Thus, we find no evidence that *AA shock* captures persistent state-wide conditions that may influence fraud revelation and household stock market participation.

Finally, Panel C of Table 3 reports the IV results. We reproduce the first stage regression showing that the *AA Shock* is positively and significantly related to *Fraud in State* even when we use the data at the frequency of the household survey (instead of using annual frequency as in Panel A). In the second stage regression, *Fraud in State* is significantly negatively related to the local households' probability to invest in equity. This result provides evidence of a causal impact of fraud revelation on household stock market participation.

*3.4 Differences in Fraud Experience across Households*

In our ordinary least squares baseline specification, we attribute identical experiences of corporate scandals to households living in the same state at a given date. However, Malmendier and Nagel (2011a, b) show that economic experiences way into the past affect risk preferences and expectations. If we consider households' experiences way into the past, corporate fraud experiences may differ even for households living in the same state at the same time for two reasons. First, the life cycle of households differs when they are surveyed. Older people in some

states may have experienced more corporate scandals than younger people. Second, some households move across states (about 15% of the households in our sample), which implies that these households' experiences of corporate fraud are not the same as for the households that never moved.

By using all past fraud experiences of a household, we can thus obtain within-state variation in the households' experiences of fraud, which we can exploit to control for state level unobserved factors that could be correlated with frauds and make our estimates spurious.

Before doing so, however, we need to verify that the lifetime experience of corporate frauds is indeed relevant. It is possible that recent experiences have larger impact on current decisions than those early in life. For instance, Malmendier and Nagel (2011a) find that households' "memory" of past stock market returns declines in an almost linear fashion. However, there are also reasons to believe that relatively rare and dramatic events such as corporate frauds will "stick" in people's minds more than general past macroeconomic conditions. If this were true, households' memory decay with respect to corporate fraud could be much slower than what Malmendier and Nagel document for macroeconomic experiences.

Thus, while in our baseline specifications, we simply add up the frauds revealed over the past four years in a state, when we extend the horizon of households' experiences, we need to evaluate empirically whether past memories decay. We follow the approach in Malmendier and Nagel (2011a) to jointly estimate the weighting scheme for past corporate fraud experiences and the effect of past corporate fraud experience on households' incentive to participate in the stock market. Specifically, for each household $i$ in state $j$ in year $t$, we calculate the following weighted average fraud experience:

$$F_{ijt}(\lambda) = \sum_{k=1}^{M_{it}} w_{it}(k,\lambda) Fraud_{j,t-k} \text{ , with } w_{it}(k,\lambda) = \frac{(age_{it} - k)^{\lambda}}{\sum_{k=1}^{age_{it}-1} (age_{it} - k)^{\lambda}}. \quad (2)$$

$Fraud_{j,t-k}$ is the corporate fraud revelation intensity in state *j* in year *t-k*. For households moving across states, we consider the state where household *i* lived in year *t-k* or earlier; $W_{it}(k,\lambda)$ is the weighting function household *i* uses to weigh past fraud experiences, and it only depends on the age of the household head in year *t* ($age_{it}$), the distance in time between a particular past experience and the present (*k*), and the parameter $\lambda$, which captures the speed of "memory decay". If $\lambda = 0$, there is no memory decay and households assign constant weights to fraud experiences across time. If $\lambda > 0$, there is memory decay, and more recent experiences will receive larger weights. The larger $\lambda$ is, the faster is memory decay. In Malmendier and Nagel (2011a), all estimates of $\lambda$ are close to one, which means that the weights decline in a linear fashion. $M_{it}$ is the minimum of ($age_{it}$ -1) and (*t*-1980). We impose this restriction because our fraud revelation data start in 1980, which prevents us from capturing the entire life experience of most households in our sample.

We estimate the following equation:

$$Participation_{ijt} = \alpha + \beta F_{ijt}(\lambda) + \gamma' x_{it} + \eta' z_{jt} + \varepsilon_{it} \quad (3)$$

The main parameters of interest are $\beta$, the sensitivity of the household's stock market participation to experienced corporate fraud, and $\lambda$, the speed of memory decay. We control for household characteristics and state conditions as in our baseline specification. We do not include household fixed effects because this would be too computationally demanding for non-linear least squares and variation in fraud experience across households is important for the

identification once we include interactions of state and year fixed effects. Since $F_{ijt}(\lambda)$ is a nonlinear function of $\lambda$, we use nonlinear least-squares.

Panel A of Table 4 reports the estimates. In column 1, the estimated $\lambda$ is 1.12 and insignificantly different from zero. This suggests that the memory of experienced corporate scandals does not decay, suggesting that corporate scandals, being rarely discovered and dramatic, stick in people's mind. The weighted average of past fraud experience has a significant and negative impact on households' stock market participation. Column 2 shows that results are invariant if we include year fixed effects and state fixed effects for robustness

Given that households in the same state at the same time do not necessarily have identical experiences of corporate scandals, these specifications allow us to control for state-year fixed effects to sharpen our identification of the effect of fraud. State-year fixed effects can capture any unobservable events happening in a state at time *t* that may affect both corporate fraud revelation and households' stock market participation. Thus, the within-state-year variation in households' corporate fraud experience arises solely from the variation in household age and mobility.

In Panel B, we set $\lambda$ equal to zero in order to be able to use ordinary least squares and to control for state-year fixed effects. Since the corporate scandal history goes only back to 1980, and 90% of the household heads in our sample are above 25 years old, there is limited within-state cross-household variation in fraud experience in the early surveys (except for the few households that move across states). We thus include only the last three surveys (2005, 2007, and 2009), for which we can reasonably expect to have within-state variation. The exogenous variation in households' past fraud experience due to age and mobility still negatively and significantly predicts their incentive to participate in the stock market.

In summary, our two identification strategies provide robust evidence of a causal impact of fraud revelation on household stock market participation.

*3.5 Which Households are Most Affected?*

We also examine the potential heterogeneity of the effect in the cross-section of households. Malmendier and Nagel (2011a) find that young people are more affected by their recent stock market experience. To see whether this is also true for corporate scandals, in Panel A of Table 5, we distinguish households into three age groups: young (households whose head is younger than 40), middle-aged (households with head between 40 and 60), and old (household whose head is older than 60). We allow the effect of *Fraud in State* on household stock market participation to be different in these three age groups.

In column 1 of Table 5, the negative effect of fraud revelation tends to be stronger for older households. Although the parameter differences are not statistically significant, the effect of fraud on old households appear significantly larger from an economic point of view if we consider the unconditional probability that each group participates in the stock market (in squared brackets).[7] Old households are approaching retirement and have a shorter investment horizon, and thus could be more sensitive to corporate scandals, which tend to generate severe short-term financial consequences.

One may also wonder whether the effect of fraud revelation we highlight affects to a larger extent naïve households that may be more likely to overreact to news about corporate scandals. We classify households into a less educated group (high school or less) and a more educated group (above high school). The unconditional stock market participation rate is 12% among the less educated, and is 35% among the more educated. Column 2 shows that fraud

[7] In our sample, the unconditional stock market participation rate increases with age and is 27% among the old households, 25% among the middle-aged, and 17% among the young households.

revelation in local companies negatively and significantly affects the participation of both groups. However, if we take into account the unconditional participation rate of each group, then a one-percentage-point increase in fraud revelation leads to a 2% reduction of participation for the less educated (=0.23% / 12%) and a 1.3% reduction for the more educated (=0.46% / 35%), thus confirming that naïve households are more affected.

