Three essays on corporate finance

Sun, Rui

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THREE ESSAYS ON CORPORATE FINANCE

RUI SUN

NANYANG BUSINESS SCHOOL

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Summary

This dissertation comprises three essays on corporate finance.

Essay one documents that the generalized public trust in companies held by residents of a state reduces the likelihood of financial misstatements made by public firms headquartered in the same state. A closer examination reveals that public trust can only significantly reduce the likelihood of misstatements due to reporting errors, but it is ineffective in reducing misstatements due to irregularities. This implies that honest managers prefer to maintain the trust placed in them and take more care not to accidentally misstate, while dishonest managers dismiss or exploit public trust. The above findings are consistent with the idea that corporate culture is aligned with local culture, and can also be partly explained by managers' concern about investors' opinions, since my results show that stock market reactions to restatements are more negative for firms located in more trusting states. Additional evidence also supports that public trust has limited impacts when earnings are manipulated in an obscure way, or when corporate governance is poor.

Essay two investigates how private firms allocate internally generated cash flow by estimating the cash flow sensitivities of various uses of cash flow. Our results reveal that, compared with public firms, private firms allocate less cash flow to investments and cash saving, and direct more to dividends and reduction of external finance, especially debt. The above pattern remains robust if we apply various matching schemes or if we only look at the transitory cash flow shock. To find out what is the driving force of private firms' cash flow allocation, we attempt to draw parallel evidence from public firms, but find that the analysis within public firms cannot be applied mechanically to private firms. Special features of private firms – more concentrated equity ownership, more powerful shareholders with a strong desire

for liquidity and diversification, and higher agency costs associated with debt finance are very likely to play a vital role in shaping their cash flow allocation.

Essay three studies the impact of Stop Trading on Congressional Knowledge (STOCK) Act on congressional stock trading. Using hand-collected congressional financial disclosure data, we find that the frequency, dollar volume, timing ability, and profitability of congress trading have been reduced much after the STOCK Act, and especially so in purchase transactions which better reflect the activeness of potentially informed trading. A parallel study using CEO insider trading data does not exhibit such changes around the passage of this law, which points to the causality relationship flowing from the STOCK Act to changing congressional trading patterns.

Essay One

Public Trust in Companies and Financial Report Misstatements

Abstract

This paper documents that the generalized public trust in companies held by residents of a state reduces the likelihood of financial misstatements made by public firms headquartered in the same state. A closer examination reveals that public trust can only significantly reduce the likelihood of misstatements due to reporting errors, but it is ineffective in reducing misstatements due to irregularities. This implies that honest managers prefer to maintain the trust placed in them and take more care not to accidentally misstate, while dishonest managers dismiss or exploit public trust. The above findings are consistent with the idea that corporate culture is aligned with local culture, and can also be partly explained by managers' concern about investors' opinions, since my results show that stock market reactions to restatements are more negative for firms located in more trusting states. Additional evidence also supports that public trust has limited impacts when earnings are manipulated in an obscure way, or when corporate governance is poor.

JEL Classification: M14, M41

Keywords: Public Trust; Reporting Quality; Financial Misstatement; Earnings Management; Corporate Governance

1. Introduction

It is widely recognized in social science literature that trust provides the cohesiveness necessary for the development of meaningful social relationships and a healthy economy (Hardin 2002). An important role that trust plays in economy is reducing the transaction costs associated with daily social interactions, and making market exchange less prone to risk and uncertainty (e.g., Putnam, 2000; Fukuyama 1995; Hearn 1997). Besides, trust increases intra-organizational efficiency and productivity, because it enhances workers' satisfaction and morale, and boosts their job performance and the profitability of the organization (e.g., Fukuyama, 1995; Miller, 2001; Cook, 2001). More recent research in economics and finance literature shows that trust promotes investments, trade, and economic growth, encourages financial development and investors' participation in the stock market, facilitates venture capital investment, corporate financing, and cross-border mergers and acquisitions, reduces corporate agency problems, and influences regulations in finance and economy (e.g., Knack and Keefer, 1997; Zak and Knack, 2001; Guiso et al., 2004, 2008; Bottazzi et al., 2016; Duarte et al., 2012; Ahern et al., 2013; Hilary and Huang, 2015; Aghion et al., 2010). There is also a burgeoning literature relating trust to accounting issues. For example, Berglund and Kang (2013) and Jha and Chen (2015) find that firms headquartered in U.S. counties with high social capital pay lower audit fees. Wei and Zhang (2014) and Pevzner et al. (2015) document that stocks headquartered in high trust areas experience larger market reactions upon earnings announcements. Jung et al. (2016) show that more trustworthy firms experience less under-reaction upon earnings news.

Even though the empirical evidence is almost one-sided toward the benefits of trust, there is evidence that excessive trust is not necessarily good. Butler et al. (2014)

show that individual income and trust have a hump-shaped relationship: the most trusting individuals are too gullible; the least trusting individuals forgo too many profitable opportunities; so the right amount of trust is somewhere in between. Levi (1999) suggests distrust, or a healthy skepticism, can improve certain systems better than outright trust.

This paper intends to examine one of the rarely tapped trust-related issues: How would the public trust in companies affect corporate financial reporting quality? When it comes to financial reporting quality, it is even more necessary to take a dialectical view on trust. There is a good chance that the management may cook the book when existing and potential shareholders are more trusting. On one hand, trusting investors tend to impose less monitoring on corporate operation, perceive the reported numbers as more credible, and demand less information. On the other hand, as long as managers have discretion over accounting choices, they would smooth reported income and the rate of growth in income (Gordon, 1964). Graham et al. (2005) also report that an overwhelming 96.9% of all survey executives prefer a smooth earnings path and 78% of all surveyed executives would give up economic value in exchange for smooth earnings. In reality, to increase compensation, to appropriate personal benefits, to secure the job, to reduce the regulatory costs, or to increase regulatory benefits, managers often exaggerate or conceal true earnings by manipulating accruals, altering investment decisions, and by obfuscating financial information in annual reports (Healy and Wahlen, 1999; Graham et al., 2005; Roychowdhury, 2006). A more trusting local culture provides perfect soil to breed managers' various incentives of misreporting.

There are also several tenable reasons that trust should improve corporate financial reporting quality. First, in a more trusting environment, people tend to honor

their obligations and be more meticulous in their jobs. Second, trust can facilitate the production and sharing of information. Dirks and Ferrin (2001) suggest that high trust can result in more positive attitudes, higher levels of cooperation, and superior levels of performance. It is intuitive that trust affects job satisfaction and communication; after all effective communication is critical to high quality financial reports. Financial statements incorporate information gathered from all levels of the organization. Higher trust and better communication will reduce the probability of employees' window dressing the financial statements or budget gaming, so that their manager will have access to a report reflecting the truth (Mayer and Gavin, 2005; Bart, 1988; Chong and Ferdiansah, 2011; Libby and Lindsay, 2013). Third, trust is hard to build once broken. The high trust between manager and outsiders would collapse if outsiders find out or suspect the manager is intentionally managing earnings or misreporting at their expenses. In this situation, investors would probably impose stronger monitoring and stricter contracting on the manager, demand more information on earnings and disclosure, express their anger by selling out the shares, or even fire the top management. If such potential cost outweighs the benefit of managing earnings or misreporting, it will forestall managers' irresponsible acts. Fourth, individuals prefer to conform to the local culture rather than confronting it; as conformity can reduce or avoid the costs or penalties associated with rejecting the standards, values, or beliefs held in that area (Kohlberg, 1984; Sunstein 1996). These costs also include psychological costs, which can be incurred even if the misbehavior is undetected (Fehr and Gaechter, 2000; Sliwka, 2007). In particular, these costs are more pronounced in high trust regions. Therefore in the repeated game setting, rational managers analyze the costs and benefits of betraying trust and they may decide that the utility obtained through deviating in current period may be lower than

the disutility in future periods.

Temptation of abusing trust and benefits of valuing trust preclude an affirmative answer to our research question: If an average resident is holding much faith in companies, will the management of companies located here be more trustworthy and prepare better quality financial statements? In our paper, we define trust by looking at the question asked in General Social Survey (GSS): “For major companies, as far as the people running them are concerned, would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in them?” The average of responses by state residents constitutes our measure of state-level public trust. Therefore, my trust measure primarily captures investors’ general trusting attitude in companies rather than a company’s perceived trustworthiness.

At the same time, we define financial reporting quality to be the extent to which financial reporting conveys information about the firm’s financial position that is complete, neutral, and free from error.¹ Our major proxy for reporting quality is misstatements extracted from Hennes et al. (2008). Since Hennes et al. (2008) data only provides the announcement date for a restatement, we determine its restated period partly by resorting to Audit Analytics restatement database, and partly by manual search of the filings on SEC EDGAR. We combine this dataset with the Compustat universe to identify which firm-years were misstated in their original financial reports. We keep all observations with valid explanatory and control variables, and in our sample there are both misstated firm-years and non-misstated firm-years.

Using a logit model, we find that higher state trust can significantly reduce the likelihood of a firm headquartered in this state making a financial misstatement, after

¹ This is following the Financial Accounting Standards Board Statement of Financial Accounting Concepts No. 8 [2010].

controlling for firm level characteristics such as information transparency, reported profit/loss, legal risk, complexity of corporate structure, operating risk, growth opportunity, stock market performance, etc, and simultaneously controlling for state fixed effects and year fixed effects. An interquartile change of trust reduces the likelihood of misstatement by 0.4%, given the unconditional probability of misstatement to be 5.5%.

Hennes et al.'s (2008) dataset has the advantage of distinguishing intentional and accidental accounting misstatements, which is important because these two types of misstatements are done by managers with different motivations, who may respond to public trust differently. We use a multinomial logit model to show that public trust can only significantly reduce the likelihood of misstatements due to errors, but it is not effective in decreasing the likelihood of misstatement due to irregularities. The negative relation between trust and error-type misstatements is consistent with the pro-trust arguments, because honest managers would try to maintain the trust placed in them by preparing better quality financial statements with fewer or no errors. The insignificantly positive relation between trust and irregularity-type misstatements is consistent with the con-side arguments, because dishonest managers who have planned to manipulate financial reports are ready to cheat the investors or even try to exploit public trust.

A common concern is the reverse causality problem, since it might be intuitive that the trust level should be lower in states which saw a larger number of misstatements. But this concern is not very likely to affect our results, especially considering there is a time lag between committing misstatement and being discovered. "Misstatements" which could affect our trust measure of interest are not those "misstatements" we are looking at in this paper, because the former happen

much earlier than the latter, and the former are already discovered, but the latter are not discovered yet. To make it clear, I use the bootstrapping methodology to estimate a two-stage regression. The first stage is a state level regression, where I use regress trust in companies on prior restatement rate, as well as trust in people, trust in court, GDP per capita, unemployment rate and year dummies. In the second stage, I replace the trust measure in the logit and multinomial logit model with trust components predicted by the five determinants and the residual which represents idiosyncratic trust. I repeat the regression for 500 times and find that only idiosyncratic trust is responsible for my baseline findings. The negative relation between current misstatements and trust has little to do with the relation between current trust and prior misstatements.

To explain why state trust can affect financial report misstatements, there are two possible mechanisms. First, public trust in companies can reflect the perceived trustworthiness of firms, or the trustworthiness of the management. It is recognized that individuals prefer to conform to the local culture rather than confronting the standards, values, or beliefs held in that area (Kohlberg, 1984; Sunstein 1996). CEOs, CFOs, and other employees usually reside close to the firm headquarter; they will inevitably incorporate local social norms into the corporate culture. In addition, firms like to hire and retain employees that share their own values, and employees also prefer to work for firms that share their own values (Holland, 1976; Tom, 1971; Vroom, 1966). This mutual selection solidifies the congruence in culture. If all employees in high trust states are trustworthy just as perceived by the public, they can improve financial reporting quality.

Second, our trust measure captures the confidence of local investors. CEOs and CFOs have incentives to maintain the trust held by investors because the firm may

need to access the capital market in the future. This argument is based on the idea that local culture is largely congruent with local investors' values, and also on the literature that investors have a bias to invest in stocks of companies they are more familiar with, such as local firms (Huberman, 2001; Coval and Moskowitz, 2001; Baik, Kang, and Kim, 2010; Cohen, 2005). In this paper, I validate the second mechanism by documenting that: 1) our baseline results are amplified if we use proxies which more accurately identify investors' trust level; 2) and more trusting investors are more likely to exert negative pressures on the stock price if financial misreporting is finally revealed.

To better capture investors' trust level, we construct a holding weighted average trust based on the state distribution of institutional investors. Using the framework of the baseline models, we find that this trust measure has a larger deterrence effect on misstatements than the simple state trust. An interquartile change of this weighted average trust reduces the overall misstatement likelihood by 1.0%. Moreover, an interquartile change of this weighted average trust significantly reduces error misstatement likelihood by 1.0%, and insignificantly increases irregularity misstatement likelihood by 0.1%. Since a firm's investors are more likely to be located in the same region than in the same state, I also insert region level trust into the baseline model, and find that region trust also has larger deterrence effect than state trust.

The consequences of misreporting once discovered can be negative market reaction (Palmrose et al., 2004), forced CEO turnover (Desai et al., 2006), enforcement actions by the SEC (Feroz et al., 1991; Dechow et al., 1996), or lawsuits by shareholders (Kellogg, 1984; Francis et al., 1994). As around three quarters of our restatements are due to errors instead of irregularities, CEO departure, lawsuits and

enforcement should not be the universal consequences. In comparison, market reaction applies to all sorts of restatements and has a standard metric, because even unintentional errors in financial reports once discovered will exacerbate the information asymmetry between managers and investors and are costly to shareholders (Beatty et al., 2010; Bharath et al., 2008; Bhojraj et al., 2009; Biddle et al., 2009; Graham et al., 2005; Gunny, 2010). Therefore in our paper, we look at the buy-and-hold abnormal returns (BHARs) around the date when those mistaken financial restatements are initially discovered, and find that firms in more trusting states experience more negative market reactions than those in less trusting states. This result holds for different event window lengths involved with both errors and irregularities. Regression results also show a negative relation between market reactions upon restatements and trust, which is especially intense and significant in the subsamples of restatements due to errors.

Our paper contributes to the extant literature in at least three ways. First, to the best of my knowledge, this is the first paper to examine how public faith in companies can affect corporate behavior. The only other paper using this measure in finance and accounting area is Pevzner et al. (2015), who do not use it as the main variable but merely in a robustness check, and what they examine is the relation of cross-country trust variations and investor behavior instead of corporate behavior. We recommend this measure because compared to trust in people used in other papers, trust in companies can better reflect the perceived trustworthiness of the relevant economic decision maker – firms, and better reflect the confidence of stock market investors; compared to intra-firm trust used in other papers, our trust measure can avoid reverse causality. Second, we bridge the gap between trust literature and financial reporting quality literature. Previous research explains why firms misreport from aspects of firm

characteristics, managers' compensation scheme, insiders' opportunism, and SEC enforcement preferences (e.g., Dechow et al., 2011; Armstrong et al., 2013; Ali and Hirshleifer, 2015; Kedia and Rajgopal, 2011). Our paper is among the first to show that public confidence as a cultural element can affect the occurrence of financial misstatements. Third, we extend Hennes et al. (2008) by shedding light on the circumstances that give rise to intentional and accidental misstatements, and we find that public trust has opposite effects on these two types of misstatements.

