Corporate Scandals and Household Stock Market Participation

Mariassunta Giannetti
Stockholm School of Economics, CEPR and ECGI

Tracy Yue Wang
University of Minnesota

January 16, 2014

Abstract

We show that after the revelation of corporate fraud 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 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. As a consequence of the decrease in local households' demand for equity, firms headquartered in the same state as the fraudulent firms experience 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

Contact information: Mariassunta Giannetti, Department of Finance, Stockholm School of Economics, email: Mariassunta.Giannetti@hhs.se; Tracy Yue Wang, Department of Finance, Carlson School of Management University of Minnesota: email: wangx684@umn.edu. We are grateful to Diego Garcia, Cam Harvey, Gur Huberman, Zoran Ivkovich, Jonathan Karpoff, George Korniotis, Adair Morse, and seminar participants at the Miami Behavioral Finance Conference, the Fraud and Misconduct Conference at the University of California, Berkeley, the U.S. Securities and Exchange Commission, the University of Minnesota, the University of Lausanne, the University of Zurich, the National University of Singapore, the University of New South Wales, De Paul University, Koc University, and the Southwestern University of Finance and Economics for comments. Giannetti acknowledges financial support from the NASDAQ/OMX Nordic Foundation.

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; Dyck, Morse, and Zingales 2013). Besides inflicting direct losses to shareholders, corporate fraud may also have indirect effects on households' willingness to participate in the stock market, which may generate even larger losses by increasing the cost of capital for other firms. Evidence of the externalities generated by corporate fraud, however, 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 committed by 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 increase 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 and state level controls. We find unambiguous evidence that household stock market participation decreases both on the extensive and intensive margins following corporate scandals in the state where the household resides. Moreover, households decrease their stock holdings in fraudulent *as well as* non-fraudulent firms. *All* households, not only the ones holding the stocks of fraudulent firms, decrease their equity

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

holdings. We also provide some evidence that households increase their holdings of bonds and other fixed income securities. Thus, the decrease in household stock market participation is not driven by financial losses associated with holdings in fraudulent stocks.

One may wonder to what extent our findings are driven by state level economic conditions that are associated with 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 its demise. We thus use the fraction of firms in a state that were Arthur Andersen's clients right before its 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 exploits *within-state* variation in households' life-time experience of corporate scandals. Even households living in the same state at a particular point in time can have different corporate fraud experiences depending on their age and because they

may have moved across states. 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.

Both identification strategies indicate that fraud revelation has a causal impact on household stock market participation. Two mechanisms could drive this effect. First, fraud revelation may undermine trust in the stock market and lead households to rebalance their portfolios away from equity. Second, fraud revelation could affect state economic conditions (e.g., by increasing uncertainty about future employment and income) in a way that leads households to rebalance their portfolios towards less risky securities. The second identification strategy exploiting within-state heterogeneity implies that our findings are not driven by the fact that fraud revelation reflects or causes deterioration in state economic conditions as these should affect all individuals in the state, independently from their past experiences. There is also no evidence that fraud revelation in a state predicts future fraud revelation or deterioration in economic conditions in the state. We also find that naïve households are more affected, and that households react to fraud revelation in the state differently from institutional investors. Taken together, our findings suggest that corporate fraud affects stock market participation by undermining households' trust in the stock market.

Importantly, fraud revelation in the state leads to a decrease in the number of retail shareholders and a (temporary) decrease in the valuation of firms that have not been revealed having committed fraud, but are headquartered in the same state as the firms committing fraud. Consistent with a negative demand effect caused by corporate fraud, we find that the decrease in valuation is more pronounced for firms with less geographically dispersed activities and less ex

ante institutional ownership, which may have to offer higher returns to attract other shareholders replacing local households. We further show that following fraud revelation in the state, non-fraudulent firms tend to repurchase their shares and that local mutual funds tend to increase their holdings in these firms. Thus, our results are unlikely to be driven by an increase in the probability that other firms in the state have committed fraud or by changes in state economic conditions.

This paper is related to several strands of literature. The first strand of literature focuses on the incidence and consequences of corporate fraud. 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 trust helps explain the decision to participate in the stock market. Malmendier and Nagel (2011, 2013) 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. We highlight the negative effect of corporate scandals on households' demand for equity.

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 fraud on 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, 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 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.

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, mutual funds, or investment trusts in a given year. For the first two survey rounds, 1984 and

[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.

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. We 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 similar when we exclude these two survey years, or 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.

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

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

marital status. We exclude the value of (non-IRA) equity from family wealth 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 PSID 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.

We conjecture that households residing in the same state as the alleged firms' headquarters are more exposed to these fraud cases. Firms tend to locate their main operating facilities close to the headquarters (Hong, Kubik and Stein, 2008; Pirinsky and Wang, 2006). Thus, households in the same state are closer to the firm's core business activities and to the center of information exchange between firm and suppliers, service providers and investors.

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, which we exploit 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 intensity as the number of revealed frauds divided by the total number of publicly traded

[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 from 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 date of the first announcement of a Wells Notice or settlement agreement).

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

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 four 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 by larger companies in a state may affect households more strongly for various reasons. The revelation of fraud in these firms may receive more local media coverage. Local households may also be more exposed to these frauds 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. "*Fraud in State 2*" is the market-share-augmented fraud revelation intensity in the past four years in a state.

Second, we measure the severity of a fraud case using alleged firm's cumulative market-adjusted stock return on the seven key event days associated with the case. Karpoff et al. (2008) call this the "market penalty" or the "reputation cost" of fraud. Since the market reaction to fraud revelation is usually negative, our augmenting factor is *(1- cumulative market reaction*), giving larger weights to more severe cases. "*Fraud in State 3*" is the market-penalty-augmented fraud revelation intensity in the past four years in a state.