Corporate scandals can reduce stock market participation either by lowering the propensity of households that currently do not hold stocks to enter the market or by increasing the probability of households that are currently in the stock market to exit. Column 3 shows that the decrease in stock market participation following fraud revelation largely comes from households that held stocks in the previous round of the survey. Among this group of households, a one percentage point increase in fraud revelation intensity corresponds to almost a one percentage point decrease in the probability of investing in stocks.

*3.6 Evidence from Brokerage Data*

Our results so far suggest that the revelation of fraud in local companies reduces households' stock market participation. In this section, we examine whether the effect is mostly due to households that were directly affected by fraud because they held the stocks of fraudulent firms or whether other households are affected as well. We also ask whether households reduce their holdings in non-fraudulent firms. To achieve this, we need information of which stocks households actually hold. Since PSID only reports aggregate households' equity holdings, we use the brokerage data of Barber and Odean (2000), which provides information on households' monthly stockholdings and their trades.

We explore the effects of fraud revelation in a state during the past year on changes in the households' equity holdings in the following year.[8] Table 6 shows that fraud revelation has pervasive negative effects on households' equity holdings in this sample as well. The effect of fraud revelation becomes even stronger after we include household fixed effects (column 2). A one-standard-deviation increase in fraud revelation intensity in a state during a year leads to a 0.4 percentage point decrease in the household's equity holdings. To further assess the economic magnitude of this effect, in column 3 we replace the yearly fraud revelation intensity with a dummy that equals 1 if the state experienced fraud revelation during the previous year. The estimates suggest that households resident in states where fraud has been revealed during the previous year decrease their equity holdings by 1.7 percentage points relative to households in other states. This is a pretty large effect, considering that households with a brokerage account may have a stronger propensity to hold equity than average households.

In column 4, we concentrate on households that did not hold stocks of firms involved in fraud during the previous 12 months and therefore were not directly affected by the fraud episodes. We find that these households also reduce their equity holdings in response to revelation of fraud in local companies. Thus, the sales of equity following fraud revelation do not appear to be exclusively driven by the financial losses experienced by households holding fraudulent stocks.

In column 5, we explore the effect of fraud revelation for households' equity holdings in firms that have *not* been revealed having committed fraud. It appears that households reduce their equity holdings in those firms as well. Finally, column 6 tests weather fraud revelation has a smaller or larger effect on households' investment in non-fraudulent firms relative to investment

[8] Differently from our previous tests using PSID, we here look at yearly changes in equity holdings, rather than level of equity holdings and stock market participation decisions. For this reason, we focus on fraud revelation during the year on the changes in stockholdings in the following year.

in fraudulent firms. In this specification, each year we have at most two observations per household: the change in holdings in fraudulent and non-fraudulent firms, respectively. All households' holdings in fraudulent firms decrease. More interestingly, the negative effect of fraud revelation in a state on firms that have not been revealed as fraudulent is the same as for fraudulent firms. These findings indicate that fraud revelation may cause a spillover affecting all firms, even the ones that did not commit fraud. We explore this possibility in the next section.

## 4. Spillover Effect and the Economic Cost of Corporate Fraud

So far we have shown that corporate scandals in a state have long-lasting negative effects on the equity holdings of households in that state. Since households are more likely to hold local stocks, corporate scandals in a state may lead to a lasting decrease in demand for *all* local stocks. The negative demand shocks due to fraud could therefore lead to an increase in cost of capital for local firms that did not commit (or were not found out having committed) fraud.

In this section, we exclude fraudulent firms and focus on firms that are not revealed having committed fraud. We explore whether the non-fraudulent firms indeed experience a decrease in their demand for equity and evaluate the consequences for their cost of capital.

Since high local demand for equity increases the valuation of local firms (Hong, Kubik and Stein, 2008), the consequences of a decrease in local demand for equity may potentially be large. In particular, we expect that the stock prices of firms in states hit by corporate scandals will have to decrease and the firms' expected returns to increase to attract institutional investors and distant households. The magnitude of the decrease –and of the externality on firms that are not revealed to have committed fraud—depends on the extent of market segmentation, which determines how easy it is to attract other investors and replace the local households. It is

ultimately an empirical question whether institutional investors and distant households would substitute local households with limited implications on stock prices and returns following fraud revelation in local firms.

*4.1 Changes in the Number of Shareholders*

If there is an overall decrease in the demand for stocks of firms located in states where the frauds are revealed, then we would expect to find a decrease in a firm's number of shareholders, particularly retail shareholders, following an increase in fraud revelations in the state. To test this hypothesis, we construct an indicator variable, "*Big Decrease in # of Shareholders*", that equals one if the percentage change in the number of shareholders of a firm is in the bottom quartile of the sample distribution (below –7%).[9]

Panel A of Table 7 estimates linear probability models for the likelihood that a firm experiences a large decrease in the number of shareholders.[10] Besides including our main variable of interest *Yearly Fraud in State*, we control for the firm's market capitalization, market-to-book ratio, and return volatility. Also, following Hong et al. (2008), we control for the ratio of state personal income to total corporate book assets, as a firm's ability to attract local shareholders depend on the local income and the supply of other local firms.

Column 1 shows that *Yearly Fraud in State* is associated with large decreases in the number of shareholders. The effect is both statistically and economically significant. The parameter estimate of *Yearly Fraud in State* is 0.72, which means that a one-standard-deviation

[9] Compustat reports only information on the number of shareholders on record, not the actual number of beneficiary shareholders. For this reason, we do not use a continuous measure of the change in the number of shareholders and focus on dramatic changes in the shareholder base.

[10] All our results are robust if we use a probit model. But since our variable of interest has many zeros and we include a number of dummies in the model, the probit model algorithm in some instances fails to converge.

increase in fraud revelation intensity in the state increases the probability of a non-fraudulent local firm experiencing a large decline in its shareholder base by 2 percent.[11]

We expect the decrease in the number of shareholders to mainly come from a decrease in the number of retail shareholders. Thus, we obtain information on the number of institutional shareholders and institutional ownership from the Thomson Financial 13F and compute the number of retail investors by subtracting the number of institutional owners from the number of shareholders. "*Big Decrease in # of Retail Shareholders*" ("*Big Decrease in # of Inst. Shareholders*") indicates that the firm experiences a large decrease in the number of retail (institutional) shareholders. We find that *Yearly Fraud in State* indeed increases the likelihood of a large decrease in the number of retail shareholders (column 2), even after controlling for the contemporaneous change in institutional ownership ("*Inst. Own. Growth*"). In contrast, in column 3, *Yearly Fraud in State* does not predict a large decrease in institutional investors' shareholdings.