The remainder of the paper proceeds as follows. Section 2 introduces related literature. Section 3 analyzes what does my trust measure capture and what drives its differences across states. Section 4 elaborates on the impact of trust on financial misstatements and the mechanisms behind. Section 5 touches on extension topics. Section 6 concludes.

2. Related literature

Trust is generally defined as a subjective belief about the reliability of a potential counterparty. For example, the most basic type of trust we usually talk about is trust in a specific individual based on familiarity and repeated interactions. A society is composed of individuals. A higher-level, more abstract type of trust is trust in the “generalized other”, such as unacquainted strangers. In this situation, we draw experience with prior acquaintances, and make decisions based on previously formed trust in our deep heart. Hearn (1997) argues that it is easier to trust someone whom we do not know very well if that person lives or works in a place where strong norms of reciprocity or trust exist and are recognized. Social trust is a local culture which aggregates individuals' generalized trust, permeates each entity and organization within the locality, and shapes people's behavior in their future interactions. Welch et

al. (2005) gives such a definition: “Social trust is the mutually shared expectation, often expressed as confidence, that people will manifest sensible and, when needed, reciprocally beneficial behavior in their interactions with others.” In our empirical part, like many other papers, we focus on generalized trust, so we are concerned with what might be considered cursory beliefs and stereotype ideas. The difference is that we do not look at trust in a “generalized other person”; we focus on trust in a “generalized firm”.² But in this literature review, we do not particularly distinguish specific trust versus generalized trust or trust in people versus trust in organizations unless otherwise indicated.

Another closely related concept is social capital. Social capital refers to features of social life, networks, norms, and trust that enable participants to act together more effectively to pursue shared objectives (Putnam, 2000). In social science literature, some treat trust and social capital as synonymous (e.g., Putnam, 2000), some see them as mutually reinforcing (e.g., Brehm and Rahn, 1997), while some do see their differences (e.g., Welch et al., 2005; Hearn, 1997). One common situation where these two concepts need to be distinguished is that trust is more of a relational concept, but social capital can be attributed to a person or entity whose total connections represent his/her/its stock of social capital. In our paper, to give a more comprehensive literature review, we do not particularly differentiate them.

The beneficial effects which trust brings to economics and finance area are well

² Alesina and La Ferrara (2000) and Hilary and Huang (2015) use data from General Social Survey (GSS). Delhey et al. (2001) and Delhey and Newton (2003) are based on Euromodule surveys coordinated by the Social Science Research Centre Berlin (WZB). Guiso et al. (2004) use World Values Survey (WVS) responses to proxy for trust. Guiso et al. (2008) insert the WVS trust question into 2003 Dutch National Bank (DNB) Household Survey. Pevzner et al. (2015) use WVS responses in 25 countries. Wei and Zhang (2014) use the US regional trust data collected by WVS. In all the above surveys, though conducted by different institutions, they all ask the respondents one simple question: “Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?”

documented. An incomplete summary of this strand of literature includes the following. Knack and Keefer (1997), Temple and Johnson (1998), and Zak and Knack (2001) establishes a positive relationship between trust and economic growth. Guiso et al. (2004) identify that households in high-social-capital areas of Italy are more likely to use checks, invest more in stocks rather than cash, rely more on institutional credit rather than informal credit. Guiso et al. (2008) find that trust can increase stock market participation. Guiso et al. (2009) study the importance of trust for bilateral trade in goods, financial assets, and direct foreign investment. Scholz and Lubell (1998) find that political trust can increase citizens' taxpaying compliance. Lins et al. (2015) find firms which invested more in social capital perform better during crisis. Pevzner et al. (2015) document that the abnormal stock return variance and abnormal trading volume during earnings announcement period are both higher in more trusting countries. Hilary and Huang (2015) find that firms located in more trusting counties suffer less from agency problems such as overinvestment and bad acquisitions, display higher profitability and valuation, utilize less incentive compensation, experience less forced CEO departures unconditionally, but they are more likely to fire CEOs conditional on ethical breaches. Our paper complements this literature by documenting the deterrence effect of trust on financial reports misstatements, which serves as direct evidence to explain why trust amplifies announcement effects and increases stock market participation. Different from these papers, our results raise the caveat that trust might not work for firms deliberating to fuzz up the financial reports.

To date, the empirical evidence showing the relation between trust and financial reporting quality is sparse. One exception is Garrett et al. (2014) who find that intra-organizational trust is associated with lower likelihood of financial misstatement. Our paper differs from theirs in several ways. First, Garrett et al. (2014) use employees'

trust in management data based on 39 survey questions collected by Great Place to Work Institute. As argued by the author, because of the design of data collection process, there is a possibility that the management may exert some influence on employees during the survey. On the contrast, our trust data is directly collected from local residents, which is unlikely to be influenced by companies. Second, trust in management is susceptible to the reliability of the firm's financial statement, which easily leads to endogeneity problems. But the trust measure in our paper is local residents' general confidence in major businesses, which represents the public psychological perception of businesses in general, rather than a direct appraisal to a specific firm, so it is not likely to be reversely caused by one firm's misstatement. Third, Garrett et al. (2014) cover only 1029 observations of public firms that applied for inclusion in the Best Companies list. These firms are relatively large and their results may not generalize to smaller firms. While our sample covers much more observations with more varied firm sizes. Fourth, they use all restatement data from Audit Analytics and do not distinguish errors and frauds. In our paper, we use data from Hennes et al. (2008) who employ a systemic procedure to denote whether a restatement is due to error or irregularity, and this enables us to better identify the different impacts of trust on financial reporting quality.

3. Understanding public trust in companies

3.1. Public trust

Many prior papers focus on cross-country variations of trust; here we look at inter-state variation of trust in the US. The advantage of using US data is that we can more clearly pin down the role of trust because firms in our sample face practically identical regulatory, legal, and accounting standards. Therefore, other institutional

factors are less likely to explain our findings. Our trust measure is based on responses to the General Social Survey (GSS). GSS is a sociological survey, administered by the National Opinion Research Center (NORC) at the University of Chicago, used to collect data on demographic characteristics and attitudes of U.S. residents. The survey is administered as a face-to-face interview within around 90 minutes.

We are interested in this question from GSS survey: “For major companies, as far as the people running them are concerned, would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in them?” For the answers to this question, GSS codes “a great deal” as 1, “only some” as 2, and “hardly any” as 3. We reverse the order of coding for each respondent so that a higher number represents high trust. Then we take the average across all respondents in the same state and same year. Then for each state-year, we have one number between 1 and 3 to represent the state trust level.

This question was included in the survey for years of 1973-1978, 1980, 1982-1984, 1986-1991, 1993-1994, 1996, 1998, 2000, 2002, 2004, 2006, 2008, 2010, and 2012. We only keep trust data from 1996 to 2006, partly due to the availability of restatement data, and partly because GSS changed the survey sampling design after 2006.³ In each of the six survey years during 1996-2006, full probability sampling is employed across the nation, and a random sample of respondents are independently drawn, so their responses are quite representative.⁴ Among all the respondents, less than 5% have finance or accounting related work experience, and excluding these

³ Since 2008, GSS changed the survey from a cross-sectional design to a rotating panel design. Therefore, not using surveys after 2006 has an additional advantage that our trust measure is based on randomly selected persons in each year and not subject to problems associated with artificially repeated respondents.

⁴ Here is an excerpt from GSS Codebook 2015: “Each survey from 1972 to 2004 was an independently drawn sample of English-speaking persons 18 years of age or over, living in non-institutional arrangements within the United States. Starting in 2006 Spanish-speakers were added to the target population.”

people do not materially affect our trust measure and subsequent results.

Table 1 summarizes trust data by alphabetic order of the state name. Since each state might not have survey participants in each of the six years, the “Years” column shows in how many years there are participants from this state, which can be called “non-missing years”. The maximum number of non-missing years is 6 and the minimum 1. If there are no participants in any of the six years, we denote it as “No data”. The “Respondents” column shows the average number of participants for the non-missing years from this state. The “Trust” column calculates the average state level trust for the non-missing years. The “Rank” column ranks the “Trust” column from highest to lowest.

In each of these six years, 39 to 43 states and federal districts are covered in the survey, and only 5 states do not have any survey participants from 1996 to 2006. From the last row of the table, we can see that on average there are only 75 participants from each state in each survey year. This validates our choice of state level trust instead of county level, because if the participants are further assigned to many counties, the average number of respondents for each county would be too small to calculate an accurate county level trust.¹

As shown in Table 1, state trust ranges from 1.85 for New Mexico to 2.34 for North Dakota, with a mean of 2.06. We further plot the “Trust” column into an US map which is Figure 1. Overall, central areas have highest trust, the next high is west coast, while the mountain area of west US and east coast area have lowest trust. But this generalized classification is not always right because it is common to see a high trust state adjoin a low trust state.

For the 46 states with trust data, we linearly interpolate trust between non-missing years, so we get yearly state trust from 1996 to 2006.

3.2. Restatements

Prior to Hennes et al. (2008), most papers implicitly assume all restatements are equivalent to aggressive accounting practice. Hennes et al. (2008) argue that it is necessary to differentiate between errors and irregularities, but the database which is most commonly used by scholars, General Accounting/Government Accountability Office (GAO) restatement database, does not do so. Palmrose et al. (2004) attempt to solve this by resorting to SEC's Accounting and Auditing Enforcement Releases (AAERs), which is in the right direction but not so perfect, as claimed by Hennes et al. (2008), AAERs can only help identify a subset of restatements caused by irregularities. So Hennes et al. (2008) propose such a procedure to pick out irregularity cases from restatement dataset: 1) self disclosure as fraud or irregularity in restatement files; 2) cases with related SEC or Department of Justice (DOJ) investigations; 3) cases with internal or other independent investigations. Using these criteria, they classify 24 percent of restatements as irregularities and 76 percent as errors. They prove the validity of this classification by documenting that the irregularity sample is associated with more class action lawsuits and higher CEO/CFO turnover rates.

Hennes et al. (2008) collect restatements from 8-K filings in their paper, but they publish online a dataset which classifies all GAO restatements (1997-2006) into errors and irregularities. This online dataset contains 2705 observations and we drop 208 observations which are tagged as duplicates. Then we merge with Audit Analytics to get the beginning dates and ending dates of restated periods.⁵ Restated periods are identified this way only for 1083 observations. For the rest of observations, we manually search SEC EDGAR filings six months before and six months after the

⁵ In Audit Analytics (AA), very few restatements are tagged to be frauds. It is generally held that AA classification is inaccurate. That is why we do not use AA.

restatement date which is recorded in the online dataset. Finally we identify the restated period for 1992 observations altogether.

Our restatement data universe contains restatements announced during 1997-2006. Thus we only keep misstated firm-years during 1996-2005; earlier years 1991-1995 and 2006 are dropped because these years' misstatements may not be fully reflected in the restatement data and contain too few observations. If a quarter or quarters or the whole fiscal year is subsequently restated, we define this fiscal year is misstated for this firm.

3.3. *What does the trust measure capture?*

Most prior studies on social trust look at trust in people, we look at trust in companies. To some extent, this state level trust measure reflect the local corporate culture and trustworthiness of local firms, as well as local investors' opinion and confidence. Since more than 95% of respondents are not financially sophisticated, their trust in companies can stem from education, general satisfaction with product and service quality in daily life, and special events exposed by media. There can be location clustering and time series fluctuation for such public opinions.

Figure 2a plots the time trend of our trust measure (*Trust*). It clearly shows that public trust in companies declined dramatically when a large number of accounting scandals hit the market in 2001-2002. Our trust measure timely and precisely captures the trust that people have in financial reporting. For comparison purpose, we also define trust in people (*Trust people*) following Hilary and Huang (2015), and define trust in the U.S. Supreme Court (*Trust court*) in a similar way as *Trust*. Figures 2b and 2c show that these variants of public trust do not reflect the collapse of faith in companies during 2001-2002.

Our trust measure is not a perfect reflection of actual misstatements incidence rate. Figures 3a and 3b show that two types of misstatements keep increasing from 1996 to 2001, but Figure 2b shows that public trust only began to drop since 2001, which is more likely to result from media effects after the Enron scandal and other events. The time trend of median *Trust* does not perfectly track the change in the incidence rate of announced restatements either (see Figures 3c and 3d), suggesting that people's subjective opinion may over-react or under-react to the news of restatements, probably depending on the media coverage scope. By any means, state trust is unlikely to be the results of actual misstatements committed by local firms in the same year.

3.4. *What drives the different level of trust across states?*

To the best of my knowledge, there are no papers on the determinants of general trust in companies. Related literature includes Delhey and Newton (2003) who study what kind of people are more likely to trust others, and Pirson et al. (2016) who use a laboratory experiment to investigate which specific firm characteristics are more likely to earn the subjects' confidence with subjects characteristics taken into account. As with other researchers, they agree on that personal well-being, life experience, and trust in other matters affect the trust level of interest.

Even though trust in companies displays a different trend from trust in people and trust in Supreme Court (see Figure 2), our trust measure is highly correlated in the cross-section with the latter two (see Table 2 Panel A). That said, *Trust people* and *Trust court* make great predictors for *Trust*. We also assume that the number of restating firms scaled by the number of all firms in the prior year will negatively affect public attitude towards firms. It turns out that *% Firms restated last year* and

Trust have a correlation coefficient of -0.3072. We also control for state economy and public wellbeing by including *GDP per capita* and *Unemployment rate*. Table 2 Panel A shows that all chosen predictors for our trust measure have a significant correlation coefficient of more than 0.2 in magnitude. While these predictors are not too highly correlated with each other to cause multicollinearity problems.

Panel B uses two OLS models to estimate the relation between *Trust* and its predictors. The first model does not include year dummies, while the second does. Both models show that when local residents are more likely to trust people and the judicial system, they are more likely to trust companies too. If the state has seen an increase in the number of local restatements in the prior year, the trust level this year would be lower. People in wealthier states are more skeptical to firms. If more people are unemployed in a state, they are less likely to trust companies. Comparing these two models, we find that adding year dummies do not increase R^2 by a large extent, so we believe these five predictors have well captured the cross-sectional difference in *Trust*.

On the outset, I also included other state level predictors, such as religiosity, religious diversity, gender ratio, education level, proportion of firms which restated in the same year, etc., but these variables' correlation with *Trust* are less than 0.2 in magnitude, and sometimes can be very low. When included in regressions separately or altogether, all of them are insignificant. To keep a parsimonious specification, I only keep five best predictors here.

4. Trust and financial misreporting

4.1. Research design and variable summary

We examine the effect of generalized public trust in companies on a firm's

likelihood of misstating using a logit model as follows:

$$D(\text{Misstatement}_{i,t}) = \alpha + \beta \text{Trust}_{i,t} + \gamma X_{i,t} + \delta \text{State}_{i,t} + \theta \text{Year}_t + \varepsilon_{i,t}, \quad (1)$$

where $\text{Misstatement}_{i,t}$ is 1 if firm i 's financial reports for fiscal year t is misstated and 0 otherwise, noting that it is measured when the misstatement occurred and not when it is eventually disclosed publicly as a restatement. The key explanatory variable is *Trust*, which is the same for all firms in the same state and year. These two variables have been introduced in Sections 3.1 and 3.2.