Third, frauds in local companies 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 next 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 retail-ownership-augmented fraud revelation intensity in the past four years in a state.

Finally, and perhaps most importantly, we consider that some firms, even though headquartered in a state, have activities in many other states. These firms are unlikely to be truly local (Garcia and Norli, 2012) and households outside the state are likely to be exposed to fraud revelation in these geographically dispersed firms. We use data on the number of states in which a firm operates, collected by Garcia and Norli (2012) from the firms' 10-K forms for the years 1994 to 2008, and construct a new weighting function equal to (50-number of states)/49.[7] Thus, frauds committed by firms that operate in fewer states are counted as more influential for households residing in the headquarters' state. "*Fraud in State 5*" is the geographic-dispersion-augmented fraud revelation intensity in the past four years in a state and is constructed only for the 1994 to 2008 period due to data availability.

Panel C of Table 1 reports the summary statistics of the four cumulative fraud revelation intensity measures. 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%.

[7] We thank Diego Garcia for kindly sharing these data with us.

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 disproportionately 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 buy-and-hold state stock market return of all publicly traded companies headquartered in the state over the past four years ("*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 6.8% GDP growth rate per annum. The average four-year state stock market return is 19%.

3. Fraud Revelation and Household Stock Market Participation

3.1 Empirical Model

We relate measures of household stock market participation to our proxies for fraud revelation using the following empirical model:

$$Participat\,ion_{ijt} = \beta \times FraudInSta\,te_{jt} + \gamma X_{ijt} + h_i + s_j + \zeta_t + \varepsilon_{ijt}, \qquad (1)$$

where participation can be *Equity Participation*, *log(Equity Value)*, *log(Net Equity Purchase)*, or *Equity-Wealth Ratio*. The matrix X_{ijt} includes a host of time-varying controls that vary across states or across households. We capture household-specific time-invariant factors by household fixed effects (h_i), and differences across states using state fixed effects (s_j). Finally, we control for changes in macroeconomic conditions using year fixed effects (ξ_t). Year fixed effects also capture country-wide changes in the extent of fraud implying that we are able to estimate only the differential effect arising from local exposure to fraud. Since presumably all households have some exposure to corporate fraud, our estimates are to be interpreted as a lower bound of the negative effects of corporate fraud on the demand for equity.

Given the large number of fixed effects, 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 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 household 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 25th percentile to the 75th 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 8 are largely invariant when we use the alternative measures of fraud exposure. Consistently with our empirical strategy, the effect of fraud appears larger in column 8, where we give a larger weight to the fraud of firms with more geographically

concentrated operations. The impact of fraud in alleged firms with more geographically concentrated operations is presumably more local. It is therefore comforting that these frauds 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 (column 4) and the proportion of equity investment in the household's total wealth decreases (column 5).

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 or other state-level economic shocks because we control for the household's wealth and income at the time of the survey and state economic conditions. Furthermore, as shown in column 6, households that previously held stocks increase their holdings of bonds, insurance policies and other fixed income securities after the revelation of fraud in their state. These results suggest that fraud leads households to rebalance their portfolio away from equity.

To further mitigate concerns that the correlation between household equity market participation and fraud revelation in the state could be driven by unobservable state level factors, in what follows, we propose two alternative identification strategies.

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, 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 indicted in March 2002 and convicted in June 2002. As a consequence, all AA clients had to find new external 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. 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 (when there was no 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 and household fixed effects allow us to control for other factors that systematically affect household stock market participation and to capture the changes in stock market participation due to the *AA Shock*. This identification strategy is valid if the instrument has enough variation to explain changes in fraud revelation in the state and no independent effect on households' decisions to hold stocks. Below we provide evidence supporting the identifying assumptions.

Column 1 in Panel A of Table 3 shows that the instrument is relevant. *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.

Our instrument, however, may not satisfy the exclusion restriction if the state proportion of AA clients was correlated with state economic conditions. This is unlikely to have been the case. AA clients and the clients of other Big 5 auditing firms have been shown to have similar characteristics, including a similar probability of financial restatements, before the collapse of AA (Eisenberg and Macey, 2004; Agrawal and Chada, 2005). Thus, if the effect of AA Shock on *Fraud in State* is indeed due to the fact that only AA clients were forced to change auditors, we should not observe an analogous increase in fraud revelation in states with a higher proportion of other Big 5 clients. We compute pseudo 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. Columns 2 to 5 in Panel A of Table 3 show that the cross-state differences in the fraction of firms that were clients of the other Big 5 auditing firms do not positively predict differences in fraud revelation intensity after 2001, indicating that the clients of the Big 5 firms are not special along some unobserved dimension.

We also 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. The IV results provide evidence in favor of the causal effect of fraud on household stock market participation.

3.4 Within-State Differences in Household Fraud Experiences

An alternative identification strategy is to exploit *within-state* variation in households' fraud experiences. In our baseline specifications, we attribute identical experiences of corporate scandals to households living in the same state in a given year. However, Malmendier and Nagel (2011, 2013) show that economic experiences way into the past affect risk preferences and expectations. Past corporate fraud experiences can differ 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.[8]

By using the past fraud experiences of a household, we can thus obtain within-state variation in the households' experiences of fraud, which helps to address two layers of identification issues. First, it allows us to include state-year fixed effects to control for any state level unobserved factors and statistically demonstrate that state-specific changes cannot drive the observed effect of fraud in state. Second, using within-state variation in fraud experience helps to shed light on the mechanisms driving the effect of fraud revelation on household stock market participation. As mentioned before, fraud may affect households for two reasons. Fraud may affect state economic conditions and uncertainty, and consequently households' equity holdings. Alternatively, fraud revelation may undermine households' trust in the stock market and lead households to spurn equity for behavioral reasons. We consider the first mechanism less likely because in results that we do not report for brevity, we do not find any effect of *Fraud in State*

[8] We find no evidence that households that have moved across states have a different propensity to participate in the stock market or respond differently to fraud, compared to households that have never moved.

on state future economic performance.[9] However, examining the effect of within-state differences in households' fraud experience provides a more rigorous test of the mechanisms because if fraud worked exclusively by affecting state economic conditions we should find no additional effect of within-state differences in households' fraud experiences on stock market participation.