The stock market participation of households who predominantly hold local stocks could be affected by corporate scandals not only because fraud revelation affects their preference for equity, but also because market participants update their beliefs about the probability of frauds in other firms and thus expectations about future stock returns. Goldman, Peyer and Stefanescu (2012) and Gleason, Jenkins, and Johnson (2008) show that these contagion effects based on information spillovers affect predominantly firms in the same industries as the fraudulent firms. Thus, we check whether the geographic spillover that we document is driven by industry information spillovers. We define "*Yearly Fraud in Industry*" as the intensity of fraud revelation

[11] This value is obtained by multiplying the coefficient estimate of 0.72 times the standard deviation of *Yearly Fraud in State* (0.423%) and dividing by the ex ante probability of a large drop in the number of shareholders, which is 0.15

in each 3-digit SIC industry during the past year, which is calculated as the number of revealed frauds scaled by the number of public firms in that industry.

Column 4 shows that both revealed frauds in the same state and in the same industry predict a large decrease in the number of retail shareholders of a firm. The effect of *Yearly Fraud in State* (0.707) is comparable to and even larger than the effect of *Yearly Fraud in Industry* (0.453). Importantly, the magnitude of the effect of *Yearly Fraud in State* remains largely unchanged after controlling for *Yearly Fraud in Industry,* suggesting that the geographical spillover effect that we highlight is independent of the information spillover identified in previous studies.

Column 5 shows that the number of institutional owners of a firm decreases following revelation of fraud in a firm's industry, but not following revelation of fraud in a firm's state. This suggests that fraud revelation in a firm's state is unlikely to convey valuable information about that the firm's probability of committing fraud or expected returns related to the state economic conditions as also institutional investors' demand for stocks should have decreased otherwise. We provide further support for this argument below, using exogenous changes in fraud revelation.

*4.2 Cost of Capital*

The decrease in the number of shareholders and, more generally, in the local demand for the stocks of firms located in the states where corporate frauds occur imply that firm returns have to increase for the market of the firm's stocks to clear. The logic is similar to the one in Hong, Kubik and Stein (2008). Since shareholders have a preference for local stocks, after a decrease in local demand, distant shareholders have to be compensated with higher returns for the market to

clear. Panel B of Table 7 explores whether a non-fraudulent firm's cost of capital increases in the years following the revelation of corporate frauds in the state.

We start by estimating expected returns for each firm and year from a Fama-French three-factor model using daily returns from 1981 to 2010. We use the firm's alpha as a proxy for the excess return that a firm must offer to shareholders in a given year. We use the firm's alpha as our measure of the firm's cost of capital and relate it to the intensity of fraud revelation in the firm's state during the past year. The results are reported in the first two columns in Panel B of Table 7. Fraud revelation in other local firms during the last year significantly increases the firm's cost of capital.

However, it is possible that fraud revelation in some local companies signals that other firms in the state are more likely to have committed fraud. In column 2, we control for *Yearly Fraud in Industry* to take into account the possibility of an information spillover effect. The effect of *Yearly Fraud in State* is largely independent of that of *Yearly Fraud in Industry*. This result reinforces our conclusion from Panel A that the geographic spillover effect associated with *Yearly Fraud in State* is unlikely to operate through an information spillover. The effect is also economically significant. A one-standard deviation increase in *Yearly Fraud in State* on average increases a non-fraudulent firm's cost of capital by about 20 basis points (=0.58*0.003), which is equivalent to 0.5% higher return per year.

Presumably, a firm's cost of capital should increase to a larger extent in the months immediately following fraud revelation in the state, when firms have to attract new shareholders. To be able to explore the dynamic effects of fraud revelation on firm's cost of capital, we also use firm monthly returns and estimate a factor model with time-varying alpha that depends on past fraud revelation in a state. Specifically, we do a panel regression of the firm's monthly

returns on the past fraud revelation intensity in the state, controlling for the three Fama-French factors and the momentum factor. We examine the impact of fraud revelation in the state during the previous 6, 12, and 48 months. The estimates reported in Columns 3 to 5 clearly show that the effect of fraud revelation is largest in the 6 months following fraud revelation in the state; it decreases when we consider frauds revealed during the previous 12 months, and becomes statistically insignificant when we consider a measure of fraud revealed during the previous 48 months. Most importantly, both approaches clearly indicate that non-fraudulent firms experience an increase in their cost of capital following fraud revelation in the state.

*4.3 IV Estimation*

Concerns may arise that fraud revelation in a state is related to changes in firm characteristics and market conditions in that state that could independently affect firms' cost of capital. To address this concern, we use the IV approach described in Section 3.3. That is, we use *AA Shock* as an instrument for *Yearly Fraud in State* during the period around the shock (1999-2005). Panel C of Table 7 reports the second-stage results. We find that the exogenous variation in *Yearly Fraud in State* due to the AA shock leads to a higher probability that non-fraudulent firms in states hit by corporate scandals experience a large decrease in the number of shareholders (column 1), a large decrease in the number of retail shareholders (column 2), and a higher cost of capital (column 3). These results suggest that the effect of *Yearly Fraud in State* on non-fraudulent firms is causal and unrelated to factors affecting those firms' future performance.

If local corporate scandals lead to a persistent decrease in the local demand for equity, which in turn increases all local firms' cost of capital, then we expect scandals to also affect local firms' valuation. In column 4, we report the second-stage results from the same IV estimation for

the industry-adjusted market-to-book ratio. Following Pastor and Veronesi (2003), we control for the following firm characteristics: Return on equity ("ROE"), R&D expenditures divided by sales ("RD/Sale"), an indicator variable for missing R&D value ("Missing RD"), and "AGE" (=1-1/(1+firm age)). Consistent with our earlier findings, we find that *Yearly Fraud in State* decreases non-fraudulent firms' valuation. The estimate suggests that the industry-adjusted market-to-book ratio of firms in states in which fraud revelation increases by one standard deviation during the past year experience a decrease in valuation of 0.89 (=223*0.004), which is approximately 20% of the average industry-adjusted market to book ratio.

## 5. Conclusion

This paper shows that the revelation of corporate frauds in a state decreases the stock market participation of households that reside in that state and are thus more exposed to fraud not only because of their equity holdings, but in general because of their proximity. Importantly, by estimating a differential effect of households' exposure to fraud, we only identify a lower bound of the negative effects of corporate frauds on the demand for equity. Presumably, all households are affected by cases of corporate frauds with high national news coverage and non-local ownership. Thus, the magnitude of the effects of corporate fraud may be much larger.

Since the effects of the revelation of corporate fraud on household stock market participation we document are obtained controlling for possible channels leading to changes in state economic conditions or informational spillovers on local firms, it appears that households' preferences for holding stocks or their expectations on the probability of future frauds may be affected for behavioral reasons.

Importantly, the decrease in the demand for equity that we document generates a negative spillover effect on other firms located in the same state as the firms committing fraud. Thus, fraud revelation in some firms ends up increasing the cost of capital for all firms in the state.