Since our restatement data can reasonably distinguish accidental errors from intentional misstatements, we also construct a categorical variable, *Misstatement category*, with the base category representing firm-years that are not misstated, category one representing firm-years misstated due to errors, and category two representing firm-years misstated due to irregularities. Specifically, we look at the impact of trust on two types of misstatements by estimating a multinomial logit model as follows:

$$C(\text{Misstatement category}_{i,t}) = \alpha + \beta \text{Trust}_{i,t} + \gamma X_{i,t} + \delta \text{State}_{i,t} + \theta \text{Year}_t + \varepsilon_{i,t}, \quad (2)$$

In both models, X represents a set of firm characteristics that have been documented as important determinants of corporate financial reporting quality by previous studies (e.g., Garrett et al., 2014; Dechow et al. 2011; Hribar and Nichols, 2007; Cao et al., 2012; Ashbaugh-Skaife et al., 2008; Hoitash et al., 2009). Because large firms have greater investor recognition and public visibility, we use the natural log of total assets ($\text{Ln}(\text{size})$) to control for information transparency. Debt to asset ratio (*Leverage*) is included because debtholders may exert extra monitoring on the management which improves the reporting quality or more strict debt covenants may lead to firms' misstatement due to fear of default. The amount of profit or loss affects

corporate motives of misreporting so we add return on assets (*ROA*) and a dummy for negative net income (*Negative income*). We also control for stock performance over the fiscal year (*market-adjusted stock return*), and industries more prone to litigation and thus more likely to misstate (*Litigious industry*). If the corporate structure is more complex, there is a higher incidence rate of erroneous or even fraudulent reporting, so we control for this complexity by checking whether the firm has any foreign operations (*Foreign operation*), whether the firm has experienced any restructuring or reorganization (*Restructuring*), and how many business segments it has (*Number of segments*). Higher operating risk motivates the management to smooth earnings, so we control for operating risk proxies (*Cash flow volatility*, *Sales volatility* and *Stock return volatility*). Finally, we control for firm growth opportunities (*Market to book*) and whether the auditor is a Big 5 firm (*Big5*).

Following Hoitash et al. (2009) and several other papers, all independent variables are contemporaneous. We include a series of state dummies to account for state specific laws, regulations, enforcement intensities, other cultures and traditions aside from trust. These state dummies have subscripts of both *i* and *t*, because firm *i* may change headquarter states in different years. We also include year fixed effects to account for unobservable variables which may lead to clustering of restatements in certain years. The standard errors of the estimated coefficients are adjusted for heteroskedasticity and allow for clustering of observations by year, but our conclusions are not affected if we allow clustering by both firm and year.

When we construct the sample, we use a firm's historical headquarter state extracted from Compact Disclosure database to determine the local *Trust* level in that year. Even though we have argued that the sampling design of the GSS survey has assured representativeness of responses, some states with few respondents as shown

in Table 1 still raise the concern of measurement error in *Trust*. I have done a panel autocorrelation test using all available trust data from 1973 to 2012, and find that *Trust* in each state shows time-series consistency as far as nearly 10 year back (not reported), implying that the quality of *Trust* is robust overall. To rule out the potential measurement error as best as I can, I only keep *Trust* when the number of survey respondents is greater than 50 for a state-year pair. Further, we drop firms in the financial and utility industries because they face a very different regulatory and economic environment; we drop firms with negative total asset and book value of equity; we drop firms with extreme low stock price (<1); we remove firms with missing variables; and we exclude states where no misstatement ever happened during 1996-2005.⁶

After all this we are left with 18354 observations. Table 3 summarizes this sample. 5.5% of firm-years (1012 observations) are later restated. I also divide the sample into high trust states and low trust states by the median of *Trust*. Among the 9379 observations in low trust states, 6.9% are misstated, in which 4.9% are due to errors, and 2% are due to irregularities. Among the 8975 observations in high trust states, 4.1% are misstated, in which 2.5% are due to errors, and 1.5% are due to irregularities. It seems that high trust states have lower misstatement rate with respect to both types of misreporting. However, conditional on misreporting, high trust states are more inclined to breed intentional misstatements ($1.5\% / (2.5\% + 1.5\%) = 38\%$), compared to low trust states ($2\% / (4.9\% + 2\%) = 29\%$). Table 3 also tells that firms in high trust states have significantly different characteristics from those from low trust states, highlighting the importance of controlling for these features.

⁶ We exclude states where no misstatement ever happened during 1996-2005 because we are going to use state fixed effects in multivariate analysis.

4.2. Baseline results

The first regression in Table 4 shows our estimation of Model (1). We find that the likelihood of misstatement is negatively and significantly associated with trust. An interquartile change of *Trust* (from 2.044 to 2.167) reduces the likelihood of misstatement by 0.4%, from 5.6% to 5.2%. The coefficients of other control variables are generally consistent with prior literature. For example, firms with larger size, more foreign operations, more litigation risk, more extreme earnings (either negative or very positive), and firms with a Big 5 firm as its auditor are more likely to misstate (or more likely to be subsequently restated).

The second multinomial logit regression in Table 4 shows that public trust can significantly reduce the likelihood of misstatement due to errors, but insignificantly increase the likelihood of misstatement due to irregularities, and the coefficients of *Trust* for two outcomes are significantly different. An interquartile change of *Trust* (from 2.044 to 2.167) reduces the likelihood of making an accidental misstatement by 0.6%, from 3.9% to 3.3%. The same change in *Trust* leads to a 0.3% increase of intentional misstatement probability, from 1.7% to 2.0%.

Table 4 also tells us that accidental and intentional misstatements are generally associated with different firm characteristics. For example, firms which have foreign operations and which are engaging in restructuring have more chances to intentionally obfuscate financial reports. Firms which have reported negative income are more likely to be scrutinized by investors, just like firms with the presence of a Big 5 auditor are more likely to be scrutinized by the auditor; these firms would more often discover errors in financial reports if they restate in the future. On the contrary, managers which desperately seek to manipulate financial reports in an illegal way will do so regardless of scrutiny by other parties.

4.3. Reverse causality and two-stage bootstrapping

A common concern for this kind of study is that reverse causality could possibly contaminate our findings, since it is intuitive that the trust level should be lower in states which experienced a larger number of misstatements. But reverse causality is unlikely to plague our analysis for several reasons. First, misstatements may reversely affect trust only when they are publicly disclosed, just as shown in Table 2, the number of restatements announced in the prior year would reduce the trust level. However, restatements announced in the prior year are associated with misreporting committed even earlier. This paper focuses on the contemporaneous relation between trust level and misreporting. Second, as argued in section 3.3, trust level does not perfectly capture the occurrence of misreporting or restatements. Instead, trust reflects local investors' subjective impression which might be heavily affected by media and biased from true situation. Third, if reverse causality holds, it would predict a negative relation between trust and misstatements, which is not consistent with the opposite effects of trust on accidental and intentional misstatements as we find.

In order to find out whether the results in Table 4 have anything to do with the relation between state trust and prior restatements, I use the bootstrapping method to run two-stage regressions in this section. Specifically, I use the model specification with year dummies in Table 2 Panel B as the first stage, and decomposed the trust level into parts predicted by each determinant. I take the residual trust as some idiosyncrasies in local culture that are not correlated with the five determinants. In the second stage, I run the regressions as in Table 4, only to replace *Trust* with residual trust and each predicted component.⁷ I repeat this process for 500 times with

⁷ I could include the part predicted by year dummies too, but that would not affect coefficient

replacement of state clusters to bootstrap the standard errors of coefficients.

Table 5 reports the results of the second stage regressions for this bootstrapping method. Other control variables in Table 4 are included in estimation but not displayed for brevity. The key information here is that only idiosyncratic trust is responsible for the pattern documented in Table 4. Thus in the pure sense of statistical significance, the deterrence effect of trust on financial misstatements, especially reporting errors, is not because *Trust* captures local residents' trust in people or judicial system, is not because trust and prior restatements have a negative relation, and is not because of some spurious correlation between misreporting and state economic situation.

4.4. Investors' trust in companies

To some extent, our trust measure captures the trustworthiness of the management, as long as the CFOs and CEOs live in the same state as the corporate headquarter. As argued previously, residents' attitude would converge to local culture, so more trusting states would have more responsible executives. Even when the CFO or CEO is in a different state, the mutual selection process between the firm and executive employees would ensure that they choose each other often because they share similar values.

However, whether the management shares the corporate values is not a necessary condition for our results to hold. Company managements have practical concerns such as they want to issue equity in the future, or their compensation is linked to stock prices, so they care about stock prices. If local investors are the majority, and if they are more trusting, they are more likely to exert negative pressures on the stock price if

estimates reported in Table 5, given that we include year dummies in the second stage too.

financial misreporting is finally revealed. This potential consequence would deter financial misreporting *ex ante*. But this argument relies heavily on the local bias literature (see Baik et al., 2010, etc.).

To relax the assumption of local investment preference, a trust measure based on the distribution of investors is needed. Individual investors are not trackable, the best thing we can do is to find the documented institutional investors. Thomson Reuters S34 dataset provides information of institutions which invest an aggregate fair market value of at least 100 million dollars. The holdings of these very large institutions are not fully disclosed either. For example, small holdings under 10,000 shares or \$200,000 are excluded. The location of institutional investors are obtained from Massa, Yasuda, and Zhang (2013). We only keep the institutions which are located in states with a valid *Trust* value (when more than 50 respondents answered the GSS survey question). With the trust level for each institution and its holding shares as the weight, we can calculate a weighted average investors' trust for each firm-year.

After the matching with this *Weighted trust*, only 9060 observations are left. For these firm-years, the institutional shares we have identified account for 20% of shares outstanding on average. This percentage may not represent majority investors, but these large institutions are marginal investors who would influence the stock price most. In addition, 18% of institutions in this sample are located in the same state as the firm headquarter, and they hold 21% of institutional shares. The mean of *Weighted trust* is 2.08, and the mean of *Misstatement* is 5.3% in this sample, both of them comparable to the original sample in Table 3.

Table 6 reports the estimation results of Models (1) and (2) by replacing *Trust* with *Weighted trust*. An interquartile change of *Weighted trust* (from 2.033 to 2.133) reduces the overall misstatement likelihood by 1.0%, from 5.5% to 4.2%; reduces

error misstatement likelihood by 1.0%, from 3.7% to 2.7%; and increases irregularity misstatement likelihood by 0.1%, from 1.8% to 1.9%. Using weighted average institutional trust seems to better target the true opinion of investors, and it has stronger influence on deterring misstatements. This also underscores the importance of investors' trust in explaining the mechanism through which state-level trust works to affect financial reporting quality.

In the previous sample, 25% of institutional investors come from the same region, and they hold 26% of institutional shares. This prompts us to break the state border when claiming "local". For each of the nine U.S. regions, we create *Region trust* variable in a similar way as the state level trust.⁸ Since each region has enough respondents to produce a valid trust measure, we could estimate Models (1) and (2) in a larger sample, and control for region fixed effects. Table 7 shows that *Region trust* may capture potential investors' confidence better and have larger impacts than state trust. I did not use *Weighted trust* and *Region trust* in baseline regressions because the former would reduce the sample size significantly, and the latter has much less variations. Besides, state trust yields the most modest estimates and serves as a good starting point for the analysis.

4.5. Market reaction of restatement announcements

Prior sections focus on misstated period; this section analyzes the market reactions on dates around restatement announcements, which can serve as the *ex post* punishment of misstatements. As argued in last section, this can potentially explain why state trust helps to improve reporting quality. We keep restatements associated with misstatements in our main sample and analyze the buy-and-hold abnormal stock

⁸ The nine regions are: East North Central, Middle Atlantic States, West South Central, Pacific, South Atlantic, East South Central, West North Central, Rocky Mountain states, and New England.

returns (BHARs) between 60 days prior to and 60 days following the restatement announcements of firms in high-trust states and low-trust states. Only initial announcement dates are treated as event dates. The abnormal return is calculated by a market model with an estimation period of [-300, -61]. We use value weighted CRSP return as market return. We require that the minimum estimation period length to be 60 days and employ Scholes-Williams (1977) method to adjust for beta estimation. Finally we retain 1010 restatements with valid return data in estimation period and event period in our market reaction sample.

Figure 4a displays the mean and median BHARs during a long window [-60, +60] for restatements announced in high trust states and low trust states. Stock returns begin to drop mildly since day -60, then there is a noticeable dip on the days surrounding the restatement announcement, and they continue to drift downward till the end of our time window. One conspicuous feature is that stock returns in high trust states always stay below low trust states, and the gap widens as time goes by.

We also separately look at restatement announcements' market reactions for firms involved with irregularities and errors. Figure 4b and Figure 4c show the contrasting graph for errors and irregularities respectively. In both figures, there is always a more negative reaction for high trust states, suggesting that trusting investors are more shocked at the news of a restatement and more anxious to sell the shares at hand. One distinction between the two figures is that, the gap between high trust states curve and low trust states curve in Figure 4b is wider than that in Figure 4c.

Table 8 gives more detailed numbers for the BHARs in six symmetric windows. Panel A summarizes all restatements market reactions, showing that more trusting states have lower market reactions. For example, the average BHAR for the full window [-60, +60] in high trust states is 8% lower than that in low trust states. Market

reactions for accidental and intentional misstatements are summarized in Panel B and Panel C. Among 1010 restatements, 74% (743) is due to errors, and 26% (267) due to irregularities. This distribution is very close to Hennes et al. (2008). From the table, we see that restatements due to irregularities experience more negative market reactions.

We also use a multivariate regression to explain the market reactions around the restatement date within a short window $[-5, +5]$. Our key explanatory variable *Trust in the restatement year* is public trust in the year when the restatement is initially disclosed, not the year when misreporting occurs. We control for *Irregularity* because restatements due to irregularities should be associated with more negative returns. If in a single announcement, multiple past years are restated, the prior misstatements tend to be more severe in nature, so we also control for *Number of years restated*. If on average more years have passed before the misstatement is found out and publicly disclosed, there is a higher chance that this misstatement has been detected prior to the restatement date, so we control for *Number of years elapsed*. Since from Figure 4, we see that there was already a decline of stock returns before this short window $[-5, +5]$, either as a consequence of information leakage, or as a reason to initiate an investigation on past financial statements, we control for *BHAR(-60, -6)*. Following Palmrose et al. (2004), we further control for the information environment with *Ln(size)* and *Leverage*.

There are 946 restatements having nonmissing values for all the variables and these variables are summarized in Panel A of Table 9. Panel B shows the regression results, and we find that an interquartile increase in restatement year trust is associated with a 3% decrease in the 11-day window market reaction of a restatement announcement released. The coefficient for *Irregularity* suggests that stocks involved

with irregularities on average experience a $BHAR(-5, +5)$ 11% lower than stocks involved with errors. Other coefficients are not significant. We also look at how restatement year trust would affect $BHAR(-5, +5)$ in error and irregularity restatements subsamples. We find that the negative relation is only significant in the error restatement subsample.