We compute the fraud experience of household *i* since adulthood as the sum of past fraud intensity experienced by household *i* in the state of residence *s* at time *t*-*k*, $\frac{1}{M_i}\sum_{k=1}^{M_i} Fraud_{i,s,t-k}$, where M_i is the minimum between ($age_{i,t}$-18) and (*t*-1980).[10, 11] We impose this restriction because our fraud revelation data start in 1980, which prevents us from capturing the early part of the adult-age life experience of the oldest households in our sample.

Table 4 shows that within-state variation in households' past fraud experience due to age and mobility still negatively and significantly predicts their stock market participation. Since we include interactions of state and year fixed effects, this test demonstrates that the effect of fraud is not driven by state level economic conditions or by the fact that fraud affects the state's future economic prospects. This test also suggests that fraud revelation in the state does not communicate relevant information on the future prospects of other firms in the states, as this information should have been relevant for all households in the state, independently from their past experience.[12] (In the subsequent analysis, we provide further evidence that a knowledge

[9] In particular, we find no evidence that past fraud revelation in a state is related to future changes in state income growth or unemployment.

[10] In unreported specifications, we evaluate empirically whether past memories about corporate fraud decay in a similar fashion as in Malmendier and Nagel (2011). The estimates suggest that the memory of experienced corporate scandals does not decay, indicating that corporate scandals, being rarely discovered and dramatic, stick in people's mind. This evidence supports the use of a constant weighting scheme.

[11] Estimates are similar to the ones we report if we consider the lifetime fraud experience of the household head by setting M_i equal to (*t*-1980).

[12] Consistently, in tests that we do not report for brevity, we find that the probability that fraud revelation in the state does not predict the probability that other firms in the state will be revealed having committed fraud.

spillover is not at work in our setting, using households' individual stockholdings and firm level data.) The effect of fraud thus appears more likely to be driven by the fact that households lose trust in the stock market for behavioral reasons.

3.5 Which Households are Most Affected?

We also examine cross-sectional differences in the effect of fraud. If fraud revelation affects household stock market participation for behavioral reasons, we would expect that naïve households are more likely to lose trust in the stock market following 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 1 shows that fraud 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. [13]

In column 2 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. Although the parameter differences are not statistically significant, the effect of fraud on old households appears significantly larger from an economic point of view, even after considering the

[13] Consistent with this interpretation, in unreported tests we find that households with at least one member employed in the finance industry (approximately, 3% of the sample) do not decrease their stock market participation following fraud revelation in the state.

unconditional probability that each group participates in the stock market.[14] This contrasts with the evidence of Malmendier and Nagel (2011a) who find that young people are more affected by their recent stock market experience, but is consistent with our previous findings on education, as empirical evidence shows that the quality of financial decision making erodes with crystallized intelligence after peaking in the mid-fifties (Agarwal, Driscoll, Gabaix and Laibson, 2009; Korniotis and Kumar, 2011).

Finally, the effect of fraud revelation on stock market participation does not appear to depend on the household's wealth, as the interaction term between the household's wealth and our measure of fraud revelation intensity is not statistically significant. This also confirms that the effects of fraud we document are unlikely to be driven by an increase in economic uncertainty, caused by fraud, as in this case we would expect the effect to be smaller for wealthier households that have a larger buffer.

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. This allows us to provide direct evidence on the spillover effect of fraud. To achieve this, we use information on which stocks households actually hold from the brokerage data of Barber and Odean (2000).

[14] 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.

We explore the effects of fraud revelation in a state on changes in the households' equity holdings during the year.[15] 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. Although this effect may appear small, households with a brokerage account likely have a stronger propensity to hold equity than average households.

In column 3, we concentrate on households that did not hold stocks of firms involved in fraud in the past 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 are not exclusively driven by the financial losses experienced by households holding fraudulent stocks.

In column 4, we explore the effect of fraud revelation on 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, we explore to what extent households' decrease in equity holdings may depend on the fact that fraud revelation communicates negative information on other stocks in the household's portfolio. For instance, firms are considered more likely to have committed fraud if other firms in the same industry are revealed as fraudulent (Goldman, Peyer and Stefanescu, 2012 and Gleason, Jenkins, and Johnson, 2008). To evaluate whether these information spillovers can explain our findings, column 5 considers households' changes in equity holdings

[15] Differently from our previous tests using PSID, here we 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.

in non-fraudulent firms that are not in the same 2-digit SIC industries as the firms that have been revealed having committed fraud during the year. Our results remain unaltered.

In the same spirit, column 6 considers the effect of fraud revelation on households' change in equity holdings in non-fraudulent firms with headquarters outside the state. These firms are unlikely to have been affected by state economic shocks leading to or following fraud revelation. We find that households that have been exposed to fraud in their states reduce their holdings in these non-fraudulent out-of-state firms as well.

These results suggest that exposure to fraud negatively affects households' general propensity to hold equity. Thus, fraud revelation may affect *all* firms, even the ones that did not commit fraud. Given that households tend to hold local stocks, fraud revelation in a state could disproportionately decrease the demand for the stocks of firms headquartered in the same state as the fraudulent firms. In the next section, we explore whether these firms are indeed disproportionately affected.