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## Appendix: Variable Definitions

| | |
|---|---|
| Equity Participation | An indicator variable that equals 1 if the household holds any shares in publicly held corporations, mutual funds, or investment trusts in a given year. |
| Equity Participation (IRA) | An indicator variable that equals 1 if the household holds any shares in publicly held corporations, mutual funds, or investment trusts in a given year, including holdings in pensions or individual retirement accounts. |
| Equity Value | Dollar value of equity investment. |
| Net Equity Purchase | The net dollar value of *new* equity investment in a year. |
| Equity-Wealth Ratio | The ratio of equity value to the household's total wealth. |
| Age | The age of the household's head. |
| Married | An indicator variable that equals 1 if the household's head is married. |
| Family Size | The number of family members in a given year. |
| Family Income | The total dollar value of family income. |
| Wealth | The total dollar value of family net wealth, excluding the value in equity investment. |
| Male | An indicator variable that equals 1 if the household's head is male. |
| Years in School | The household's head years of education. |
| Fraud in State | The sum of the yearly fraud revelation intensity in the past four years in a state. The yearly fraud revelation intensity is the number of frauds revealed divided by the number of publicly traded companies in a state-year. |
| Fraud in State 2 | The cumulative market-share-augmented fraud revelation intensity in the past four years in a state. The market share is the firm's book assets divided by the total book assets of all public companies in the state. |
| Fraud in State 3 | The cumulative market-penalty-augmented fraud revelation intensity in the past four years in a state. The market penalty is the cumulative market reaction across all key fraud revelation events associated with a case. |
| Fraud in State 4 | The cumulative market-retail-ownership-augmented fraud revelation intensity in the past four years in a state. The retail ownership is (1-percentage institutional ownership). |
| Fraud In Industry | The sum of the yearly fraud revelation intensity in the past four years in a three-digit SIC code industry. The yearly fraud revelation intensity is the number of frauds revealed in an industry-year divided by the number of publicly traded companies in that industry. |
| # of Firms | The number of publicly traded companies in a state-year. |
| State GDP Growth | The average annual GDP growth rate in the past four years in a state. |
| State Stock Return | The buy-and-hold value-weighted stock market return in the past four years in a state. |
| Personal Income / Corp. Assets | The state total personal income divided by the total book value of assets of publicly traded companies in the state. |
| AA Shock | The fraction of public firms in a state that were Arthur Andersen clients and had to change auditors during 2001-2002. |

| | |
|---|---|
| AA Clients | The fraction of public firms in a state that were Arthur Andersen clients in a given year (before 2001). |
| Log(MVE) | Logarithm of the firm's market value of equity. |
| Log(M/B) | Logarithm of the firm's market-to-book ratio of equity. |
| Ind-adj. M/B | The firm's M/B ratio adjusted for the industry-year median. |
| Return Vol. | The standard deviation of the firm's daily stock returns in a year. |
| Alpha | The alpha in the Fama-French 3-factor model estimated using daily returns for each firm and year. |
| ROE | Return on equity, defined as earnings over lagged book value of equity. Earnings are the sum of income before extraordinary items, deferred income taxes, and investment tax credit. |
| RD/Sale | R&D expenditures divided by net sales. |
| Missing RD | An indicator variable that equals 1 if the R&D expenditures are not reported. |
| Age | =1-1/(1+IPO), where IPO is the number of years since IPO. |
| Big Decrease in # of Shareholders | Indicator variable that equals 1 if the % change in the number of shareholders is in the bottom quartile of the distribution (< -7%). |
| Big Decrease in # of Retail Shareholders | Indicator variable that equals 1 if the % change in the number of retail shareholders is in the bottom quartile of the distribution. |
| Big Decrease in # of Inst. Shareholders | Indicator variable that equals 1 if the % change in the number of institutional shareholders is in the bottom quartile of the distribution. |
| Inst. Own. Growth | The growth rate of percentage institutional ownership. |

## Table 1: Summary Statistics

*Panel A: Household Data*

This table presents the main household characteristics. The unit of observation is the household-year. All variable definitions are in the Appendix.

| Variable | N | Mean | S.D. | 25th p. | Median | 75th p. |
|---|---|---|---|---|---|---|
| Equity Participation | 66615 | 0.218 | 0.413 | 0 | 0 | 0 |
| Equity Participation (IRA) | 66615 | 0.298 | 0.457 | 0 | 0 | 1 |
| Equity Value | 66574 | 24,203 | 153,424 | 0 | 0 | 0 |
| Net Equity Purchase | 65540 | 7,719 | 87,441 | 0 | 0 | 0 |
| Equity-Wealth Ratio | 66556 | 0.043 | 0.134 | 0 | 0 | 0 |
| Age (household head) | 66615 | 45.07 | 16.20 | 32 | 42 | 55 |
| Married | 66615 | 0.554 | 0.497 | 0 | 1 | 1 |
| Family Size | 66615 | 2.730 | 1.497 | 2 | 2 | 4 |
| Family Income (in thousands) | 66115 | 54.1 | 78.4 | 19.4 | 38.1 | 67.6 |
| Wealth (excl. equity, in thousands) | 66594 | 130.9 | 929.3 | 0.3 | 10.2 | 60 |
| Years in School | 64720 | 12.734 | 2.766 | 12 | 12 | 15 |

*Panel B: Distribution of Fraud Revelation by State*

| State | # of Frauds | State | # of Frauds | State | # of Frauds | State | # of Frauds | State | # of Frauds | State | # of Frauds |
|---|---|---|---|---|---|---|---|---|---|---|---|
| AL | 5 | FL | 68 | LA | 2 | NC | 8 | OK | 5 | TX | 71 |
| AR | 1 | GA | 22 | MA | 29 | NE | 2 | OR | 3 | UT | 12 |
| AZ | 10 | IA | 1 | MD | 8 | NH | 2 | PA | 24 | VA | 14 |
| CA | 127 | ID | 1 | MI | 12 | NJ | 29 | PR | 4 | WA | 6 |
| CO | 16 | IL | 19 | MN | 13 | NM | 2 | RI | 1 | WI | 3 |
| CT | 18 | IN | 8 | MO | 8 | NV | 13 | SC | 5 | WV | 1 |
| DC | 2 | KS | 10 | MS | 2 | NY | 84 | SD | 3 | WY | 1 |
| DE | 2 | KY | 1 | MT | 1 | OH | 25 | TN | 6 | | |

| Year | # of Frauds | Year | # of Frauds | Year | # of Frauds | Year | # of Frauds | Year | # of Frauds |
|---|---|---|---|---|---|---|---|---|---|
| 1980 | 4 | 1986 | 12 | 1992 | 24 | 1998 | 29 | 2004 | 27 |
| 1981 | 6 | 1987 | 23 | 1993 | 27 | 1999 | 29 | 2005 | 40 |
| 1982 | 11 | 1988 | 14 | 1994 | 40 | 2000 | 51 | 2006 | 42 |
| 1983 | 12 | 1989 | 12 | 1995 | 26 | 2001 | 38 | 2007 | 17 |
| 1984 | 17 | 1990 | 16 | 1996 | 32 | 2002 | 59 | 2008 | 9 |
| 1985 | 11 | 1991 | 21 | 1997 | 23 | 2003 | 25 | 2009 | 11 |