There are two possible explanations. First, both high trust and low trust investors react rationally to irregularities, but high trust investors over-react to unintentional misreporting. As pointed out by Palmrose et al. (2004), “negative reactions to restatement announcements are caused by declines in future prospects (decreases in expected earnings) and/or increases in risk/uncertainty (increases in discount rates)”. Therefore, high trust investors feel that even unintentional misreporting is intolerable because it increases uncertainty of financial reports. Second, there is a possibility that high trust investors are more gullible, and they may lack the acumen to differentiate errors and irregularities accurately and timely. If more trusting investors were also the smarter ones, they might have punished irregularities more harshly and errors mildly.

Even though it is impossible to completely disentangle the above two explanations, restatements announced in high trust states do experience incremental stock price punishment, especially for accidental reporting errors. And all the results in this section are robust if we use equal weighted CRSP return in the market model or if we do not use Scholes-Williams (1977) method to adjust for beta estimation.

5. Extensions

5.1. Trust and earnings management

In this section, I check the impact of trust on other reporting quality proxies based on earnings management. I am not arguing that earnings management and financial

misstatements can be substitutes, i.e., if the firm misstate reports, it is less likely to engage in earnings management. I am not arguing that they are complements either, i.e., the presence of one must be accompanied by the other. I just want to draw some parallel evidence, considering earnings management is a common proxy for reporting quality. Especially as intentional misstatements are more difficult to detect than reporting errors, real activities manipulation is conducted in a more obscure way than accrual management (Graham et al., 2005; Cohen et al., 2008). So I expect that trust may have better deterrence effect on accrual management than real earnings management.

The accrual-based earnings management (*Accruals*) and two measures of real earnings management (*Real earnings management 1* and *Real earnings management 2*) are defined in the appendix following Cohen and Zarowin (2010). We use the same set of control variables as previously which are taken as determinants of financial misstatements. The OLS regression results reported in Table 10 show that *Accruals* can be significantly reduced by public trust, but the reduction effects of trust on *Real earnings management 1* and *Real earnings management 2* are not statistically significant. Untabulated results show that each component of *Real earnings management 1* and *Real earnings management 2*, which are abnormal operating cash flows, abnormal discretionary expenses and abnormal production costs respectively, is also reduced by higher public trust, but the results are again insignificant. Hence it appears that public trust only plays its deterring role in the easier-to-detect scenario.

5.2. Trust and corporate governance

It is also interesting to see how state trust interacts with corporate governance. There are two differing views about the relationship of trust and institutions or

contracts. Some research shows that they are substitutes. For example, Guiso et al. (2004) find that the effect of social capital or trust is stronger where legal environment is poorer or people are less educated. Hilary and Huang (2015) document that higher trust is associated with lower outside monitoring and less incentive pays for the management. However, it is also argued that trust is but an implicit and invisible contract between people, other mechanisms and explicit contracts are also needed. Misztal (1996) states that trust only sets the most basic limiting conditions for human actions. Bottazzi et al. (2016) find that investors make use of sophisticated contracts only when there is sufficient trust. Because if they have low trust in their counterparts' nation, investors would worry about the enforceability of the contracts and do not see it worthwhile to write a sophisticated contract.

To examine whether the influence of trust on financial misreporting is greater with the presence or absence of good governance, we introduce proxies for poor governance and the interaction terms of trust and poor governance into Models (1) and (2). If trust is effective only when governance is out of place, which can be called “substitute” hypothesis, then the *Trust* variable would be insignificant, and the interaction term should be significantly negative.

However, our evidence does not support this substitute hypothesis. First, we use board size to gauge internal governance. More exactly, we posit that in each year, if the board size is in the top three deciles, the board is considered to be inefficient due to incremental cost of poor communication and decision making, or rather, the internal governance is considered to be ineffective (Yermack, 1996; John and Senbet, 1998). By the above criterion, we construct a dummy variable *Big board*, where the highlighted category contains firm-years with a board of 11-28 directors, and the default category includes firm-years with a board of 1-11 directors. Untabulated

results show that trust is significantly negative, with a magnitude of more than twice of the baseline model (coefficient equals -0.99, t-statistic equals -3.00), suggesting that public trust is very effective in improving FRQ when good internal governance is in place. The interaction term is significantly positive, with a magnitude of more than overturning the benefits of trust (coefficient equals 2.00, t-statistic equals 2.47). However, Ai and Norton (2003) alert us to the fact the interpretation of the interaction coefficient in a logit regression is not straightforward.⁹ We implement their approach and report the interaction effect in Figure 5a. All data points are above 0, indicate that the trust effect is mitigated by poor internal governance for all observations. Most of the data points have a z-statistic above the 5% significance level threshold, and those that do not often occur when the predicted probability of misstatement is low. The Ai and Norton (2003) corrected z-statistic based on all observations for the untabulated interaction is 2.25. This is consistent with the idea that trust and internal governance are complements.

Second, we use G-index as proposed by Gompers et al. (2003) to measure outside governance. Higher G index keeps takeover threat at bay, encroaches on shareholder rights and is value destructive *per se*. After adding G index and its interaction with trust to the baseline mode, we find that trust has a coefficient of -2.02 with a t-statistic of -1.67, implying that for the best-governed firms, trust has a strong effect in deterring misreporting. The interaction term has a coefficient of 0.24 with a t-statistic of 1.80 in the regression. Ai and Norton (2003) corrected z-statistic for the interaction term is 1.75, and Figure 5b confirms that the interaction term coefficients are positive for all observations, manifesting that even one incremental anti-takeover clause has a non-negligible effect of corroding the trust benefits.

⁹ With the diagnostic method given by Ai and Norton (2003), we actually find that in our case, the marginal effect of interaction term estimated by an ordinary logit is as good as Ai and Norton (2003) method. The details are not exhibited to save space.

These results reveal that without proper corporate governance, the impact of public trust on misreporting can be limited. And good governance fares better on reducing restatements in a more trusting environment. Trust and governance are mutually reinforcing.

6. Conclusion

Public trust as a cultural environment factor has been shown to benefit various economic areas. Our paper finds that public trust can help to deter financial misstatements, especially to discourage reporting errors, but it is ineffective in reducing reporting irregularities. We identified one of the channels for this deterrence effect: managers worry about investors' stock market reaction once the misreporting is disclosed. Honest managers would try to maintain the public trust placed in them by more careful and responsible work. Additional channels can be corporate culture is influenced by local culture and a culture of high trust can facilitate information sharing and promote good work ethic within all employees. Immoral and desperate individuals who wish to commit fraud in financial reporting will do it regardless of public trust. For these intentional and well-hidden behaviors, public trust has little power of restraint. After all, trust is a soft constraint; formal institutions should never be neglected. We stress the role of good corporate governance without which public trust has limited effects. All the findings alert us to take a prudent and dialectical view toward generalized public trust.

Appendix: Variable Definition

Key Variables

<i>Trust</i>	<p>Generalized public trust in companies. This measure stems from a GSS survey question: “For major companies, as far as the people running them are concerned, would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in them?” For the answers to this question, GSS codes “a great deal” as 1, “only some” as 2, and “hardly any” as 3. We reverse the order of coding for each respondent so that a higher number represents higher trust. Then we take the average across all respondents in the same state and same year. During 1996 to 2006, this question was surveyed every other year. We linearly interpolate the years in between.</p> <p><i>Source: General Social Survey (GSS)</i></p>
<i>Weighted trust</i>	<p>This measure is based on <i>Trust</i>. For each firm-year, we get the institutional holding profile at the fiscal year end. <i>Weighted trust</i> is the average of each institutional investor’s state <i>Trust</i> weighted by their shareholding in the firm.</p> <p><i>Source: General Social Survey (GSS), Thomson Reuters Institutional Holding Database and Massa, Yasuda, and Zhang (2013)</i></p>
<i>Region trust</i>	<p>This measure is similarly constructed as <i>Trust</i>, except that we take the average across all respondents’ answers in the same region-year instead of the same state-year.</p> <p><i>Source: General Social Survey (GSS)</i></p>
<i>Misstatement category</i>	<p>A categorical variable that is equal to 2 if the misstatement is due to irregularities, 1 if due to errors, and 0 if there is no misstatement for a firm-year.</p> <p><i>Source: Hennes et al. (2008)</i></p>
<i>Error</i>	<p>An indicator variable that is equal to 1 if the misstatement is due to errors and 0 otherwise. (Only used in Table 2)</p> <p><i>Source: Hennes et al. (2008)</i></p>
<i>Irregularity</i>	<p>An indicator variable that is equal to 1 if the misstatement is due to irregularities and 0 otherwise. (Only used in Table 2)</p> <p><i>Source: Hennes et al. (2008)</i></p>

Firm Level Controls

<i>Market-adjusted stock return</i>	<p>Annual buy-and-hold return inclusive of delisting returns minus the annual buy-and-hold value-weighted market return.</p> <p><i>Source: CRSP</i></p>
<i>Ln(size)</i>	<p>Natural logarithm of total assets (at) adjusted for inflation.</p> <p><i>Source: COMPUSTAT</i></p>
<i>Negative income</i>	<p>An indicator variable that is equal to 1 if net income (ni) is negative and 0 otherwise.</p> <p><i>Source: COMPUSTAT</i></p>
<i>Leverage</i>	<p>Total debt (dltt + dlc) divided by total assets (at).</p>

Source: COMPUSTAT

<i>ROA</i>	EBIT (oiadp) divided by total assets (at). <i>Source: COMPUSTAT</i>
<i>Foreign operation</i>	An indicator variable equal to 1 if the company has non-zero foreign currency translation (fca) and 0 otherwise. <i>Source: COMPUSTAT</i>
<i>Restructuring</i>	An indicator variable equal to 1 if a firm was involved in a restructuring (any of the following items are non-zero: rcp, rca, rceps, rcd) and 0 otherwise. <i>Source: COMPUSTAT</i>
<i>Number of segments</i>	The number of reported business segments. <i>Source: COMPUSTAT Segment File</i>
<i>Litigious industry</i>	An indicator variable equal to 1 if a firm is in a litigious industry in a given year — SIC codes 2833 to 2836, 3570 to 3577, 3600 to 3674, 5200 to 5961, and 7370 — and 0 otherwise. <i>Source: CRSP and COMPUSTAT</i>
<i>Market to book</i>	Market to book equity. <i>Source: COMPUSTAT and CRSP</i>
<i>Big5</i>	An indicator variable equal to 1 if the firm is audited by one of the five largest audit firms and 0 otherwise. <i>Source: Audit Analytics</i>
<i>Cash flow volatility</i>	The standard deviation of cash flow from operations calculated over past 5 years. <i>Source: COMPUSTAT</i>
<i>Sales volatility</i>	The standard deviation of sales calculated over past 5 years. <i>Source: COMPUSTAT</i>
<i>Stock return volatility</i>	The standard deviation of monthly stock return in a fiscal year. <i>Source: CRSP</i>

Determinants of Trust – State Level

<i>Trust people</i>	Generalized public trust in people. This measure stems from a GSS survey question: “Generally speaking, would you say that most people can be trusted or that you can’t be too careful in dealing with people?” For the answers to this question, I code “can’t be too careful” as 1, “depends” as 2, and “most people can be trusted” as 3. Then I take the average across all respondents in the same state and same year. During 1996 to 2006, this question was surveyed every other year. We linearly interpolate the years in between. <i>Source: General Social Survey (GSS)</i>
<i>Trust court</i>	Generalized public trust in people. This measure stems from a GSS survey question: “Would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in those people who are running the U.S. Supreme Court?” For the answers to this question, I code “hardly any” as 1, “only some” as 2, and “a great deal” as 3. Then I take

the average across all respondents in the same state and same year. During 1996 to 2006, this question was surveyed every other year. We linearly interpolate the years in between.

Source: General Social Survey (GSS)

% Firms restated last year The number of firms which made restatement announcements in the last year divided by the number of all firms in each state.

Source: Hennes et al. (2008) and COMPUSTAT

GDP per capita State level GDP per capita in thousands of dollars.

Source: US Bureau of Economic Analysis (BEA) and US Census Bureau county population estimates

Unemployment rate State level unemployment rate.

Source: US Bureau of Labor Statistics

Market Reaction Sample – Restatement Level

BHAR(-5, +5) Buy-and-hold abnormal return during a window of [-5, +5] around restatement date. The abnormal return is calculated by a market model with an estimation period of [-300, -61]. The market return is value-weighted CRSP return. We require that the minimum estimation period length to be 60 days and use Scholes-Williams (1977) method to adjust for beta estimation.

Source: CRSP

Trust in the restatement year State level trust corresponding to the restatement year.

Source: General Social Survey (GSS)

Irregularity An indicator variable equal to 1 if a restatement is classified as irregularity and 0 if as error.

Source: Hennes et al. (2008)

Number of years restated The number of past years involved in a single restatement.

Source: Hennes et al. (2008), Audit Analytics, and SEC EDGAR

Number of years elapsed The average number of years elapsed from each of the misstatement years to the restatement year. If these two dates happen in the same year, elapse is set to 0.

Source: Hennes et al. (2008), Audit Analytics, and SEC EDGAR

Ln(size) Firm size, as defined previously, but corresponding to the restatement year.

Source: COMPUSTAT

Leverage Debt ratio, as defined previously, but corresponding to the restatement year.

Source: COMPUSTAT

BHAR(-60, -6) Buy-and-hold abnormal return during a window of [-60, -6] around restatement date. The abnormal return is calculated by a market model with an estimation period of [-300, -61]. The market return is value-weighted CRSP return. We require that the minimum estimation period length to be 60 days and use Scholes-Williams (1977) method to adjust for beta estimation.

Source: CRSP

Earnings Management Variables

Accruals

The absolute value of the discretionary accruals calculated using the modified Jones (1991) model. To calculate the discretionary accruals, I first run the following regression using all firms in the same two-digit SIC code in a given year:

$$\frac{TA_{it}}{Assets_{i,t-1}} = k_1 \frac{1}{Assets_{i,t-1}} + k_2 \frac{\Delta Sales_{it}}{Assets_{i,t-1}} + k_3 \frac{PPE_{it}}{Assets_{i,t-1}} + \varepsilon_{it}$$

where TA is the total accruals ($ibc - (oancf - xidoc)$), $Assets$ is the total assets(at), $Sales$ is the total sales (sale), and PPE is the gross plant property and equipment (ppegt). Next, I use the coefficient of the regression in the first step to calculate the discretionary accruals (DA) as follows:

$$NA_{it} = \hat{k}_1 \frac{1}{Assets_{i,t-1}} + \hat{k}_2 \frac{\Delta Sales_{it}}{Assets_{i,t-1}} + \hat{k}_3 \frac{PPE_{it}}{Assets_{i,t-1}}$$

$$DA_{it} = TA_{it}/Asset_{i,t-1} - NA_{it}$$

And *Accruals* is the absolute value of DA .

Source: COMPUSTAT

Real earnings management 1

The sum of the abnormal discretionary expense (AB_DISX) and the abnormal level of production (AB_PROD). Following Cohen and Zarowin (2010), this variable is constructed so that the higher values represent greater real earnings management.

AB_DISX is based on the residual of the following regression:

$$\frac{DISX_{it}}{Assets_{i,t-1}} = k_1 \frac{1}{Assets_{i,t-1}} + k_2 \frac{Sales_{i,t-1}}{Assets_{i,t-1}} + \varepsilon_{it}$$

where $DISX$ is the total discretionary expenses ($xsga + xad + xrd$, note that the advertising xad and the R&D expense xrd should be set to zero as long as the SG&A expense $xsga$ is available), $Assets$ is the total assets(at), and $Sales$ is the total sales (sale). The residuals are then standardized and multiplied by -1 so that higher values represent higher earnings management.