4. Spillover Effect and the Economic Cost of Corporate Fraud

In this section, we exclude fraudulent firms and focus on firms that were *not* revealed having committed fraud. We explore whether the demand for equity of non-fraudulent firms indeed decreases 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 be large. The stock prices of firms in states hit by corporate scandals may have to decrease and the expected returns may have to increase in order to attract institutional investors and distant households. The magnitude of the stock price decrease –and of the externality on firms that are not revealed to have committed

fraud—should depend on the extent of market segmentation, which determines how easily a firm can attract institutional investors and distant households. It is thus an empirical question whether other investors substitute local households with limited implications on stock prices and returns.

4.1 Changes in the Number of Shareholders

If there were a decrease in the demand for the stocks of firms located in states where frauds are revealed, then we would expect to observe a decrease in a firm's number of shareholders, particularly retail shareholders, following fraud revelation 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%).[16]

Panel A of Table 7 estimates linear probability models for the likelihood that a firm experiences a large decrease in the number of shareholders. Besides including our main variable of interest *Yearly Fraud in State*, we control for the firm's market capitalization, market-to-book ratio, return volatility and past returns. 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 depends 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 0.934, which means that a one-standard-deviation

[16] 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.

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 2.6 percent.[17]

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 could be affected by corporate scandals not only because fraud revelation undermines their trust in the stock market, but also because market participants rationally update their beliefs about the probability of frauds in other firms and their expectations about future stock returns. Since these information spillovers are believed to affect predominantly firms in the same industries as the fraudulent firms, 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 in each 3-digit SIC industry during the year, which is calculated as the number of revealed frauds scaled by the number of public firms in that industry.

[17] This value is obtained by multiplying the coefficient estimate of 0.934 with the standard deviation of *Yearly Fraud in State* (0.41%) and dividing it by the ex ante probability of a large drop in the number of shareholders (0.15).

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. 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. In contrast, 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 supports our conjecture that the drop in retail shareholders following fraud revelation in a state is not driven by information on the probability of future fraud revelation in the state or on expected returns of non-fraudulent firms in the state.

To further show that retail investors' response to fraud is unlikely to reflect rational updating on the extent of fraud in the state, we test whether we observe an analogous drop in the number of shareholders following an increase in the level of earnings management in the state during the year. A high level of earning management reflects poor information disclosure, and is correlated with firms' propensity to misreport (Healy and Wahlen, 1999). If households' stock market participation reflects rational updating on the future prospects of firms in the state, we would expect to observe a decrease in the number of retail investors if the extent of earnings management in the state increases.

As a proxy for earnings management, we use discretionary accruals, measured with the modified Jones' model of Kothari, Leone, and Wasley (2005) as explained in the Appendix. Then we test whether yearly changes in the average level of earnings management in a state *"*Δ*(Earnings Mgmt. in State)*" are related to changes in the numbers of shareholders similarly to fraud revelation in the state. In column 6, we find that the number of retail investors of a firm

does not drop following an increase in earnings management in the state. Interestingly, column 7 shows that the number of institutional investors is more likely to drop following an increase in earnings management, confirming that the change in earnings management in a state provides valid information about the disclosure quality and the probability of misreporting in the state.

Overall, these results confirm that the demand for equity of local firms decreases following fraud revelation in the state and that this effect is unlikely to be due to rational updating, but rather to the fact that households in the state lose trust in the stock market as suggested by our earlier results.

One may wonder who substitutes the local households that sell stocks in non-fraudulent firms following the revelation of fraud. Other shareholders who are not affected by the revelation of fraud could increase their stockholdings in these firms. Also, firms could repurchase their shares, especially if the decrease in demand for equity leads to undervaluation (as we show below). Panel B of Table 7 provides evidence that these mechanisms are at work. Column 1 shows that in the years with more fraud revelation in the state, non-fraudulent firms repurchase more equity thus decreasing the amount of outstanding stocks. Furthermore, the proportion of shares of local non-fraudulent firms held by mutual funds whose management companies are located in the state increases with fraud revelation in the state (column 2). Since mutual funds are generally believed to have access to superior information on local firms (Coval and Moskowitz, 2001), this also indicates that the reaction of retail investors to fraud in state cannot be driven by a knowledge spillover.

4.2 Firm Valuation and Ex Post Returns

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 are revealed may lower the

valuation of non-fraudulent firms. Panel A of Table 8 shows that the valuations of non-fraudulent firms indeed decrease in the year in which fraud is revealed in the state. The economic magnitude is substantial. The coefficient estimates imply that a one-standard-deviation increase in *Yearly Fraud in State* is associated with a $29 million decline in the market capitalization of firms with the average book value of equity of $651.24 million. This result is robust to controlling for *Yearly Fraud in Industry* to take into account the possibility of an information spillover effect and for the change in earnings management in the state to control for any potential change in fraud propensity in the state.

If *Yearly Fraud in State* indeed matters because of a demand effect, we expect that the effect of fraud in state on non-fraudulent firms' valuation depends on how easy it is to attract other investors replacing the local households. Thus, if local firms have characteristics that make them unattractive to institutional investors, we expect the effects to be larger. This is precisely what we find in column 4, where the negative interaction effect between *Yearly Fraud in State* and the lagged firm institutional ownership indicates that the effect of fraud in state on firm valuation is mitigated for firms that attract more interest of institutional investors.

Similarly, following Garcia and Norli (2012), we recognize that firms with operations in many states are unlikely to be truly local to the state in which they are headquartered. Therefore, we should observe a smaller effect of *Yearly Fraud in State* on the valuation of these firms, not only because they can more easily attract distant investors, but also because the negative spillover of fraud they cause is unlikely to be limited to their state (and would be absorbed by the year fixed effects). This is precisely what we find in column 5, further confirming that *Yearly Fraud in State* likely captures the negative demand effect of fraud revelation in the state, rather than a knowledge spillover.