*Panel C: Cumulative Fraud Revelation Intensity*

"Cumulative # of Frauds" is the total number of frauds revealed in a state in the past four years. We also present the alternative proxies for fraud revelation intensity in the state over the past four years, defined as described in the Appendix. "Fraud in Industry" measures fraud revelation intensity in a 3-digit SIC code industry in the past four years.

| Variable | N | Mean | S.D. |
|---|---|---|---|
| Cumulative # of Frauds | 1402 | 2.430 | 0.048 |
| Fraud in State | 1402 | 0.010 | 0.024 |
| Fraud in State 2 | 1402 | 0.011 | 0.025 |
| Fraud in State 3 | 1402 | 0.013 | 0.029 |
| Fraud in State 4 | 1402 | 0.019 | 0.044 |
| Fraud in Industry | 7975 | 0.012 | 0.046 |

*Panel D: State and Firm Level Control Variables*

This table presents descriptive statistics for the main variables. The unit of observation is the state-year for state level variables, and firm-year for firm level variables.

| Variable | N | Mean | S.D. | $25^{th}$ p. | Median | $75^{th}$ p. |
|---|---|---|---|---|---|---|
| # of Firms | 1558 | 168 | 249 | 23 | 72 | 192 |
| State GDP Growth | 1528 | 0.068 | 0.031 | 0.048 | 0.063 | 0.085 |
| State Stock Return | 1350 | 0.194 | 0.403 | 0.073 | 0.183 | 0.414 |
| Personal Income / Corp. Assets | 1528 | 4.045 | 33.765 | 0.463 | 0.867 | 1.655 |
| Alpha (%) | 145065 | 0.047 | 0.270 | -0.062 | 0.025 | 0.129 |
| Big Decrease in # of Shareholders | 231469 | 0.153 | 0.360 | 0 | 0 | 0 |
| Big Decrease in # of Retail Shareholders | 231469 | 0.096 | 0.294 | 0 | 0 | 0 |
| Big Decrease in # of Inst. Shareholders | 231469 | 0.118 | 0.322 | 0 | 0 | 0 |
| Growth in # of Shareholders | 148643 | 0.264 | 1.609 | -0.068 | -0.009 | 0.053 |
| Inst. Own. Growth | 108466 | 0.355 | 1.510 | -0.077 | 0.037 | 0.240 |
| Log(MVE) | 182831 | 4.688 | 2.081 | 3.193 | 4.558 | 6.059 |
| Log(Assets) | 195454 | 5.130 | 2.309 | 3.386 | 5.015 | 6.731 |
| Log(M/B) | 165999 | 0.590 | 0.870 | 0.032 | 0.517 | 1.079 |
| Ind-adj. M/B | 165999 | 0.989 | 4.487 | -0.555 | 0 | 0.981 |
| Return Vol. | 161745 | 3.286 | 2.330 | 1.744 | 2.708 | 4.167 |
| ROE | 168599 | 0.022 | 0.437 | -0.040 | 0.098 | 0.186 |
| RD/Sale | 191311 | 0.102 | 0.491 | 0 | 0 | 0.017 |
| Missing RD | 231469 | 0.629 | 0.483 | 0 | 1 | 1 |
| Age | 200927 | 0.814 | 0.216 | 0.750 | 0.900 | 0.947 |

## Table 2: Fraud Revelation in a State and Household Stock Market Participation

The dependent variable is "Equity Participation" in all columns of Panel A but column 4; in column 4 of Panel A the dependent variable is equity market participation (IRA). In Panel B, the dependent variable is indicated on top of each column. In column 3, we exclude observations from the 1984 and 1989 surveys, for which we are unable to separate any equity held in the IRA. The sample period is 1984-2009. All variables are defined in the Appendix. Parameter estimates are obtained by ordinary least squares. All regressions include a constant term, which we do not report. Standard errors are clustered by household and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively. The numbers in square brackets are estimated coefficients on "Fraud in State" variables, standardized to make the coefficients comparable. The standardization is done by subtracting the sample mean from the variable and by dividing by 100 times the sample standard deviation.

*Panel A: Probability of Participation*

| Dependent Variable: Equity Participation | (1) | (2) | (3) Excl. 1984 & 1989 | (4) With IRA | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Fraud in State | -0.289** | -0.363** | -0.386** | -0.327*** | | | |
| | (0.146) | (0.165) | (0.173) | (0.119) | | | |
| | | [-0.851] | | | | | |
| Fraud in State 2 | | | | | -0.357** | | |
| | | | | | (0.161) | | |
| | | | | | [-0.901] | | |
| Fraud in State 3 | | | | | | -0.291** | |
| | | | | | | (0.135) | |
| | | | | | | [-0.859] | |
| Fraud in State 4 | | | | | | | -0.194** |
| | | | | | | | (0.090) |
| | | | | | | | [-0.849] |
| Log(Age) | 0.091*** | 0.345*** | 0.289*** | 0.389*** | 0.345*** | 0.345*** | 0.345*** |
| | (0.006) | (0.041) | (0.056) | (0.038) | (0.041) | (0.041) | (0.041) |
| Married | 0.059*** | 0.017** | 0.006 | 0.016** | 0.017** | 0.017** | 0.017** |
| | (0.005) | (0.007) | (0.008) | (0.006) | (0.007) | (0.007) | (0.007) |
| Log(Family Size) | -0.048*** | 0.004 | 0.000 | -0.003 | 0.004 | 0.004 | 0.004 |
| | (0.004) | (0.005) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) |
| Log(Family Income) | 0.032*** | 0.009*** | 0.007*** | 0.007*** | 0.009*** | 0.009*** | 0.009*** |
| | (0.002) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Log(Wealth) | 0.021*** | 0.010*** | 0.009*** | 0.009*** | 0.010*** | 0.010*** | 0.010*** |
| | (0.000) | (0.000) | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) |
| State GDP Growth | 0.098 | 0.245** | 0.238 | 0.162 | 0.245** | 0.246** | 0.245** |
| | (0.109) | (0.124) | (0.175) | (0.111) | (0.124) | (0.124) | (0.124) |

| Dependent Variable: Equity Participation | (1) | (2) | (3) Excl. 1984 & 1989 | (4) With IRA | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| State Stock Return | 0.005 | 0.004 | 0.003 | 0.007 | 0.004 | 0.004 | 0.004 |
| | (0.005) | (0.005) | (0.006) | (0.004) | (0.005) | (0.005) | (0.005) |
| Years in School | 0.032*** | | | | | | |
| | (0.001) | | | | | | |
| Household F.E. | | x | x | x | x | x | x |
| Year F.E. | x | x | x | x | x | x | x |
| State F.E. | x | x | x | x | x | x | x |
| Observations | 64,192 | 66,085 | 53,974 | 66,085 | 66,085 | 66,085 | 66,085 |
| R-squared | 0.233 | 0.609 | 0.645 | 0.760 | 0.609 | 0.609 | 0.609 |