AB_PROD is based on the residual of the following regression:

$$\begin{aligned} \frac{PROD_{it}}{Assets_{i,t-1}} = & k_1 \frac{1}{Assets_{i,t-1}} + k_2 \frac{Sales_{it}}{Assets_{i,t-1}} + k_3 \frac{\Delta Sales_{it}}{Assets_{i,t-1}} \\ & + k_4 \frac{\Delta Sales_{i,t-1}}{Assets_{i,t-1}} + \varepsilon_{it} \end{aligned}$$

where the $PROD$ is the sum of the cost of the goods sold and the change in inventory ($cogs + (inv_t - L.inv_t)$), $Assets$ is the total assets (at), and $Sales$ is the total sales (sales). The residuals are then standardized.

And *Real earnings management 1* is the sum of AB_DISX and AB_PROD .

Source: COMPUSTAT

Real earnings management 2

The sum of the abnormal discretionary expense (*AB_DISX*) and the abnormal level of cash flows (*AB_CASH*). Following Cohen and Zarowin (2010), this variable is constructed so that the higher values represent greater real earnings management.

DISX is calculated as describe above. *AB_CASH* is based on the residual of the following regression:

$$\frac{CFO_{it}}{Assets_{i,t-1}} = k_1 \frac{1}{Assets_{i,t-1}} + k_2 \frac{Sales_{it}}{Assets_{i,t-1}} + k_3 \frac{\Delta Sales_{it}}{Assets_{i,t-1}} + \varepsilon_{it}$$

where the *CFO* is the cash flow from operations (oancf - xidoc), *Assets* is the total assets (at), and *Sales* is the total sales (sale). The residuals are then standardized and multiplied by -1 so that higher values represent higher earnings management.

And *Real earnings management 2* is the sum of *AB_DISX* and *AB_CASH*.
Source: COMPUSTAT

Governance Variables

Big board

An indicator variable equal to 1 if the number of directors is in the top three deciles for the year and 0 otherwise.

Source: RiskMetrics

G index

The governance index with 24 components developed by Gompers et al. (2003).

Source: Legacy IRRC Governance Database

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Figure 1: Average public trust in each state and federal district

Our state level trust measure is depicted in Section 3.1 and defined in Appendix. This figure shows the differing levels of average trust for fifty states and one federal district. The exact number of trust is reported in Table 1. We divide the all the states with non-missing trust into six groups so that each group has a similar number of states and those states share similar trust levels. This figure is drawn with darker shades representing higher trust levels. See the legend for more details.

Figure 2: Time trend of trust variables

Our sample period is from 1996 to 2005. Figure 2a plots the time trend of *Trust*. Figure 2b plots the time trend of *Trust people*. Figure 2c plots the time trend of *Trust court*. In each year, the median value of these three variables across all the states are plotted as a red dot in the figures. These three variables are defined in Appendix.

2a. Median trust in companies

2b. Median trust in people

2c. Median trust in Supreme Court

Figure 3: Time trend of misstatements and restatements

Figure 3a and 3b plot the time trend of financial report misstatements due to errors and irregularities from 1996 to 2005. Figure 3c and 3d plot the time trend of financial report restatements due to errors and irregularities announced from 1997 to 2006.

3a. Incidence rate of financial report misstatements due to errors

3b. Incidence rate of financial report misstatements due to irregularities

3c. Incidence rate of financial report restatements due to errors

3d. Incidence rate of financial report restatements due to irregularities

Figure 4: Market reactions upon restatements

Figure 4a plots the mean and median buy-and-hold abnormal return during a window of $[-60, +60]$ around restatement date for high trust states and low trust states; Figure 4b only includes restatements due to errors; and Figure 4c only includes restatements due to irregularities.

The abnormal return is calculated by a market model with an estimation period of $[-300, -61]$. The market return is value-weighted CRSP return. We require that the minimum estimation period length to be 60 days and use Scholes-Williams (1977) method to adjust for beta estimation.

4a. Market reactions for all restatements

4b. Market reactions for restatements due to errors

4c. Market reactions for restatements due to irregularities

Figure 5: Trust and corporate governance

These two figures plot the interaction term of the corporate governance proxies and our test variable *Trust* using the Ai and Norton (2003) method.

In Figure 5a, the proxy for governance is a dummy variable *Big board*. Specifically, we estimate the following logit model:

$$D(\text{Misstatement}_{i,t}) = \alpha + \beta_1 \text{Trust}_{i,t} + \beta_2 \text{Big board}_{i,t} + \beta_3 \text{Trust}_{i,t} \times \text{Big board}_{i,t} + \gamma X_{i,t} + \delta \text{State}_{i,t} + \theta \text{Year}_t + \varepsilon_{i,t}$$

In Figure 5b, the proxy for governance is *G index*. Specifically, we estimate the following logit model:

$$D(\text{Misstatement}_{i,t}) = \alpha + \beta_1 \text{Trust}_{i,t} + \beta_2 \text{G index}_{i,t} + \beta_3 \text{Trust}_{i,t} \times \text{G index}_{i,t} + \gamma X_{i,t} + \delta \text{State}_{i,t} + \theta \text{Year}_t + \varepsilon_{i,t}$$

In both models, i indexes the firm, t indexes years, and all the variables are defined in the appendix. State and year fixed effects are controlled for. The standard errors of the estimated coefficients are adjusted for heteroskedasticity and allow for clustering of observations by year.

5a. Plot of the interaction between big board dummy and trust

5b. Plot of the interaction between G index and trust

Table 1: Description of GSS survey dataset and our trust measure

This table summarizes the data source of our trust measure – General Social Survey (GSS). We are focusing on state level generalized public trust in companies, which is depicted in Section 3.1 and defined in Appendix. Due to the availability of other variables, we only keep GSS trust data in 1996, 1998, 2000, 2002, 2004 and 2006. This table is ordered by alphabets of the state or federal district name. State name abbreviations are given in the “State” column. Not every state is surveyed in each of the six years. The “Years” column shows the number of years when non-zero participants from this state are surveyed for this question, which can be called “non-missing” years. If there are no participants in any of the six years, we denote it as “No data”. The “Respondents” column shows the average number of participants for the non-missing years. The “Trust” column calculates the average state level trust for the non-missing years. The “Rank” column ranks the “Trust” column from highest to lowest. The last row calculates the average statistics.

	State Name	State	Respondents	Years	Trust	Rank
1.	Alabama	AL	44	6	2.1042	13
2.	Alaska	AK	19	4	1.9603	42
3.	Arizona	AZ	54	6	2.1813	5
4.	Arkansas	AR	39	2	2.0153	34
5.	California	CA	272	6	2.0969	16
6.	Colorado	CO	75	6	2.0664	25
7.	Connecticut	CT	34	6	1.9836	39
8.	Delaware	DE	18	5	1.9972	35
9.	District of Columbia	DC	8	6	2.0167	33
10.	Florida	FL	124	6	2.0603	28
11.	Georgia	GA	114	6	2.2010	2
12.	Hawaii	HI	28	2	2.1810	6
13.	Idaho	ID	139	2	2.0643	26
14.	Illinois	IL	125	6	2.0240	32
15.	Indiana	IN	74	6	2.0805	21
16.	Iowa	IA	83	2	2.1278	9
17.	Kansas	KS	38	6	2.0972	15
18.	Kentucky	KY	23	6	2.1817	4
19.	Louisiana	LA	52	6	2.0567	29
20.	Maine	ME	32	2	1.9048	44
21.	Maryland	MD	48	6	2.1412	7
22.	Massachusetts	MA	70	6	2.0678	24
23.	Michigan	MI	133	6	2.0983	14
24.	Minnesota	MN	46	6	2.2005	3
25.	Mississippi	MS	43	4	2.0756	22
26.	Missouri	MO	77	6	1.9779	41
27.	Montana	MT	22	4	1.9819	40
28.	Nebraska	NE	No data	No data	No data	N.A.
29.	Nevada	NV	No data	No data	No data	N.A.
30.	New Hampshire	NH	No data	No data	No data	N.A.
31.	New Jersey	NJ	79	6	2.0874	19
32.	New Mexico	NM	38	2	1.8466	46
33.	New York	NY	223	6	2.0269	31
34.	North Carolina	NC	147	6	1.9948	37
35.	North Dakota	ND	30	4	2.3387	1
36.	Ohio	OH	141	6	2.0937	17
37.	Oklahoma	OK	41	6	2.0820	20
38.	Oregon	OR	33	6	2.0746	23
39.	Pennsylvania	PA	134	6	1.9840	38
40.	Rhode Island	RI	No data	No data	No data	N.A.
41.	South Carolina	SC	38	6	2.0560	30

42.	South Dakota	SD	19	4	2.1118	12
43.	Tennessee	TN	106	6	2.0632	27
44.	Texas	TX	215	6	2.1208	11
45.	Utah	UT	No data	No data	No data	N.A.
46.	Vermont	VT	35	4	1.9327	43
47.	Virginia	VA	109	6	2.0906	18
48.	Washington	WA	101	6	2.1384	8
49.	West Virginia	WV	40	2	1.8466	45
50.	Wisconsin	WI	58	6	2.1273	10
51.	Wyoming	WY	44	2	1.9952	36
	Average		75	5	2.0642	

Table 2: Determinants of public trust in companies

This table shows how other state level variables drive our trust measure. All variables are introduced in Section 4 and defined in the appendix. The sample time span is 1996-2005. All variables are winsorized at the 1% level at both tails of the distribution. Panel A displays the pairwise correlation coefficients among our trust measure and its determinants, where the column variable names are omitted and represented by numbers. Panel B shows the OLS regression results with the dependent variable being our trust measure and explanatory variables consisting of five state level variables. The symbols ***, **, and * indicate that the correlation or regression coefficients are significant at the 1%, 5%, and 10% levels, respectively.

Panel A: Correlation coefficients

	(1)	(2)	(3)	(4)	(5)	(6)
(1) <i>Trust</i>	1					
(2) <i>Trust people</i>	0.3708***	1				
(3) <i>Trust court</i>	0.2618***	0.1988***	1			
(4) <i>% Firms restated last year</i>	-0.3072***	-0.0547	-0.1432**	1		
(5) <i>GDP per capita</i>	-0.2381***	-0.0457	0.0461	-0.1268*	1	
(6) <i>Unemployment rate</i>	-0.2169***	-0.0418	-0.0567	0.0083	-0.0580	1

Panel B: OLS Regressions

	<i>Trust</i>	<i>Trust</i>
<i>Trust people</i>	0.195*** (0.048)	0.178*** (0.048)
<i>Trust court</i>	0.147* (0.080)	0.173** (0.086)
<i>% Firms restated last year</i>	-0.501*** (0.078)	-0.295** (0.136)
<i>GDP per capita</i>	-0.006*** (0.001)	-0.005*** (0.001)
<i>Unemployment rate</i>	-3.375*** (0.641)	-2.230*** (0.848)
Year FE	N	Y
N	233	233
(Adj.) R ²	0.3733	0.4293

Table 3: Summary statistics

This table summarizes major variables in this paper, all of which are introduced in Section 3 and defined in the appendix. The sample time span is 1996-2005. All variables are winsorized at the 1% level at both tails of the distribution. Columns headed by “All” report the number of observations, mean and standard deviation of the full sample. We further divide the full sample by the median of *Trust*, and report subsample number of observations and mean in columns headed by “Low trust” and “High trust” respectively. *T*-tests based on unequal variances are conducted to test for differences in mean values between the high and low *trust* subsamples. The symbols ***, **, and * indicate that subsample means are significantly different from each other at the 1%, 5%, and 10% levels, respectively.

variable	All			Low trust		High trust	
	N	Mean	Std.Dev	N	Mean	N	Mean
<i>Trust</i>	18354	2.095	0.094	9379	2.022	8975	2.172***
<i>Misstatement</i>	18354	0.055	0.228	9379	0.069	8975	0.041***
<i>Misstatement category</i>	18354	0.073	0.322	9379	0.089	8975	0.056***
<i>Error</i>	18354	0.037	0.189	9379	0.049	8975	0.025***
<i>Irregularity</i>	18354	0.018	0.132	9379	0.020	8975	0.015**
<i>Market-adjusted stock return</i>	18354	0.040	0.963	9379	0.111	8975	-0.035***
<i>Ln(size)</i>	18354	5.495	1.911	9379	5.577	8975	5.409***
<i>Negative income</i>	18354	0.340	0.474	9379	0.350	8975	0.329***
<i>Leverage</i>	18354	0.194	0.187	9379	0.177	8975	0.210***
<i>ROA</i>	18354	0.007	0.245	9379	0.002	8975	0.012***
<i>Foreign operation</i>	18354	0.161	0.367	9379	0.188	8975	0.132***
<i>Restructuring</i>	18354	0.106	0.308	9379	0.186	8975	0.022***
<i>Number of segments</i>	18354	2.074	1.682	9379	2.137	8975	2.008***
<i>Litigious industry</i>	18354	0.300	0.458	9379	0.307	8975	0.292**
<i>Market to book</i>	18354	3.653	5.395	9379	3.620	8975	3.688
<i>Big5</i>	18354	0.862	0.345	9379	0.839	8975	0.887***
<i>Cash flow volatility</i>	18354	0.413	2.089	9379	0.473	8975	0.350***
<i>Sales volatility</i>	18354	0.362	0.304	9379	0.344	8975	0.381***
<i>Stock return volatility</i>	18354	0.171	0.104	9379	0.162	8975	0.180***

Table 4: State level public trust and misstatements

This table consists of two sets of regressions showing the effects of state level public trust on financial report misstatements. The first regression is a logit model with the dependent variable being *Misstatement*. The second regression is a multinomial logit model with the dependent variable being *Misstatement category*. The sample time span is 1996-2005. All the variables are introduced in Section 3 and defined in the appendix. The standard errors in parentheses are Huber/White/Sandwich heteroskedastic-consistent errors and allowing for clustering of observations by year. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	<i>Misstatement</i>	<i>Misstatement category</i>	
		<i>Error</i>	<i>Irregularity</i>
<i>Trust</i>	-0.616** (0.312)	-1.309*** (0.237)	1.201 (0.826)
<i>Market-adjusted stock return</i>	-0.017 (0.043)	0.026 (0.057)	-0.102 (0.073)
<i>Ln(size)</i>	0.148*** (0.029)	0.092*** (0.016)	0.263*** (0.054)
<i>Negative income</i>	0.259*** (0.099)	0.352*** (0.110)	0.066 (0.157)
<i>Leverage</i>	0.020 (0.156)	-0.059 (0.257)	0.159 (0.322)
<i>ROA</i>	0.845** (0.340)	1.020** (0.483)	0.529* (0.321)
<i>Foreign operation</i>	0.171** (0.085)	0.052 (0.114)	0.414*** (0.095)
<i>Restructuring</i>	-0.110 (0.156)	-0.337 (0.228)	0.442*** (0.164)
<i>Number of segments</i>	-0.020 (0.025)	0.001 (0.024)	-0.070* (0.042)
<i>Litigious industry</i>	0.464*** (0.074)	0.476*** (0.086)	0.440*** (0.090)
<i>Market to book</i>	0.008** (0.003)	-0.006 (0.010)	0.023*** (0.006)
<i>Big5</i>	0.421*** (0.157)	0.511*** (0.193)	0.266 (0.196)
<i>Cash flow volatility</i>	-0.025 (0.039)	-0.000 (0.040)	-0.080 (0.057)
<i>Sales volatility</i>	-0.278* (0.158)	-0.693*** (0.205)	0.413** (0.175)
<i>Stock return volatility</i>	0.555 (0.506)	0.102 (0.638)	1.460** (0.664)
State FE	Y		Y
Year FE	Y		Y
N	18354		18354
Pseudo R ²	0.0752		0.0798
[1]trust = [2]trust		Chi ² (1) = 11.60 (p = 0.0007)	