Panel B of Table 8 performs the same set of tests considering the ex post risk-adjusted returns of non-fraudulent firms during the year following fraud revelation. To account for firms' different exposure to systematic risk factors, for each firm and year, we estimate a Fama-French three-factor model using daily returns from 1984 to 2010. We then relate the firm's excess return to the intensity of fraud revelation in the firm's state in the past year. We find that fraud revelation in the state during the last year is associated to higher abnormal returns for non-fraudulent firms, particularly those with lower institutional ownership and those that operate in fewer states. This suggests that the decrease in valuation of non-fraudulent firms in states with more fraud revelation is temporary. However, it leads to a higher cost of capital for these firms (as measured by ex post realized returns) in the short-run. It also affects firms' behavior by increasing stock repurchases.

5. Conclusion

This paper shows that the revelation of corporate fraud in a state decreases the stock market participation of households that reside in that state and are thus more exposed to fraud. Importantly, by estimating a differential effect of households' exposure to fraud, we only identify a lower bound of the negative effects of corporate fraud on the demand for equity. Presumably, all households are affected by corporate fraud cases with high national news coverage and non-local ownership. Thus, the magnitude of the effects of corporate fraud may be much larger.

Since the documented effects of fraud revelation on household stock market participation are obtained after controlling for possible channels leading to changes in state economic conditions or informational spillovers on local firms, it appears that households lose trust in the

stock market because of corporate fraud. 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 creating a cost for all firms in the state.

References

Agarwal, Sumit. John C. Driscoll, Xavier Gabaix and David Laibson, 2009, The age of reason: Financial decisions over the life-cycle with implications for regulation, *Brookings Papers on Economic Activity* Vol. Fall, 51-117.

Agrawal, Anup, and Sahiba Chadha, 2005. Corporate governance and accounting scandals. *Journal of Law and Economics* 48, 371–406.

Barber, Brad M., and Terrance Odean, 2000. Trading is hazardous to your wealth: The common stock investment performance of individual investors, *Journal of Finance* 55, 773–806.

Coval, Joshua, and Tobias Moskowitz, 2001. The geography of investment: Informed trading and asset prices. *Journal of Political Economy* 109, 811–841.

Dyck, Alexander, Adair Morse, and Luigi Zingales, 2010, Who blows the whistle on corporate fraud, *Journal of Finance* 65, 2213-2253.

Dyck, Alexander, Adair Morse, and Luigi Zingales, 2013. How pervasive is corporate fraud? Working paper, University of Chicago.

Eisenberg, Theodore, and Jonathan R. Macey, 2004. Was Arthur Andersen different? An empirical examination of major accounting firm audits of large clients. *Journal of Empirical Legal Studies* 1(2), 263-300.

Garcia, Diego, and Øvind Norli, 2012. Geographic dispersion and stock returns. *Journal of Financial Economics* 106(3), 547-565.

Gleason, C. A., N. T. Jenkins, and W. B. Johnson, 2008, The contagion effects of accounting restatements. *Accounting Review*, *83*, 83–110.

Goldman, Eitan, Urs Peyer, and Irina Stefanescu, 2012, Financial misrepresentation and its impact on rivals. *Financial Management*, Winter 2012, 915 – 945.

Graham, John R., Si Li, and Jiaping Qiu, 2008. Corporate misreporting and bank loan contracting. *Journal of Financial Economics* 89, 44-61.

Grinblatt, M. and M. Keloharju, 2001, How distance, language, and culture influence stock-holdings and trades, *Journal of Finance* 56, 1053–1073.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales, 2008, Trusting the stock market, *Journal of Finance* 63, 2557–2600.

Guiso, Luigi and Paolo Sodini, 2013, Household Finance: An Emerging Field, *Handbook of the Economics of Finance*, volume 2B, Constantinides, G. M., M. Harris, and R. M. Stulz, eds, Elsevier, North Holland.

Healy, Paul M., James M. Wahlen., 1999. "A review of the earnings management literature and its implications for standard setting," 13 *Accounting Horizons* 365-383.

Hong, H., J. Kubik, J. Stein, 2008, The only game in town: Stock-price consequences of local bias. *Journal of Financial Economics* 90, 20–37.

Hribar, Paul, Nicole T. Jenkins, 2004. The effect of accounting restatements on earnings revisions and the estimated cost of capital. *Review of Accounting Studies* 9, 337-356.

Ivkovic, Zoran and Scott Weisbenner, 2005, Local does as local is: Information content of the geography of individual investors' common stock investments, *Journal of Finance* 60, 267–306.

Karpoff, Jonathan M., D. Scott Lee, and Gerald S. Martin, 2008. The cost to firms of cooking the books. *Journal of Financial and Quantitative Analysis* 43, 581–612.

Karpoff, Jonathan M., Allison Koester, D. Scott Lee, and Gerald S. Martin, 2012. An analysis of database challenges in financial misconduct research. Working paper, University of Washington.

Korniotis, George M. and Alok Kumar, 2011. Do older investors make better investment decisions?, *Review of Economics and Statistics* 93, 244-265.

Korniotis, George M. and Alok Kumar, 2013. State-Level business cycles and local return predictability, *Journal of Finance* 68, 1037–1096

Kothari, S.P., Leone, Andrew J., Charles E. Wasley, 2005. Performance matched discretionary accrual measures. *Journal of Accounting and Economics* 39, 163-197.

Kravet, Todd, and Terry Shevlin, 2010. Accounting restatements and information risk. *Review of Accounting Studies* 15, 264-294.

Malmendier, Ulrike and Stephan Nagel, 2011. Depression babies: Do macroeconomic experiences affect risk taking? *Quarterly Journal of Economics*, *126*(1), 373–416.

Malmendier, Ulrike and Stephan Nagel, 2013. Learning from inflation experiences. Working Paper, UC Berkeley.

Pirinsky, Christo and Qinghai Wang, 2006. Does corporate headquarters location matter for stock returns? *Journal of Finance* 61, 1991–2015.