*Panel B: Level of Participation*

| | (1) Log(Equity Value) | (2) Log(Equity Value) (excl. 1984 & 1989) | (3) Log(Equity Value) (with IRA) | (4) Log(Net Equity Purchase) | (5) Equity-Wealth Ratio |
|---|---|---|---|---|---|
| Fraud in State | -3.895** | -4.714** | -3.918* | -2.659** | -0.112** |
| | (1.912) | (2.159) | (2.038) | (1.152) | (0.055) |
| Log(Age) | 1.698*** | 1.878*** | -4.558*** | 2.227*** | 0.025 |
| | (0.497) | (0.676) | (0.521) | (0.290) | (0.016) |
| Married | 0.138 | -0.000 | 0.034 | 0.044 | 0.000 |
| | (0.085) | (0.098) | (0.081) | (0.056) | (0.003) |
| Log(Family Size) | 0.164** | 0.163** | 0.303*** | 0.024 | 0.001 |
| | (0.064) | (0.076) | (0.062) | (0.041) | (0.002) |
| Log(Family Income) | 0.072*** | 0.052*** | 0.027* | 0.040*** | 0.002*** |
| | (0.016) | (0.018) | (0.015) | (0.011) | (0.000) |
| Log(Wealth) | 0.139*** | 0.130*** | 0.090*** | 0.026*** | 0.003*** |
| | (0.006) | (0.006) | (0.005) | (0.003) | (0.000) |
| State GDP Growth | 0.670 | 2.292 | 0.888 | 1.505 | -0.035 |
| | (1.455) | (2.213) | (1.225) | (0.978) | (0.043) |
| State Stock Return | 0.061 | -0.005 | 0.044 | 0.038 | 0.004** |
| | (0.091) | (0.099) | (0.088) | (0.046) | (0.002) |
| Household F.E. | x | x | x | x | x |
| Year F.E. | x | x | x | x | x |
| State F.E. | x | x | x | x | x |
| Observations | 66,045 | 53,963 | 66,082 | 65,013 | 66,048 |
| R-squared | 0.579 | 0.612 | 0.583 | 0.325 | 0.471 |

## Table 3: Identification Through an Exogenous Shock to Fraud Revelation

*Panel A: Validity of the Instrument*

The dependent variable is "Fraud in State", the measure of the intensity of fraud revelation in the past four years in a state. The sample period is 1994-2005. "AA Shock" is the fraction of public firms in a state that were AA clients and had to change auditors during 2001-2002. We set the value of AA Shock to zero for years before 2001, and at the 2001-2002 value for the years after 2001. The pseudo instrument (XX) is created in the same way except that we use the fraction of public firms that were clients of the auditing firm XX during 2001-2002. Parameter estimates are obtained by ordinary least squares. All regressions include a constant term, which we do not report. Standard errors are clustered by year and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

| | Fraud in State | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| AA Shock | 0.052*** | | | | |
| | (0.013) | | | | |
| Pseudo Instrument | | 0.002 | | | |
| (Deloitte & Touche) | | (0.003) | | | |
| Pseudo Instrument | | | -0.008 | | |
| (Ernst & Young) | | | (0.005) | | |
| Pseudo Instrument | | | | -0.029** | |
| (KPMG) | | | | (0.009) | |
| Pseudo Instrument | | | | | -0.010 |
| (PWC) | | | | | (0.021) |
| State GDP Growth | -0.057 | -0.059 | -0.057 | -0.058 | -0.056 |
| | (0.060) | (0.062) | (0.063) | (0.062) | (0.065) |
| State Stock Return | -0.000 | -0.000 | -0.000 | -0.001 | -0.000 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Year F.E. | x | x | x | x | x |
| Observations | 612 | 612 | 612 | 612 | 612 |
| R-squared | 0.254 | 0.240 | 0.241 | 0.242 | 0.240 |

*Panel B: Exclusion Restriction*

The dependent variable is "Equity Participation", an indicator variable that equals one if the household holds equity in a given year. "AA Clients" is the fraction of public firms in a state that are Arthur Andersen clients in a given year. The sample period includes all survey years before 2001. Parameter estimates are obtained by ordinary least squares. All regressions include a constant term, which we do not report. Standard errors are clustered by household and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

| | Equity Participation (before 2001) |
|---|---|
| AA Clients | 0.031 |
| | (0.151) |
| Log(Age) | 0.443*** |
| | (0.062) |
| Married | 0.034** |
| | (0.014) |
| Log(Family Size) | 0.014 |
| | (0.009) |
| Log(Family Income) | 0.008*** |
| | (0.002) |
| Log(Wealth) | 0.014*** |
| | (0.001) |
| State GDP Growth | 0.052 |
| | (0.155) |
| State Stock Return | -0.017 |
| | (0.011) |
| Household F.E. | x |
| Year F.E. | x |
| State F.E. | x |
| Observations | 26,218 |
| R-squared | 0.481 |

*Panel C: IV Regression*

We present 2SLS estimates for household equity participation. All variables are defined in the Appendix. The sample period is 1994-2005. All regressions include a constant term, which we do not report. Standard errors are clustered by household and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

| | First Stage<br>Fraud in State | Second Stage<br>Equity Participation |
|---|---|---|
| *Instrumental Variable* | | |
| AA Shock | 0.030*** | |
| | (0.002) | |
| *Endogenous Variable* | | |
| Fraud in State | | -8.845*** |
| | | (0.779) |
| *Control Variables* | | |
| Log(Age) | 0.001 | 0.047** |
| | (0.001) | (0.023) |
| Married | 0.000 | 0.003 |
| | (0.000) | (0.010) |
| Log(Family Size) | -0.000 | 0.011 |
| | (0.000) | (0.008) |
| Log(Family Income) | -0.000 | 0.007*** |
| | (0.000) | (0.002) |
| Log(Wealth) | -0.000 | 0.010*** |
| | (0.000) | (0.001) |
| State GDP Growth | 0.085*** | 0.971*** |
| | (0.012) | (0.196) |
| Household F.E. | X | x |
| Year F.E. | X | x |
| Observations | 37,579 | 37,579 |

## Table 4: Identification Through Cross-Households Differences in Fraud Experience

*Panel A: Nonlinear Estimation of Experienced Frauds and Household Participation*

The dependent variable is "Equity Participation", an indicator variable that equals one if the household holds equity in a given year. "Experienced Fraud" is the weighted average of a household's fraud experience, $F(\lambda)$, as defined in equation (2). $\lambda$ is the parameter defining the shape of the weighting function as specified in equation (2). The sample period is 1984-2009. All variables are defined in the Appendix. Estimates are obtained by non-linear least squares. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

| | Equity Participation | |
|---|---|---|
| | (1) | (2) |
| Experienced Fraud | -4.580*** | -3.003** |
| | (1.081) | (1.195) |
| Weighting parameter $\lambda$ | 1.117 | 2.013 |
| | (1.247) | (2.593) |
| Log(Age) | 0.035*** | 0.036*** |
| | (0.004) | (0.004) |
| Married | 0.070** | 0.061*** |
| | (0.004) | (0.004) |
| Log(Family Size) | -0.075*** | -0.070*** |
| | (0.003) | (0.003) |
| Log(Family Income) | 0.053*** | 0.053*** |
| | (0.001) | (0.001) |
| Log(Wealth) | 0.025*** | 0.024*** |
| | (0.0004) | (0.0004) |
| State GDP Growth | 0.643*** | -0.005 |
| | (0.076) | (0.116) |
| Year F.E. | | x |
| State F.E. | | x |
| Observations | 66,085 | 66,085 |
| R-squared | 0.171 | 0.156 |