Table 5: Idiosyncratic state trust and misstatements

This table reports the second stage results of two-stage estimations, with the first stage results reported as the second specification in Table 2 Panel B. The independent variables in the second stage regressions (this table) are predicted from the first stage results. I take the residual trust as the idiosyncratic trust for each state. This table also consists of two sets of regressions. The first regression is a logit model with the dependent variable being *Misstatement*. The second regression is a multinomial logit model with the dependent variable being *Misstatement category*. The sample time span is 1998-2005. All the variables are introduced in Section 4 and defined in the appendix. Other control variables which appeared in Table 3 are also included for estimation but omitted for exhibition. The standard errors in parentheses are calculated from 500 bootstrap replications of the two-step estimations based on resampling from the data set with replacement of state clusters. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	<i>Misstatement</i>	<i>Misstatement category</i>	
		<i>Error</i>	<i>Irregularity</i>
<i>Trust (residual)</i>	-0.849* (0.473)	-1.355** (0.658)	0.153 (0.861)
<i>Trust (predicted by Trust people)</i>	1.658 (2.341)	3.252 (2.589)	-0.818 (4.788)
<i>Trust (predicted by Trust court)</i>	2.651 (2.592)	1.952 (3.912)	4.700 (4.611)
<i>Trust (predicted by % Firms restated last year)</i>	5.070 (5.991)	1.678 (7.010)	18.877 (13.170)
<i>Trust (predicted by GDP per capita)</i>	2.195 (9.728)	4.936 (13.576)	-9.754 (15.423)
<i>Trust (predicted by Unemployment rate)</i>	4.888 (5.758)	8.504 (7.196)	-6.797 (8.903)
State FE	Y	Y	Y
Year FE	Y	Y	Y

Table 6: Weighted average trust of institutional investors and misstatements

This table is similar to Table 3, except that firm headquarter state trust is replaced with a weighted average measure which takes account of the local trust level for each documented institutional investor and their shareholding amount. In this table, the first regression is a logit model with the dependent variable being *Misstatement*. The second regression is a multinomial logit model with the dependent variable being *Misstatement category*. The sample time span is 1996-2005. All the variables are defined in the appendix. The standard errors in parentheses are Huber/White/Sandwich heteroskedastic-consistent errors and allowing for clustering of observations by year. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	<i>Misstatement</i>	<i>Misstatement category</i>	
		<i>Error</i>	<i>Irregularity</i>
<i>Weighted Trust</i>	-2.344** (1.128)	-3.229** (1.262)	0.518 (2.838)
<i>Market-adjusted stock return</i>	0.017 (0.072)	0.021 (0.076)	-0.007 (0.098)
<i>Ln(size)</i>	0.108* (0.056)	0.032 (0.048)	0.270*** (0.070)
<i>Negative income</i>	0.275*** (0.104)	0.342** (0.134)	0.063 (0.161)
<i>Leverage</i>	-0.267 (0.229)	0.172 (0.207)	-0.687** (0.321)
<i>ROA</i>	0.594 (0.452)	0.771 (0.547)	0.169 (0.252)
<i>Foreign operation</i>	0.204* (0.110)	0.082 (0.095)	0.341** (0.138)
<i>Restructuring</i>	0.106 (0.177)	-0.219 (0.204)	0.675*** (0.223)
<i>Number of segments</i>	-0.015 (0.032)	-0.017 (0.034)	-0.036 (0.047)
<i>Litigious industry</i>	0.410*** (0.121)	0.382** (0.150)	0.386*** (0.134)
<i>Market to book</i>	0.003 (0.006)	-0.012 (0.008)	0.019** (0.010)
<i>Big5</i>	0.339* (0.183)	0.485** (0.236)	0.031 (0.221)
<i>Cash flow volatility</i>	-0.057 (0.057)	-0.020 (0.055)	-0.360** (0.149)
<i>Sales volatility</i>	-0.618*** (0.209)	-0.787*** (0.248)	-0.095 (0.298)
<i>Stock return volatility</i>	0.610 (0.939)	0.357 (0.963)	0.932 (1.030)
State FE	Y		Y
Year FE	Y		Y
N	9060		9060
Pseudo R ²	0.0800		0.0723
[1]trust = [2]trust		Chi ² (1) = 1.46 (p = 0.2267)	

Table 7: Region trust and misstatements

This table is similar to Table 3, except that state level trust is replaced with region level trust. In this table, the first regression is a logit model with the dependent variable being *Misstatement*. The second regression is a multinomial logit model with the dependent variable being *Misstatement category*. The sample time span is 1996-2005. All the variables are defined in the appendix. The standard errors in parentheses are Huber/White/Sandwich heteroskedastic-consistent errors and allowing for clustering of observations by year. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	<i>Misstatement</i>	<i>Misstatement category</i>	
		<i>Error</i>	<i>Irregularity</i>
<i>Region Trust</i>	-1.125*** (0.332)	-1.821*** (0.394)	0.562 (0.688)
<i>Market-adjusted stock return</i>	0.046* (0.024)	0.069* (0.037)	-0.006 (0.058)
<i>Ln(size)</i>	0.136*** (0.025)	0.078*** (0.019)	0.263*** (0.038)
<i>Negative income</i>	0.229** (0.090)	0.280*** (0.084)	0.110 (0.201)
<i>Leverage</i>	0.176 (0.138)	0.014 (0.212)	0.524*** (0.154)
<i>ROA</i>	0.692*** (0.212)	0.871*** (0.292)	0.279 (0.297)
<i>Foreign operation</i>	0.176** (0.085)	0.052 (0.097)	0.440*** (0.078)
<i>Restructuring</i>	-0.097 (0.102)	-0.253* (0.154)	0.308*** (0.084)
<i>Number of segments</i>	0.001 (0.016)	-0.002 (0.016)	0.006 (0.022)
<i>Litigious industry</i>	0.525*** (0.079)	0.546*** (0.088)	0.478*** (0.093)
<i>Market to book</i>	-0.011** (0.004)	-0.014 (0.008)	-0.005 (0.006)
<i>Big5</i>	0.355** (0.158)	0.412*** (0.139)	0.299 (0.246)
<i>Cash flow volatility</i>	-0.059* (0.033)	-0.027 (0.032)	-0.154** (0.071)
<i>Sales volatility</i>	0.015 (0.106)	-0.366*** (0.141)	0.715*** (0.157)
<i>Stock return volatility</i>	0.592** (0.245)	0.097 (0.368)	1.636*** (0.478)
Region FE	Y		Y
Year FE	Y		Y
N	31794		31794
Pseudo R ²	0.0738		0.0757
[1]trust = [2]trust		Chi ² (1) = 9.27 (p = 0.0023)	

Table 8: Market reactions upon restatements' initial announcements

This table reports the market reactions for the initial disclosures of restatements during 1997-2006. Panel A includes all the 1100 restatements, Panel B includes restatements due to errors, and Panel C includes restatements due to irregularities. In each panel, we only list market reactions for six event windows which are narrowing one by one. We divide all restatements by looking at whether their filing year's state public trust level is above the median or not, and report the number of observations, the mean and median buy-and-hold abnormal return for high trust states and low trust states respectively. The abnormal return is calculated by a market model with an estimation period of [-300, -61]. The market return is value-weighted CRSP return. We require that the minimum estimation period length to be 60 days and use Scholes-Williams (1977) method to adjust for beta estimation. *T*-tests based on unequal variance are conducted to test for differences in mean values between the high and low trust subsamples. Wilcoxon rank-sum tests are conducted to test for differences in median values between the high and low trust subsamples. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: All						
BHAR Window	N		Mean (%)		Median (%)	
	Low trust	High trust	Low trust	High trust	Low trust	High trust
(-60, +60)	503	507	-13.12	-21.36**	-9.54	-15.21**
(-30, +30)	502	507	-8.84	-12.32*	-7.45	-11.41**
(-20, +20)	502	507	-7.11	-10.24*	-5.03	-7.19*
(-10, +10)	502	507	-4.23	-7.06**	-2.70	-4.15**
(-5, +5)	501	506	-3.46	-6.70***	-1.87	-3.93***
(-1, +1)	501	506	-2.59	-4.97***	-1.05	-2.73***

Panel B: Error						
BHAR Window	N		Mean (%)		Median (%)	
	Low trust	High trust	Low trust	High trust	Low trust	High trust
(-60, +60)	375	368	-8.19	-16.74**	-5.40	-13.25**
(-30, +30)	374	368	-5.83	-8.75	-4.64	-8.87*
(-20, +20)	374	368	-4.25	-7.29	-3.65	-5.67
(-10, +10)	374	368	-2.67	-4.16	-1.49	-2.31
(-5, +5)	374	368	-1.72	-4.15**	-1.28	-2.70**
(-1, +1)	374	368	-1.11	-3.39***	-0.56	-2.26***

Panel C: Irregularity						
BHAR Window	N		Mean (%)		Median (%)	
	Low trust	High trust	Low trust	High trust	Low trust	High trust
(-60, +60)	128	139	-27.54	-33.60	-20.86	-23.82
(-30, +30)	128	139	-17.61	-21.79	-14.44	-17.45
(-20, +20)	128	139	-15.46	-18.05	-9.78	-12.40
(-10, +10)	128	139	-8.79	-14.77**	-6.37	-8.77*
(-5, +5)	127	138	-8.57	-13.50**	-4.94	-7.00
(-1, +1)	127	138	-6.94	-9.18	-2.43	-4.71

Table 9: Regressions of market reactions on trust

This table reports the multivariate analysis of the relation between market reaction and public trust in the restatement filing year during 1997-2006. All the variables in this table are defined in the appendix. There are 946 observations with nonmissing values for all the variables. Panel A summarizes the mean, standard deviation and median for all variables. Panel B reports the OLS regression result when the dependent variable is *BHAR*(-5, +5). The standard errors in parentheses are Huber/White/Sandwich heteroskedastic-consistent errors. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Summary statistics			
	Mean	Std.Dev	Median
<i>BHAR</i> (-5, +5)	-0.050	0.154	-0.027
<i>Trust in the restatement year</i>	2.037	0.136	2.041
<i>Irregularity</i>	0.264	0.441	0.000
<i>Number of years restated</i>	2.122	1.454	2.000
<i>Number of years elapsed</i>	0.867	0.818	0.630
<i>Ln(size)</i>	5.986	1.793	5.876
<i>Leverage</i>	0.198	0.182	0.169
<i>BHAR</i> (-60, -6)	-0.048	0.294	-0.039

Panel B: OLS regression			
	All	Error	Irregularity
<i>Trust in the restatement year</i>	-0.203* (0.122)	-0.529*** (0.143)	-0.052 (0.419)
<i>Irregularity</i>	-0.111*** (0.036)		
<i>Number of years restated</i>	-0.021 (0.017)	0.015 (0.021)	-0.066 (0.052)
<i>Number of years elapsed</i>	0.021 (0.027)	-0.027 (0.029)	0.077 (0.068)
<i>Ln(size)</i>	0.004 (0.033)	-0.004 (0.036)	-0.061 (0.181)
<i>Leverage</i>	-0.186 (0.156)	0.030 (0.154)	0.261 (1.187)
<i>BHAR</i> (-60, -6)	0.015 (0.045)	0.011 (0.050)	0.217 (0.176)
Firm FE	Y	Y	Y
Year FE	Y	Y	Y
N	946	696	250
R ²	0.1874	0.1856	0.3117

Table 10: State trust and earnings management

This table reports the OLS regression results of three earnings management measures on trust. The sample time span is 1996-2005. All the variables in this table are introduced in Section 3 and defined in the appendix. In Columns (1), (2) and (3), the dependent variables are *Accruals*, *Real earnings management 1*, and *Real earnings management 2*, respectively. The number of observations varies with the availability of dependent variables. The standard errors in parentheses are Huber/White/Sandwich heteroskedastic-consistent errors. The symbols ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	<i>Accrual quality</i>	<i>Real Management 1</i>	<i>Real Management 2</i>
<i>Trust</i>	-0.024** (0.008)	-0.061 (0.045)	-0.043 (0.037)
<i>Market-adjusted stock return</i>	0.005*** (0.001)	-0.010 (0.008)	-0.019*** (0.004)
<i>Ln(size)</i>	-0.007*** (0.000)	0.027*** (0.002)	0.008*** (0.001)
<i>Negative income</i>	0.001 (0.002)	0.015 (0.013)	0.033*** (0.009)
<i>Leverage</i>	0.004 (0.005)	0.225*** (0.030)	0.188*** (0.018)
<i>ROA</i>	0.001 (0.004)	-0.069* (0.031)	-0.171*** (0.022)
<i>Foreign operation</i>	-0.001 (0.001)	-0.032* (0.016)	-0.019* (0.009)
<i>Restructuring</i>	-0.003* (0.002)	-0.002 (0.009)	0.005 (0.006)
<i>Number of segments</i>	0.002*** (0.000)	0.014*** (0.001)	0.010*** (0.001)
<i>Litigious industry</i>	0.001 (0.001)	-0.002 (0.021)	-0.007 (0.016)
<i>Market to book</i>	0.001*** (0.000)	-0.017*** (0.002)	-0.011*** (0.001)
<i>Big5</i>	-0.005** (0.002)	-0.080*** (0.007)	-0.043*** (0.006)
<i>Cash flow volatility</i>	-0.002** (0.001)	-0.002 (0.007)	0.003 (0.003)
<i>Sales volatility</i>	0.049*** (0.006)	-0.030 (0.019)	-0.094*** (0.011)
<i>Stock return volatility</i>	0.073*** (0.009)	-0.038 (0.049)	0.002 (0.043)
State FE	Y	Y	Y
Year FE	Y	Y	Y
N	15934	16116	16133
R ²	0.1236	0.1011	0.1276

Essay Two

How Do Private Firms Allocate Their Cash Flow?

Abstract

We investigate how private firms allocate internally generated cash flow by estimating the cash flow sensitivities of various uses of cash flow. Our results reveal that, compared with public firms, private firms allocate less cash flow to investments and cash saving, and direct more to dividends and reduction of external finance, especially debt. The above pattern remains robust if we apply various matching schemes or if we only look at the transitory cash flow shock. To find out what is the driving force of private firms' cash flow allocation, we attempt to draw parallel evidence from public firms, but find that the analysis within public firms cannot be applied mechanically to private firms. Special features of private firms – more concentrated equity ownership, more powerful shareholders with a strong desire for liquidity and diversification, and higher agency costs associated with debt finance are very likely to play a vital role in shaping their cash flow allocation.