Povel, Paul, Raj Singh, and Andrew Winton, 2007. Booms, busts, and fraud. *Review of Financial Studies* 20, 1219-1254.

Seasholes, Mark and Ning Zhu, 2010, Individual investors and local bias. *Journal of Finance* 65, 1987–2010.

Wang, Tracy Yue, Andrew Winton, and Xiaoyun Yu, 2010. Business conditions and corporate securities fraud: Evidence from IPOs. *Journal of Finance* 65, 2255-2292.

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.
Bonds & Fixed Income Securities	A dummy that equals 1 if the household holds bonds, treasuries, insurance policies, banks' certificate of deposits and other fixed income securities in a given year.
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 State 5	The cumulative fraud revelation intensity, in which the weight of each case of fraud is (50-# of Operating States)/49, where # of Operating States is the number of states mentioned in the firms 10K, as collected by Garcia and Norli (2012).
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.
Δ (Earnings Mgmt. in State)	The yearly change in the average level of earnings management in a state. To construct the earnings management measure, we follow three steps. First, for each year and Fama-French industry, we estimate the following cross-sectional model:

	$\frac{TA_{i,t+1}}{Asset_{i,t}} = \beta_0 + \beta_1 \frac{1}{Asset_{i,t}} + \beta_2 \frac{(\Delta Sale_{i,t+1} - \Delta AR_{i,t+1})}{Asset_{i,t}} + \beta_3 \frac{PPE_{i,t+1}}{Asset_{i,t}}$ where TA is earnings before extraordinary items minus operating cash flows, AR is account receivables, and PPE is the gross value of property, plant and equipment. The coefficient estimates in this model are used to compute the normal accruals. Second, we compute the discretionary accruals as the actual accruals minus the normal accruals. Finally, we take the average change in discretionary accruals in a state-year.
# 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.
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.
Past Return	The firm's stock return during the last year.
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 proportion of institutional ownership of a firm in a given year.
Share Repurchase	The value of stock repurchases of a firm relative to the firm's market capitalization at the end of the previous year.
State Mutual Fund Ownership	The proportion of shares of a firm held by mutual funds whose management companies are located in the same state as the firm at the end of a given year.

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
Bonds & Fixed Income Securities	17142	0.130	0.336	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. All fraud intensity measures have been multiplied by 100.

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 State 5	728	0.008	0.021
Fraud in Industry	7975	0.012	0.046
Δ(Earnings Mgmt. in State)	1402	0.000	0.058

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 the 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
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
Past Return	136990	0.175	0.879	-0.231	0.035	0.307
Share Repurchase	156221	0.011	0.039	0.000	0.000	0.002
Local Mutual Fund Ownership	68934	0.005	0.014	0.000	0.000	0.002

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 (including IRA). 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 at the household level 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", 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)	(8)
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]	
Fraud in State 5								-0.486*
								(0.293)
								[1.006]
Log(Age)	0.091***	0.345***	0.289***	0.389***	0.345***	0.345***	0.345***	0.170**
	(0.006)	(0.041)	(0.056)	(0.038)	(0.041)	(0.041)	(0.041)	(0.067)
Married	0.059***	0.017**	0.006	0.016**	0.017**	0.017**	0.017**	0.003
	(0.005)	(0.007)	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)
Log(Family Size)	-0.048***	0.004	0.000	-0.003	0.004	0.004	0.004	0.001
	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
Log(Family Income)	0.032***	0.009***	0.007***	0.007***	0.009***	0.009***	0.009***	0.006***
	(0.002)	(0.001)	(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***	0.008***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)

Dependent Variable: Equity Participation	(1)	(2)	(3) Excl. 1984 & 1989	(4) With IRA	(5)	(6)	(7)	(8)
State GDP Growth	0.098	0.245**	0.238	0.162	0.245**	0.246**	0.245**	0.199
	(0.109)	(0.124)	(0.175)	(0.111)	(0.124)	(0.124)	(0.124)	(0.179)
State Stock Return	0.005	0.004	0.003	0.007	0.004	0.004	0.004	0.007
	(0.005)	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
Years in School	0.032***							
	(0.001)							
Household F.E.		x	x	x	x	x	x	x
Year F.E.	x	x	x	x	x	x	x	x
State F.E.	x	x	x	x	x	x	x	x
Observations	64,192	66,085	53,974	66,085	66,085	66,085	66,085	46,810
R-squared	0.233	0.609	0.645	0.760	0.609	0.609	0.609	0.660

Panel B: Level of Participation

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 at the household level and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

	(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	(6) Bonds & Fixed Income Securities
Fraud in State	-3.895**	-4.714**	-3.918*	-2.659**	-0.112**	-0.399
	(1.912)	(2.159)	(2.038)	(1.152)	(0.055)	(0.348)
Fraud in State *						1.012*
Equity Participation (lagged)						(0.586)
Equity Participation (lagged)						0.018
						(0.017)
Log(Age)	1.698***	1.878***	-4.558***	2.227***	0.025	-0.117
	(0.497)	(0.676)	(0.521)	(0.290)	(0.016)	(0.144)
Married	0.138	-0.000	0.034	0.044	0.000	0.019
	(0.085)	(0.098)	(0.081)	(0.056)	(0.003)	(0.018)
Log(Family Size)	0.164**	0.163**	0.303***	0.024	0.001	-0.009
	(0.064)	(0.076)	(0.062)	(0.041)	(0.002)	(0.012)
Log(Family Income)	0.072***	0.052***	0.027*	0.040***	0.002***	-0.001
	(0.016)	(0.018)	(0.015)	(0.011)	(0.000)	(0.003)
Log(Wealth)	0.139***	0.130***	0.090***	0.026***	0.003***	0.011
	(0.006)	(0.006)	(0.005)	(0.003)	(0.000)	(0.001)
State GDP Growth	0.670	2.292	0.888	1.505	-0.035	-0.141
	(1.455)	(2.213)	(1.225)	(0.978)	(0.043)	(0.281)
State Stock Return	0.061	-0.005	0.044	0.038	0.004**	-0.011
	(0.091)	(0.099)	(0.088)	(0.046)	(0.002)	(0.016)
Household F.E.	x	x	x	x	x	x
Year F.E.	x	x	x	x	x	x
State F.E.	x	x	x	x	x	x
Observations	66,045	53,963	66,082	65,013	66,048	14,013
R-squared	0.579	0.612	0.583	0.325	0.471	0.273