*Panel B: Controlling for State-Year Fixed Effects*

The sample period includes only surveys between 2005 and 2009 because within state variation in experienced fraud is minimal in the surveys before 2005 (see discussion in Section 3.3). "Experienced Fraud" is the weighted average of a household's fraud experience, with the weighting parameter λ set to be zero. Parameter estimates are obtained by ordinary least squares. All variables are defined in the Appendix. All regressions include a constant term and interaction of state and year fixed effects, which we do not report. Standard errors are clustered by household and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

| | Equity Participation (2005-2009) |
|---|---|
| Experienced Fraud (with λ=0) | -14.463* |
| | (8.482) |
| Log(Age) | 0.001*** |
| | (0.000) |
| Married | 0.058*** |
| | (0.005) |
| Log(Family Size) | -0.072*** |
| | (0.004) |
| Log(Family Income) | 0.046*** |
| | (0.002) |
| Log(Wealth) | 0.021*** |
| | (0.000) |
| State-year F.E. | x |
| Observations | 24,525 |
| R-squared | 0.146 |

## Table 5: Which Households are More Affected?

The dependent variable "Equity Participation" indicates whether the household holds equity in a given year. "Young" is a dummy variable that equals 1 if the household head is less than 40 years old, "MiddleAged" is a dummy variable that equals 1 if the household head is between 40 and 60, and "Old" is a dummy variable that equals 1 if the household head is above 60. "LessEducated" is a dummy variable that equals 1 if the household head has no more than 12 years of schooling. "MoreEducated" is a dummy variable that equals 1 if the household head has more than 12 years of schooling. $I_{\{Participation(t-1)=1\}}=1$ is an indicator variable that equals 1 if the household held equity in the previous survey round. The sample period is 1984-2009. All variables are defined in the Appendix. Parameter estimates are obtained by ordinary least squares. All regressions include a constant term, which we do not report. Standard errors are clustered by household and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively. The numbers in the square brackets are the coefficient estimates divided by the proportion of equity holders in that group, and can be viewed as the estimated percentage change in the probability of participation for that group.

| | Equity Participation | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Fraud in State *Young | -0.194 | | |
| | (0.194) | | |
| | [-1.14%] | | |
| Fraud in State *MiddleAged | -0.372** | | |
| | (0.189) | | |
| | [-1.50%] | | |
| Fraud in State *Old | -0.631** | | |
| | (0.298) | | |
| | [-2.34%] | | |
| Fraud in State *LessEducated | | -0.229** | |
| | | (0.105) | |
| | | [-1.92%] | |
| Fraud in State *MoreEducated | | -0.458** | |
| | | (0.210) | |
| | | [-1.31%] | |
| Fraud in State | | | -0.112 |
| | | | (0.189) |
| Fraud in State $*I_{\{Participation(t-1)=1\}}$ | | | -0.860** |
| | | | (0.378) |
| Log(Age) | 0.337*** | 0.345*** | 0.245*** |
| | (0.034) | (0.048) | (0.067) |
| Married | 0.017*** | 0.017* | 0.006 |
| | (0.006) | (0.009) | (0.010) |
| Log(Family Size) | 0.004 | 0.004 | 0.003 |
| | (0.004) | (0.004) | (0.007) |
| Log(Family Income) | 0.009*** | 0.009*** | 0.007*** |
| | (0.001) | (0.001) | (0.002) |
| Log(Wealth) | 0.010*** | 0.010*** | 0.010*** |
| | (0.000) | (0.001) | (0.001) |
| State GDP Growth | 0.243** | 0.242** | 0.345** |
| | (0.106) | (0.080) | (0.164) |
| State Stock Return | 0.004 | 0.004 | 0.002 |
| | (0.005) | (0.006) | (0.006) |
| Household F.E. | x | x | x |
| Year F.E. | x | x | x |
| State F.E. | x | x | x |
| Observations | 66,085 | 66,085 | 52,257 |
| R-squared | 0.609 | 0.609 | 0.652 |

## Table 6: Fraud in State and Households' Change in Equity Position

This table reports the estimated effects of frauds on changes in households' equity holdings using brokerage data from Barber and Odean (2000). "Change in Equity Holdings" is the ratio of the sum of price-weighted changes in shareholdings in a year scaled by the value of the households' equity holdings at the beginning of the year. The value of all positions is computed using share prices at the beginning of the year. "Yearly Fraud in State" is the fraud revelation intensity in the past 12 months in a state. "Fraud in State Dummy" is a dummy that takes a value of 1 if any fraud has been revealed in a state during the last year, and equals zero otherwise. "Fraudulent Stock" indicates whether a stock was involved in fraud in the past 12 months. "Portfolio Return" is the raw return to the investor's equity portfolio in the past year. "State GDP Growth" is the GDP growth rate of the investor's state of residence. "Age" is the age of the investor. "Married" indicates whether the investor is married or not. "Family Size" is the total number of people in the investor's household. Model (4) excludes investors that hold any stock(s) that are involved in fraud in the past 12 months. Model (5) excludes stocks of firms that have been revealed having committed fraud during the past 12 months (fraudulent stock) in the calculation of change in equity holdings. Model (6) considers changes in equity holdings in at most two portfolios for each household: the portfolios of fraudulent and non-fraudulent stocks, respectively.

| Dependent Var.: Change in Equity Holdings | (1) | (2) | (3) | (4) Excluding investors w/ fraudulent stocks | (5) Excluding fraudulent stocks | (6) |
|---|---|---|---|---|---|---|
| Yearly Fraud in State | -0.746*** | -0.980** | | -0.733** | -0.745*** | -0.739*** |
| | (0.283) | (0.404) | | (0.288) | (0.284) | (0.284) |
| Fraud in State Dummy | | | -0.017*** | | | |
| | | | (0.004) | | | |
| Fraudulent Stock | | | | | | -0.089*** |
| | | | | | | (0.004) |
| Fraud in State x Fraudulent Stock | | | | | | 0.978 |
| | | | | | | (0.876) |
| Portfolio Return | 0.000 | 0.001* | 0.001* | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.001) | (0.001) | (0.000) | (0.000) | (0.000) |
| State GDP Growth | 0.127*** | -0.117 | -0.106 | 0.126*** | 0.128*** | 0.124*** |
| | (0.046) | (0.084) | (0.083) | (0.047) | (0.047) | (0.046) |
| Log(Age) | -0.007*** | | | -0.007*** | -0.007*** | -0.007*** |
| | (0.003) | | | (0.003) | (0.003) | (0.003) |
| Married | 0.005** | | | 0.005** | 0.005** | 0.005*** |
| | (0.002) | | | (0.002) | (0.002) | (0.002) |
| Log(FamilySize) | 0.001 | | | 0.001 | 0.001 | 0.001 |
| | (0.002) | | | (0.002) | (0.002) | (0.002) |
| Year-month F.E. | x | x | x | x | x | x |
| Household F.E. | | x | x | | | |
| Observations | 106,590 | 127,263 | 127,263 | 105,001 | 106,353 | 107,942 |
| R-squared | 0.013 | 0.282 | 0.282 | 0.013 | 0.013 | 0.013 |