JEL Classification: G31, G32, G35

Keywords: Cash Flow Sensitivity; Internal Cash Flow; Private Firms

1. Introduction

Cash flow from daily operating activities is one of the major funding sources for multiple purposes: to pay out dividends, to reduce debt or equity financing, to replenish precautionary cash savings, or to spend more on investments. Chang, Dasgupta, Wong, and Yao (2014) has done a creative study to show the interrelated and competitive nature of various uses of cash flow for public firms, but will private firms exhibit some different traits in cash flow allocation?

This question deserves attention at least for the following reasons. First, private firms are economically important in the US, but little is known about private firms' cash flow allocation behavior. According to the estimation by Asker et al. (2015), in 2010, private firms accounted for 52.8% of aggregate non-residential fixed investment and 68.7% of private sector employment in the US. 86.4% of those larger US firms with more than 500 employees are privately held. We all know that private firms are essentially different from public firms in the broadness and connectedness of shareholders; so examining private firms' cash flow allocation can shed some light on the theories of how financing structure and concentrated ownership affect daily operational decisions. Specifically, we are interested in: To what extent and in which aspects private firms allot their cash flows differently from public firms? And what is the mechanism?

Second, this issue has important implications to macroeconomic policies. Both fiscal and monetary policies are extensively examined on the macro level, and many of these studies are concerned about whether these policies can boost investment and growth.¹ Actually most policies, such as tax cuts, interest cuts, and government

¹ For example, Alesina and Ardagna (2010) find that tax cuts are more effective in boosting GDP growth than government spending, Easterly and Rebelo (1993) find that public investment in transport and communication is positively correlated with per capita GDP growth, Christiano, Eichenbaum, and

purchases, are installed to directly raise firms' profits and cash flow. Knowing how much of the increase in cash flow can stream into investment on the micro level is essential. One of the relevant papers about the micro level effect of public policies is Dambra (2014), who uses a 2012 change in the ERISA pension rule to document the unintended policy consequences. Although Dambra is not examining cash flow sensitivities, but his findings are illuminating for our research: after an exogenous shock of mandatory pension contribution decrease, there is no immediate increase in public firms' capital expenditures, R&D expenses, acquisitions, instead, recipients of pension funding relief respond to the shock by increasing shareholder and debtholder payouts etc., and this effect is more prevalent among financially constrained firms. Thus, examining private firms' cash flow allocation has both theoretical importance and policy implications.

Following Chang et al. (2014), we define cash flow and its uses in such a way that the sum of all uses of cash flow must equal cash flow itself (*cash flow identity*). Then we analyze the cash flow allocation by looking at the coefficients of cash flow in a series of regressions where various uses of cash flow are separately regressed on cash flow and other controls. The cash flow sensitivity of a particular use of cash flow reveals how much of an additional dollar of cash flow is directed towards that use. Cash flow identity implicitly guarantees that the cash flow sensitivities of various uses of cash flow must add up to unity.

To explore the cash flow allocation of private firms, we obtain data from Capital IQ database, which is frequently used by recent papers (e.g., Gao et al., 2013; Asker et al., 2015). We utilize a comprehensive dataset of large private firms and public firms from 1993 to 2012, and add an interaction term of private firm indicator and cash flow

Evans (2005) show that after an expansionary monetary policy shock, investment, productivity, profits, etc, respond in a hump-shaped fashion, peaking after about 1.5 to 2 years.

into the regression framework of Chang et al. (2014), so that we are able to highlight the different allocation patterns of private firms and public firms. Our results show that compared with public firms in the same industry and with similar size, private firms spend less cash flow on investments and cash holdings, more on dividends, and they use more of the total cash flow to reduce external finance, especially debt. On the aggregate, public firms use around 40% of cash flow to reduce external finance and around 60% to support other uses. Nevertheless, private firms use more than 65% of cash flow to reduce external finance but only less than 35% on other items. Besides, independent regressions using private firm subsample and public firm subsample are consistent with this finding.

A common critique of interpreting cash flow coefficients is that cash flow may be correlated with unobservable factors which capture investment opportunities (e.g., Kaplan and Zingales, 1997; Erickson and Whited, 2000; Bushman et al., 2012; Damdra, 2014). Following Chang et al. (2014), we employ time series filters to decompose each firm's cash flow into a trend (permanent) component and a cycle (transitory) component. The trend part is associated with growth prospects, which is sustainable. The cycle part will be transitory windfalls or shortfalls of cash flow, which is an exogenous shock to a firm's internal funds independent of its investment opportunity set. We want to know how private firms respond to an exogenous cash flow shock. Thus, we incorporate both components and their interactions with the private firm indicator into our integrated regression models of cash flow allocation. We find that private firms' overall cash flow allocation pattern remains with the cycle part of cash flow, even though the investment-cycle cash flow sensitivity becomes insignificant.

Previous industry-size matched sample shows that for a one-dollar increase in

cash flow, private firms allocate 11 cents less than public firms to investments, 14 cents more to cash holdings, 2 cents more to dividends, 16 cents more to net debt retirement, and 7 cents more to equity repurchase. Further investigation shows that this contrasting pattern does not disappear if we match each private firm to a public firm with a similar leverage ratio in addition to similar size and same industry, indicating that the greater amount of debt level for private firms cannot fully explain the above pattern. This difference in cash flow allocation style persists for various alternative schemes of matching private firms and public firms.

To explain private firms' cash flow allocation style, there are two plausible channels. The first one is that private firms have more financing frictions (see Brav, 2009; Kovner and Wei, 2012; Maksimovic et al., 2013; Mortal and Reisel, 2014; Phillips and Sertsios, 2014; Gilje and Taillard, 2016). Our finding of lower investment-cash flow sensitivity for private firms is consistent with Gilje and Taillard (2016) and Phillips and Sertsios (2014) who document that private firms respond less than public firms to positive changes in common investment opportunities and attribute this to their differences in access to external capital. Maksimovic et al. (2013) also show that public firms participate more than private firms as buyers in merger waves due to better access to capital. Brav (2009) finds that public firms increase their investments as soon as their profits increase, while private firms do so with a lag. Earlier studies of Kaplan and Zingales (1997) and Cleary (1999) have parallel evidence in public firms that more constrained public firms have a lower investment-cash flow sensitivity than the less constrained ones. Hennessy and Whited (2007) argue that for large and small public firms alike, costly external funds depresses the path of investment. Thus we posit that financing frictions can affect private firms' decision as for how much cash flow to be deployed on investments.

An alternative hypothesis, which is not mutually exclusive with financing frictions, is that private firms display the above mentioned cash flow allocation pattern because they have less agency problems. Jensen (1986) and Harford (1999) document that firms with more free cash flow tend to make value decreasing acquisitions due to agency costs, so private firms spend less cash flow on investments perhaps because their managers and shareholders have more aligned interests. La Porta, Lopez-de-Silanes, Shleifer, and Vishny (2000) and many other papers argue that firms with more powerful shareholders can pressure the management to disgorge cash as dividends, which will be otherwise diverted for managerial perquisites or unprofitable projects. Private firms generally have more concentrated ownership and more powerful shareholders, so managers of private firms should be more active in rewarding shareholders instead of intercepting the cash flows within their own control. Gao et al. (2013) demonstrate that private firms hold less cash than public firms due to agency problems of public firms, which appears consistent with the lower cash holding-cash flow sensitivity of private firms.

We do not have the data to compare private firms and public firms' financing frictions or agency problems, so we can only verify this hypothesis by looking at how financial constraints or agency problems affect public firms' cash flow allocation. Specifically, we only keep the subsample of public firms, and create dummy variables which take one if the public firm is more financially constrained or less likely to suffer from agency problems. By observing the interaction terms between these dummy variables and public firms' cash flow, we find that more constrained public firms or public firms with fewer agency problems do not always show expected differences in cash flow allocation compared with their counterparties. This suggests that directly drawing conclusions from public firms is not a good strategy to answer

questions related to private firms. Further discussion alludes that private firms' concentrated ownership may be important in explaining their behavior.

Our paper contributes to the literature by being the first to investigate the cash flow sensitivities (allocation) of the U.S. private firms and to explain the phenomenon. Previous studies about cash flow sensitivities are limited to public firms and show mixed results (e.g., Fazzari, Hubbard, and Petersen, 1988; Kaplan and Zingales, 1997; Almeida et al., 2004; Almeida and Campello, 2010; Chang et al., 2014). By looking at the cash flow allocation of private firms, we also join a recent surge of papers studying different aspects of private firms (e.g., Brav, 2009; Kovner and Wei, 2012; Michaely and Roberts, 2012; Maksimovic et al., 2013; Gao et al., 2013; Badertscher et al., 2013; Mortal and Reisel, 2014; Phillips and Sertsios, 2014; Asker et al., 2015). The distinct style of private firms' cash flow allocation also adds evidence to the emerging literature which refutes the traditional view that cash should be viewed as negative debt and it is a matter of indifference whether a firm uses internal resources to accumulate cash or repay debt (e.g., Acharya et al., 2007; Bolton et al., 2014).

The rest of the paper is organized as follows. Section 2 describes our sample and variables. Section 3 presents the empirical results. Section 4 discusses the possible mechanisms driving private firms' cash flow allocation pattern. Section 5 concludes.

2. Data and variables

2.1. Initial full sample formation

Our primary data source is the Capital IQ database. We start with all firms tagged by "public" or "private" and headquartered in the US from 1993 to 2012. We follow the convention to remove financial and utility firms.² During the process of handling

² In unreported tables, we also follow Almeida and Campello (2010), Chang et al. (2014) to remove not-for-profit organizations and governmental enterprises (SICs greater than 8000), this will

data, there are quite a few issues that need to be clarified. One of the biggest complications is that Capital IQ only provides the current listing status of the company. If a company that was once public has undergone a delisting from an exchange and no other common stock is now trading, the company is flipped to "private", which means if you collect the historical information of all firms under the "private" tag, this firm will be one of them, and this so-called "private" firm will have very rich historical financial data, only because it was reporting to SEC regularly as a public firm. In the same vein, a firm tagged as "public" may contain financials in the years when it was private. We figure out a way to restore the historical company listing status as follows.

First, we move those firm-year observations whose market capitalization on or closest to the fiscal year end date is greater than 0, from the private firm subsample into the public firm subsample. Second, we move those firm-year observations whose fiscal year end predates the first trade date of the firm's stock, from the public firm subsample to the private firm subsample. Later we notice that first trade date in the second step only refers to the most recent primary listing beginning date, and before this date, there were going private transactions and even earlier listing date(s), so the second step may wrongly move some public firm-years into the private firm subsample. To recover the true status history of firms which have changed listing status more than once, in the third step, we pull out the first stock trading dates of all past listings for each firm which ever went through going private transactions, and rectify mistaken categorizations by comparing dates.

All private firms in the database are also reporting firms. According to the SEC website, private firms are required to file with SEC if 1) they have more than \$10

not change our results.

million of assets and a class of equity securities held of record by 500 or more holders at the end of its fiscal year, or 2) they have issued public debts.³ If some private firms are included in our sample only because they file reports due to public debts, we will be unduly selecting high leverage firms. That said, we only keep firms with more than \$10 million of assets, whether it be private or public.⁴ This makes our private company observations very different from those observations extracted from Sagedworks database as in Asker et al. (2015), because the private firms in my sample are among the largest companies. The downside is that my sample is not representative of average private firms, and my results in this paper might not be generalized to all private firms. The upside is that the private firms in my sample are comparable to public firms in terms of disclosure and information asymmetry, and it is easier to match these private firms with public firms of similar sizes. Another feature worth noting is that, even though many of them have some access to the bond markets, they are still more constrained than public firms due to difficulty to raise new equity.

For public firms we only keep common stock type; units, ADRs, REITs, mutual funds, etc, are screened out. Also we make sure that all financials are measured in third quarter of 2009 U.S. dollars. Following Almeida and Compello (2010) and Chang et al. (2014), we require that firm annual sales exceed \$1 million so as to minimize the sampling of distressed firms. We also eliminate firm-years with asset growth greater than 100% or less than -50% to avoid major corporate events such as mergers and acquisitions.

³ 500 shareholders was the threshold before Jumpstart Our Business Startups Act (“JOBS Act”). The JOBS Act has amended Section 12(g) of the Exchange Act to increase the holders of record threshold that triggers Exchange Act registration to: 1) 2000 persons, or 2) 500 persons or more who are not “accredited investors” as defined under applicable SEC rules.

⁴ Our results are essentially unchanged if we reduce the \$10 million asset threshold to \$5 million or \$1 million.

2.2. Main variables definition

Chang et al. (2014) has formulated the *cash flow identity* as follows:

$$Inv_t + \Delta Cash_t + Div_t - \Delta D_t - \Delta E_t = CF_t, \quad (1)$$

where the uses of funds include investment (Inv), the change in cash holdings ($\Delta Cash$), and cash dividends (Div). The sources of funds comprise the internally generated cash flow (CF) and external financing that consists of the net debt issuance (ΔD) and the net equity issuance (ΔE). $-\Delta D$ and $-\Delta E$ represent net reductions in external financing, which are regarded as uses of funds.

In this paper, we replace $-\Delta D$ with $nDREP$ (net debt repayment) and replace $-\Delta E$ with $nEREP$ (net equity repurchase) for the ease of exhibition. Hence, we have the following identity:

$$Inv_t + \Delta Cash_t + Div_t + nDREP_t + nEREP_t = CF_t \quad (2)$$

Capital IQ has conveniently assembled cash flow statement data which conform to cash flow identity. Cash flow statement items which produce positive net change in cash are given a positive sign. We define variables in equation (2) in such a way that is congruent with Chang et al. (2014). Table 1 Panel A describes how we use Capital IQ items to define the raw items in Equation (2), which are further scaled by lagged total assets. We will use only the scaled variables hereafter. Table 1 Panel B lists our definition of major control variables, which are chosen following Chang et al. (2014).

All variables are winsorized at the top and bottom 1% of their distribution. We revisit the cash flow identity using scaled and winsorized variables, and exclude observations for which the absolute value of the difference between the left-hand side and right-hand side of Equation (2) is greater than 1%. At last, our full sample consists of 54782 public firm-year observations representing 7609 unique public firms, and

9575 private firm-year observations representing 2962 unique private firms.

Table 2 Panel A describes our full sample. In a univariate analysis, there is no obvious evidence about the difference of investment and cash saving activities between private firms and public firms. Although private firms seem to invest more on the mean, but less on the median. Average or median cash change for both types of companies are close to 0. Private firms have a higher dividend payout level on the mean. One of the most salient characteristics is that the average net debt issuance of private firms is 2.4 times more than public firms, and their net equity issuance is similar to public firms. So we are not surprised that private firms have a much higher mean (median) leverage ratio. It is also worth mentioning that in our sample, private firms are not small firms, their mean and median total assets and tangible assets are even greater than public firms. But public firms in our sample are more profitable, since they have higher mean (median) cash flows. Overall, private firms inherently differ very much from public firms in many ways.