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 at the year level 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 at the household level 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 Estimates

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 at the household level and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

	First Stage Fraud in State	Second Stage Equity Participation
Instrumental Variable		
AA Shock	0.029***	
	(0.002)	
Endogenous Variable		
Fraud in State		-8.927***
		(0.917)
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	0.088***	0.985***
	(0.011)	(0.199)
State Stock Return	-0.002**	-0.004
	(0.001)	(0.010)
Household F.E.	x	x
Year F.E.	x	x
Observations	37,579	37,579

Table 4: Within-State Households' Differences in Fraud Experience

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 average fraud intensity experienced by a household in the state of residence since when the head of the household was 18. 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, whose coefficients we do not report. Standard errors are clustered at the household level and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

	Equity Participation
Experienced Fraud	-8.137*
	(4.533)
Log(Age)	0.035***
	(0.004)
Married	0.062***
	(0.004)
Log(Family Size)	-0.071***
	(0.003)
Log(Family Income)	0.053***
	(0.002)
Log(Wealth)	0.024***
	(0.000)
State-year F.E.	x
Observations	66094
R-squared	0.193

Table 5: Which Households are Most 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. 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 at the household level 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 *LessEducated	-0.229**		
	(0.105)		
	[-1.92%]		
Fraud in State *MoreEducated	-0.458**		
	(0.210)		
	[-1.31%]		
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 * Log(Wealth)			0.005
			(0.028)
Fraud in State		0.337***	-0.406*
		(0.034)	(0.203)
Log(Age)	0.345***	0.017***	0.345***
	(0.048)	(0.006)	(0.048)
Married	0.017*	0.004	0.017
	(0.009)	(0.004)	(0.009)
Log(Family Size)	0.004	0.009***	0.004
	(0.004)	(0.001)	(0.004)
Log(Family Income)	0.009***	0.010***	0.009***
	(0.001)	(0.000)	(0.001)
Log(Wealth)	0.010***	0.243**	0.010***
	(0.001)	(0.106)	(0.001)
State GDP Growth	0.242**	0.004	0.245**
	(0.080)	(0.005)	(0.081)
State Stock Return	0.004	x	0.004
	(0.006)	x	(0.006)
Household F.E.	x	x	x
Year F.E.	x	66,085	x
State F.E.	x	0.609	x
Observations	66,085	52,257	66,084
R-squared	0.609	0.652	0.609

Table 6: Households' Changes in Equity Holdings across Different Stock Portfolios

This table reports the estimated effects of fraud 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. Models (1) and (2) consider all stocks in the household's portfolio to compute the change in equity holdings. Model (3) excludes investors that hold any stock(s) that are involved in fraud in the past 12 months. Model (4) excludes stocks of firms that have been revealed having committed fraud during the past 12 months (fraudulent stocks) in the calculation of change in equity holdings. Model (5) considers changes in equity holdings in non-fraudulent firms in 2-digit SIC code industries other than the industries of the firms committing fraud. Model (6) considers changes in equity holdings in stocks of non-fraudulent firms headquartered out of the state in which the household resides. "Yearly Fraud in State" is the fraud revelation intensity in the past 12 months in a state. "Portfolio Return" is the return of 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. All regressions include a constant term and fixed effects as indicated in the Table, whose coefficients we do not report. Standard errors are clustered at the household level and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

Dependent Var.: Change in Equity Holdings	(1)	(2)	(3) Excluding investors w/ fraudulent stocks	(4) Excluding fraudulent stocks	(5) Non-fraudulent stocks in different industries	(6) Out-of-state non-fraudulent stocks
Yearly Fraud in State	-0.746***	-0.980**	-0.733**	-0.745***	-0.808***	-0.727**
	(0.283)	(0.404)	(0.288)	(0.284)	(0.282)	(0.330)
Portfolio Return	0.000	0.001*	0.000	0.000	0.000	0.004
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.002)
State GDP Growth	0.127***	-0.117	0.126***	0.128***	0.137***	-0.727**
	(0.046)	(0.084)	(0.047)	(0.047)	(0.046)	(0.330)
Log(Age)	-0.007***		-0.007***	-0.007***	-0.006**	0.001
	(0.003)		(0.003)	(0.003)	(0.003)	(0.001)
Married	0.005**		0.005**	0.005**	0.005**	0.117**
	(0.002)		(0.002)	(0.002)	(0.002)	(0.054)
Log(FamilySize)	0.001		0.001	0.001	0.001	0.001
	(0.002)		(0.002)	(0.002)	(0.002)	(0.003)
Year-month F.E.	x	x	x	x	x	x
Household F.E.		x				
Observations	106,590	127,263	105,001	106,353	105,247	68,305
R-squared	0.013	0.282	0.013	0.013	0.012	0.015