## Table 7: Spillover Cost of Fraud on Local Non-Fraudulent Companies

*Panel A: Effect on the Number of Shareholders*

This table reports the ordinary least squares parameter estimates for the change in the number of shareholders. "Big Decrease in # of Shareholders" is an indicator variable that equals 1 if the annual % change in the number of shareholders is in the bottom quartile of the distribution (< -7%). "Big Decrease in # of Retail (Inst.) Shareholders" indicates that the % change in the number of retail (institutional) shareholders is in the bottom quartile of the distribution. "Yearly Fraud in State" is the sum of the fraud revelation intensity in a state during the past year. "Yearly Fraud in Industry" is the fraud revelation intensity in three digit SIC industry during the past year. "Personal Income / Corp. Assets" is the total personal income in a state divided by the total book assets of public firms in the state. All remaining variables are defined in the Appendix. The sample period is 1984-2009. All regressions include a constant term and year fixed effects, whose coefficients we do not report. Standard errors are clustered by firm and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

| | (1) Big Decrease in # of Shareholders | (2) Big Decrease in # of Retail Shareholders | (3) Big Decrease in # of Inst. Shareholders | (4) Big Decrease in # of Retail Shareholders | (5) Big Decrease in # of Inst. Shareholders |
|---|---|---|---|---|---|
| Yearly Fraud in State | 0.718** | 0.715** | -0.028 | 0.707** | 0.523 |
| | (0.301) | (0.351) | (0.266) | (0.351) | (0.342) |
| Yearly Fraud in Industry | | | | 0.453*** | 0.315*** |
| | | | | (0.121) | (0.117) |
| Personal Income / | 0.000 | -0.000 | 0.000*** | -0.000 | 0.000*** |
| Corp.Assets | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Log(MVE) | 0.001 | 0.001 | -0.009*** | 0.001 | -0.026*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Log(M/B) | -0.027*** | -0.019*** | -0.025*** | -0.019*** | -0.023*** |
| | (0.002) | (0.003) | (0.002) | (0.003) | (0.002) |
| Return Vol. | 0.006*** | 0.011*** | 0.002** | 0.011*** | 0.022*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) |
| Inst. Own. Growth | | 0.002 | | | |
| | | (0.001) | | | |
| Year F.E. | x | x | x | x | x |
| Observations | 123,369 | 85,215 | 123,369 | 77,479 | 107,913 |
| R-Squared | 0.010 | 0.010 | 0.039 | 0.010 | 0.091 |

*Panel B: Fraud and Non-Fraudulent Firms' Cost of Capital*

In columns 1 and 2 the dependent variable is the firm's alpha (Cost of Capital) estimated from a Fama-French 3-factor model using daily returns (in percentage) for each firm and year. The unit of observation is the firm-year. In columns 3 to 5, the dependent variable is the firm's monthly return (in percentage). The variables of interest are the number of frauds revealed in the state during the past 6, 12, and 48 months respectively, divided by the number of firms in the state. We also control for the return of the market portfolio, the small minus high, the high minus low and the momentum factors. All variables are defined in the Appendix. The sample period is 1984-2009. Parameter estimates are obtained by ordinary least squares. All regressions include a constant term, which we do not report. Standard errors are corrected for heteroskedasticity and clustered by firm in columns 1 and 2 and by firm and year in columns 3 to 5. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | Alpha | | Monthly Return | | |
| Yearly Fraud in State | 0.580*** | 0.592*** | | | |
| | (0.133) | (0.123) | | | |
| Past 6 Months Fraud in State | | | 16.103* | | |
| | | | (9.213) | | |
| Past 12 Months Fraud in State | | | | 14.866** | |
| | | | | (7.002) | |
| Past 48 Months Fraud in State | | | | | 3.863 |
| | | | | | (3.433) |
| Yearly Fraud in Industry | | 0.006*** | | | |
| | | (0.000) | | | |
| Personal Income / Corp.Assets | | -0.000 | | | |
| | | (0.000) | | | |
| mkt_rf | | | 0.899*** | 0.897*** | 0.903*** |
| | | | (0.026) | (0.027) | (0.026) |
| smb | | | 0.734*** | 0.735*** | 0.735*** |
| | | | (0.045) | (0.044) | (0.045) |
| hml | | | 0.141*** | 0.146*** | 0.145*** |
| | | | (0.046) | (0.046) | (0.046) |
| mom | | | -0.166*** | -0.170*** | -0.162*** |
| | | | (0.035) | (0.035) | (0.035) |
| Observations | 138,889 | 138,709 | 1,881,090 | 1,867,288 | 1,894,537 |
| R-squared | 0.000 | 0.002 | 0.093 | 0.093 | 0.094 |

*Panel C: IV Estimates*

This table reports the second-stage results of the instrumental variable estimates. In column 1, the dependent variable is "Big Decrease in # of Shareholders", an indicator variable that equals 1 if the annual % change in the number of shareholders is in the bottom quartile of the distribution (< -7%). In column 2, the dependent variable is "Big Decrease in # of Retail (Inst.) Shareholders", an indicator variable that equals 1 if % change in the number of retail (institutional) shareholders is in the bottom quartile of the distribution. In column 3, the dependent variable is the firm's alpha (Cost of Capital) estimated from a Fama-French 3-factor model using daily returns for each firm and year. The unit of observation is the firm-year. In column 4, the dependent variable is "Ind-adj. M/B" is the firm's market-to-book ratio adjusted for the industry-year median. All variables are defined in the Appendix. The sample period is 1994-2005. All regressions include a constant term, which we do not report. Standard errors are clustered by firm and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

| VARIABLES | (1) Big Decrease in # of Shareholders | (2) Big Decrease in # of Retail Shareholders | (3) Alpha | (4) Ind-adj. M/B |
|---|---|---|---|---|
| Yearly Fraud in State | 52.866* | 45.708* | 5.670** | -222.863*** |
| | (29.376) | (24.793) | (2.232) | (56.164) |
| Personal Income / | 0.000 | 0.000 | 0.000 | -0.001** |
| Corp. Assets | (0.000) | (0.000) | (0.000) | (0.000) |
| Return Vol. | 0.007*** | 0.007*** | | |
| | (0.002) | (0.002) | | |
| Log(MVE) | -0.015*** | -0.012*** | | |
| | (0.004) | (0.004) | | |
| Log(M/B) | 0.013*** | 0.020*** | | -0.010 |
| | (0.002) | (0.002) | | (0.018) |
| Log(Assets) | | | | -0.022 |
| | | | | (0.034) |
| ROE | | | | -0.080 |
| | | | | (0.205) |
| Dividend Payer | | | | -0.206 |
| | | | | (0.138) |
| RD/Sales | | | | 0.866*** |
| | | | | (0.119) |
| Missing RD | | | | -0.480*** |
| | | | | (0.079) |
| AGE | | | | -0.965*** |
| | | | | (0.286) |
| Observations | 38,864 | 29,545 | 43,887 | 37,553 |