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 year. "Yearly Fraud in Industry" is the fraud revelation intensity in three digit SIC industry during the 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 1984-2010 and includes only non-fraudulent firms. All regressions include a constant term and year fixed effects, whose coefficients we do not report. Standard errors are clustered at the firm level 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	(6) Big Decrease in # of Retail Shareholders	(7) Big Decrease in # of Inst. Shareholders
Yearly Fraud in State	0.934***	0.980***	-0.180	0.956***	-0.200	1.155***	-0.191
	(0.315)	(0.368)	(0.285)	(0.368)	(0.285)	(0.380)	(0.298)
Yearly Fraud in Industry				0.924***	0.516***		
				(0.143)	(0.103)		
Δ Earnings Mgmt. in State						0.064	0.106*
						(0.064)	(0.054)
Personal Income / Corp.Assets	-0.002***	-0.002***	-0.001**	-0.002***	-0.001**	-0.002***	-0.001**
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Log(MVE)	0.002	0.001	-0.012***	0.001	-0.012***	0.001	-0.014***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(M/B)	-0.025***	-0.017***	-0.018***	-0.017***	-0.019***	-0.010***	-0.017***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Return Vol.	0.006***	0.011***	0.001*	0.011***	0.001	0.013***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Past Firm Return	0.009***	0.009***	-0.026***	0.009***	-0.026***	0.008***	-0.027***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Inst. Own. Growth		0.001		0.001		0.001	
		(0.001)		(0.001)		(0.001)	
Year F.E.	x	x	x	x	x	X	x
Observations	112,122	79,057	112,122	79,057	112,122	70,125	96,745
R-squared	0.010	0.010	0.042	0.011	0.042	0.009	0.038

Panel B. Who Replaces Retail Investors

In column 1, the dependent variable is the value of stock repurchases of a firm relative to the firm's market capitalization at the end of the previous year. In column 2, the dependent variable is the proportion of shares of a firm held by mutual funds whose management companies are located in the same state as the firm. All remaining variables are defined in the Appendix. The sample period is 1984-2010 and includes only non-fraudulent firms. Parameter estimates are obtained using ordinary least squares. All regressions include a constant term and fixed effects as indicated in the table, whose coefficients we do not report. Standard errors are clustered at the firm level and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

	(1) Share Repurchase	(2) Local Mutual Funds Ownership
Yearly Fraud in State	0.072*	0.011*
	(0.037)	(0.007)
Personal Income /	-0.000***	0.000
Corp.Assets	(0.000)	(0.000)
Log(MVE)	0.002***	0.001***
	(0.000)	(0.000)
Log(M/B)	-0.004***	-0.000*
	(0.000)	(0.000)
Return Vol.	-0.001***	-0.000
	(0.000)	(0.000)
Past Return	0.000	0.000
	(0.000)	(0.000)
Inst. Own. Growth	-0.000*	
	(0.000)	
Firm F.E.		x
Year F.E.	x	x
Observations	75,051	60,986
R-squared	0.020	0.582

Table 8: Valuation Effects of Fraud Revelation in the State

Panel A. Non-Fraudulent Firms' Valuations

The dependent variable is the change in firm market capitalization between year t-1 and t, divided by the value of the firm's book asset at t-1. All remaining variables are defined in the Appendix. The sample period is 1984-2010 and includes only non-fraudulent firms. Parameter estimates are obtained using ordinary least squares. All regressions include a constant term and fixed effects as indicated in the table, whose coefficients we do not report. Standard errors are clustered at the firm level and corrected for heteroskedasticity. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Yearly Fraud in State	-10.793**	-10.616**	-12.116**	-73.599***	-19.700***
	(5.162)	(5.139)	(5.550)	(8.774)	(7.134)
Yearly Fraud in Industry		-5.234***			
		(1.562)			
Δ Earnings Mgmt. in State			-3.209*		
			(1.734)		
Yearly Fraud in State x Firm Inst Own				164.490***	
				(20.483)	
Yearly Fraud in State x # of Operating States					1.119**
					(0.456)
Firm Inst Own				-0.712***	
				(0.244)	
# of Operating States					-0.003
					(0.004)
Δ Personal Income / Corp.Assets	0.019	0.019	0.023	0.012	0.040
	(0.014)	(0.014)	(0.014)	(0.008)	(0.030)
Return Vol.	0.066***	0.067***	0.068***	0.047**	0.043***
	(0.014)	(0.014)	(0.015)	(0.024)	(0.017)
Δ ROE	-0.011	-0.011	-0.009		
	(0.164)	(0.165)	(0.175)		
Year F.E.	x	x	x	x	x
Observations	100,181	100,181	90,069	80,198	59,690
R-squared	0.002	0.002	0.002	0.002	0.014

Panel B. Non-Fraudulent Firms' Ex Post Returns

The dependent variable is the firm's abnormal return (in percentage) estimated from a Fama-French 3-factor model using daily returns for each firm and year. The unit of observation is the firm-year. "# of Operating States" is the number of states in which a firm operates, obtained from Garcia and Norli (2012), computed as the number of states mentioned in the 10K form. All remaining variables are defined in the Appendix. The sample period is 1984-2010, except for column (6) where the sample period is 1994 to 2008. 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 at the firm level. ***, **, * denote significance at 1%, 5%, and 10% levels, respectively.

VARIABLES	(1)	(2)	(3)	(4)	(6)
Past Year Fraud in State	0.671***	0.507***	0.579***	1.499***	1.325***
	(0.124)	(0.124)	(0.129)	(0.254)	(0.235)
Past Year Fraud in Industry		0.006***			
		(0.000)			
Past Δ(Earnings Mgmt. in State)			-0.090***		
			(0.026)		
Past Year Fraud in State x Firm Inst Own				-2.320***	
				(0.463)	
Past Year Fraud in State x # of Operating States					-0.036*
					(0.020)
Firm Inst Own				-0.019***	
				(0.002)	
# of Operating States					-0.001***
					(0.000)
Personal Income / Corp.Assets		0.000	0.000	0.000	0.001*
		(0.000)	(0.000)	(0.000)	(0.000)
Observations	135,279	135,279	110,423	97,468	60,994
R-squared	0.000	0.000	0.000	0.002	0.004