2.3. Industry-size matched sample

To make private firms and public firms in our sample more homogeneous, we follow the literature to match two types of firms by industry and size.⁵ To facilitate our future use of time series filter which will be introduced in Section 3.2, we first require that throughout the consecutive period each private firm lasts, the matching public firm must also exist in our sample and come from the same 2 digit SIC industry. Then we require that during this common period, the average firm size (total assets) of the matching public firm is closest to that of the private firm, and the ratio of their total assets should be within 0.5 to 2. This procedure helps to find matches for 9014

⁵ It is very difficult to match each characteristic of private firms and public firms, since they are inherently different. Industry-size match is extensively used and widely accepted in literature.

private firm-years with replacement. Table 2 Panel B shows that after matching, public firms' size is indistinguishable to private firms, and public firms' market to book ratio, tangibility and leverage are also getting closer to private firms.

We also try many other matching methods, but will not tabulate the summary statistics here. None of the matching methods can perfectly eliminate the differences in all characteristics between private firms and public firms. These alternative matching schemes are discussed in Section 3.

3. Private firms' cash flow allocation pattern

3.1. Baseline results

Based on Chang et al. (2014), we use the following regression system in our paper:

$$Y_{i,t}^k = \beta_1^k \text{Private}_{i,t} + \beta_2^k \text{Private}_{i,t} \times CF_{i,t} + \beta_3^k CF_{i,t} + \beta_X^k X_{i,t-1} + f_i^k + y_t^k + \varepsilon_{i,t}^k$$

where $k = 1, 2, 3, 4, 5$ and

$$Y^1 = \text{Inv}, Y^2 = \Delta \text{Cash}, Y^3 = \text{Div}, Y^4 = n\text{DREP}, Y^5 = n\text{EREP} \quad (3)$$

Model (3) represents five standalone regressions. The five dependent variables Y^k are five usages of the cash flow: investment, change of cash holding, dividend payout, net debt repayment, and net equity repurchase, which are defined in Table 1 Panel A in detail. X represents firm level controls such as market to book asset, sales growth, size, leverage, and tangibility, which are defined in Table 1 Panel B in detail. Dummy variable $\text{Private}_{i,t}$ is 1 if firm i is not publicly listed in year t and 0 otherwise. The key variable in this paper is the interaction of this dummy variable and the cash flow variable CF . We also add firm fixed effect f_i and year fixed effect y_t into each regression. Bounded by Equation (2), we expect the following results by theory:

$$\sum_{k=1}^5 \beta_3^k = 1 \quad \text{and} \quad \sum_{k=1}^5 \beta_p^k = 0 \quad \text{for } \forall p \neq 3 \quad (4)$$

The five coefficients for CF add up to 1, so each of them will be measuring how much of cash flow goes to each usage for public firms given other control variables. These coefficients are also called cash flow sensitivities. Other control variables also influence the within firm cash flow allocation more or less, but their influences take a zero-sum fashion, because they are not the source for allocation. *Ceteris paribus*, public firms' average cash flow sensitivities will be β_3^k , while private firms' average cash flow sensitivities will be $\beta_2^k + \beta_3^k$. The latter will measure how much proportion of cash flow goes to each purpose for private firms, and we have $\sum_{k=1}^5 (\beta_2^k + \beta_3^k) = 1$.

This is private firms' cash flow allocation, and our target in this paper. To have a better understanding of their allocation style through comparison, we can focus on the difference in cash flow sensitivities between private and public firms, which is the coefficient on the interaction term, β_2^k .

Table 3 Panel A reports the regression results obtained by implementing Model (3) on the full sample. We can see that Condition (4) holds perfectly except some rounding errors. For public firms, we show that a one-dollar increase in cash flow increases investment by 29 cents, increases cash holdings by 29 cents, increases dividends by 1 cent, repays debt by 22 cents, and repurchases equity by 20 cents. By Contrast, private firms responds to a one-dollar increase in cash flow by 20 cents increase in investment, 15 cents increase in cash holding, 3 cents increase in dividends, 38 cents increase in debt retirement, and 24 cents increase in equity repurchase. Or rather, if we make public firms' cash flow allocation style as the benchmark, private firms allocate 1/3 less than the benchmark to investment, 1/2 less to cash savings, 2 times more to dividend payouts, 4/5 more to debt reduction, and 1/4 more to equity reduction. On the aggregate, public firms spend almost 60% of cash flow on normal

uses, private firms only use 35% of cash flow on normal uses, and the largest portion 39% goes to repay debt, and another 24% to repurchase equity! Untabulated results show that our findings do not change if we regress the five uses on cash flow and control variables for private firms and public firms respectively and then compare the cash flow coefficients.

Table 3 Panel B presents the results of implementing Model (3) on the 2 digit SIC industry and total assets matched sample. This sample contains more homogeneous private and public firms especially regarding firm size, and the detailed matching method is described in Section 2.2. Regression results show that private firms devote 20 cents to investments, 14 cents to cash saving, 3 cents to dividends, 45 cents to debt repayment, and 19 cents to equity repurchase. This reconfirms our previous findings, and compared to the full sample results, it even more highlights the importance of paying off debts for private firms.

3.2. Cash flow shock: Cyclical fluctuations

Although we have used *Market to book* and *Sales growth* to control for industry and firm level growth opportunities in the regressions, it is undeniable that cash flows may incorporate information about future growth prospects. This is based upon the idea that the current cash flows are often positively associated with future cash flows, which presumes that a trend part of cash flow is sustained by growth opportunities. We would like to know how private firms would respond to an exogenous cash flow shock. Similar to Chang et al. (2014), we employ the Butterworth time series filter to decompose cash flow into a trend (permanent or sustainable) component, *CF_Trend*, and a cycle (transitory or contingent) component, *CF_Cycle*.⁶ *CF_Trend* contains

⁶ Pollock (2000) and Gomez (1999) argue that the additional flexibility produced by the additional parameter makes the high-pass Butterworth filter a better filter than the Hodrick–Prescott filter for

information about future growth prospects, while CF_Cycle represents contingent windfalls or shortfalls of cash flow. Untabulated results show that the mean of CF_Trend is very close to CF and the mean of CF_Cycle is very close to zero. We run the following regressions:

$$Y_{i,t}^k = \beta_1^k Private_{i,t} + \beta_2^k Private_{i,t} \times CF_Trend_{i,t} + \beta_3^k CF_Trend_{i,t} + \beta_4^k Private_{i,t} \times CF_Cycle_{i,t} + \beta_5^k CF_Cycle_{i,t} + \beta_X^k X_{i,t-1} + f_i^k + y_t^k + \varepsilon_{i,t}^k$$

where $k = 1, 2, 3, 4, 5$ and

$$Y^1 = Inv, Y^2 = \Delta Cash, Y^3 = Div, Y^4 = nDREP, Y^5 = nEREP \quad (5)$$

In the same vein, we expect that the coefficients for both trend part and cycle part of cash flow will add up to unity, since both parts are to be allocated across five uses, and the coefficients for other variables will add up to 0:

$$\sum_{k=1}^5 \beta_3^k = 1, \quad \sum_{k=1}^5 \beta_5^k = 1, \quad \text{and} \quad \sum_{k=1}^5 \beta_p^k = 0 \quad \text{for } \forall p \neq 3 \text{ and } p \neq 5 \quad (6)$$

Table 4 reports the results of Model (5) for the industry-size matched sample. We find that the interaction terms of private firm dummy and the two cash flow components display the same pattern as the interaction term of private firm dummy and overall cash flow. What we are interested in here is the interaction of private firm dummy and cycle cash flow. It shows that compared with public firms, private firms would allocate about 1/4 less cash flow shock to investment, 3/5 less to cash holding, 1/2 more to debt retirement, and more than double to equity repurchase. Thus, the most extraordinary feature of private firms' cash flow allocation is that they hoard less cash and pay more attention to reduce external finance given the chance.

3.3. Role of leverage in cash flow allocation

estimating the cyclical components. We also try Hodrick–Prescott filter and Christiano–Fitzgerald filter, and get basically the same results.

One of the most significant features in private firms' cash flow allocation is that they allocate less cash flow to cash holding and more cash flow to debt reduction. One of the possible reasons is that private firms may have higher cash holdings at the beginning of the year, so they do not need corner the cash flows during the year to cash. So we extract cash holding data for sample firms from Capital IQ. Untabulated statistics show that the ratio of cash holding to total asset is 6.6% for private firms and 11.5% for public firms. It seems that private firms are short in cash and they have every reason to hoard more cash.

The other possible reason is that private firms' beginning-of-year leverage ratio is too high, so they need to spare more money to repay debt. This is echoed by Table 2 summary statistics. Since all usages of cash flow are interrelated, less cash flow can be possibly allocated to other uses under the dominating demands for repaying debts. Note that we have already controlled for leverage in our regressions, as in Model (3). To better account for the different leverage distributions, we match private firms with public firms which exist in the same period, come from the same 2 digit SIC industry, and have closest average total assets and average leverage during this period. Same as before, we also require that the ratio of private and public firms' total assets should be between 0.5 and 2, and the ratio of private and public firms' leverage should be between 0.5 and 2.

This match has greatly reduced the difference of private firms and public firms. Untabulated statistics show that private (public) firms' size measure has a mean of 6.94 (6.93), and the difference is insignificant. Private (public) firms' leverage ratio has a mean of 0.47 (0.41). This difference is much smaller than the full sample, but it is still significant. To achieve the proximity of leverage ratio by doing this match, we have lost about 60% of data compared to a mere industry-size match. Table 5 reports

the regression results of this triple matched sample. Our previous findings are not materially changed. Untabulated results show that matching on industry and leverage also yields similar results. This might emanate from the inability to match private firms and public firms with indistinguishable leverage, or it might allude that merely controlling for the amount of debt is not sufficient, because private firms and public firms may experience different costs of external finance.

3.4. Robustness checks

Since private firms are inherently different from public firms, we try multiple matching schemes to mitigate the sample heterogeneity and report the regression results in Table 6. Panel A applies propensity matching method to account for the fact that firms with different characteristics may have different propensity for the listing status. First, we predict each firm's likelihood of being privately held by a logit model regressing the private firm dummy on firm cash flow, size, leverage, sales growth, tangibility, market to book ratio and a series of 2 digit SIC industry dummies. This predicted likelihood is also called propensity score, which is scattered between 0 and 1. Then for each private firm, we find a matching public firm in the same industry with the closest propensity score. We restrict the propensity score difference between the treated private firm and the matching public firm to be less than 0.05. So in this matched sample, the treated firm and matching firm choose their listing status for factors that are less likely to affect cash flow allocation, or we can say that the listing status is supposed to be exogenous in Panel A.

Panel B replicates the matching method used in Section 2.2 except that we change 2 digit SIC industry into Fama and French (1997) 48 industry classification. Panel C follows a slightly different matching approach which is adopted by Gao et al. (2013)

and Asker et al. (2015). In this sample, we match each private firm with a public firm in the same 2 digit SIC industry and closest in size (total asset) from the beginning year that the private firm appears in our full sample, and only switch to a new matching public firm when the previous match drops out of our full sample.

Around 1/6 of firms in our sample have experienced two statuses: listed and not listed. Most of them have changed their listing status only once, either from private to public or vice versa. Some of them have changed listing status several times. In the spirit of Asker et al (2015), Panel D picks out only the firms with variation in their listing status, so each private firm-year observation can match itself to the years when it is publicly listed. We can examine how listing status change can affect the cash flow allocation of the same firms whose many features are unchanged.

In all specifications of Table 6, our results are quite robust: Private firms spend less cash flow to investment activities and cash hoarding, but use more cash flow to pay out dividend and reduce external finance, especially debt.

We also conduct some other robustness checks in unreported tables. For example, we find that prior year's cash flow has very limited influence on this year's cash flow allocation. Besides, Gatchev, Pulvino, and Tarhan (2010) argue that it is indispensable to take into account the intertemporal dependencies within and across corporate decision variables. Thus we include lagged dependent variables (Inv_{t-1} , $\Delta Cash_{t-1}$, Div_{t-1} , $nDREP_{t-1}$, and $nEREP_{t-1}$) in all the equations to account for the interdependent nature of corporate policies, and find that our baseline results still hold.

4. Discussion of the mechanisms

The whole section 3 has convinced us that whatever the model specification is, private firms have an almost unwavering cash flow allocation pattern compared to

public firms: they spend less cash flow to investment activities and cash hoarding, but use more cash flow to pay out dividend and reduce external finance. However, why do private firms act like this? In this session, we draw ideas and findings from literature and implement some basic empirical tests in the hope of solving this mystery.

4.1. Financing friction hypothesis

There are many papers showing private firms' difficulty to get financing and their higher cost of external financing. By nature, private firms tend to avoid external capital market and usually have higher leverage, either due to high cost of issuing new equity or fear of ownership dilution. However, the external debt financing is also more costly for private firms. For example, Kovner and Wei (2012) document that public bonds of private companies have spreads ranging from 31 to 56 basis points higher than those of public firms, despite of no difference in default risk. Using a sample of large European firms, Mortal and Reisel (2014) find that compared to their public counterparts, private firms are more likely to use cash to pay off debt and are less likely to accumulate cash. They argue that the lower cash holding of private firms is related to the higher cost of debt. Acharya et al. (2007) also support this idea by arguing that cash is not negative debt theoretically. Their empirical tests using public firms show that even unconstrained firms display a systematic preference for using excess cash flows to reduce debt, considering the much lower yield of holding cash relative to the high borrowing cost. And they recommend retiring current debt as a more effective way than holding cash for financially constrained firms to boost investments in future high cash flow states. Bolton et al. (2014) also argue that interest payments drain the firm's valuable precautionary liquidity reserves and

sometimes even exhaust the firm's risk-free debt capacity, thus it is justifiable to use cash to reduce outstanding debts.

The above mentioned literature seems to explain why private firms allocate less cash flow to cash saving and more to debt reduction. Table 2 in this paper also suggests that private firms rely heavily on debt finance. Intuitively, financing frictions are very likely to be the most important driving force behind private firms' cash flow allocation. But we lack the data to compare private firms and public firms' financing frictions directly. A workaround is to look at how financing frictions affect public firms' cash flow allocation. In this section, we create dummy variables indicating whether a public firm is relatively more financially constrained. If more constrained firms allocate cash flow in a way similar to private firms, it would lend support to our financing friction hypothesis. Specifically, I use three measures that are widely accepted financial constraint indexes: WW Index based on the work of Whited and Wu (2005), HP Index based on Hadlock and Pierce (2010), and Cleary Index based on Cleary (1999). In every year, a public firm is classified as more (less) constrained if the index is in the top (bottom) three deciles. Then a financing friction dummy is created, which takes one if the public firm falls into the "more constrained" category and zero if the public firm falls into the "less constrained" category.

Table 7 shows the regressions of five fund uses on financing friction dummy, cash flow, and their interaction term, together with the set of standard control variables in previous analysis. Only the interaction term and cash flow's coefficients are reported in Table 7 for brevity. In investment (*Inv*) regressions, we find that more constrained public firms allocate less cash flow to investment than less constrained public firms. This is consistent with the finding that private firms allocate less cash flow to investment and private firms are more constrained than public firms. However, there

is some discrepancy between Table 7 and classic literature. The majority of prior studies find that more constrained firms have higher investment-liquidity sensitivity, because firms have to spend more of cash flow on investment activities when cash flow shrinks owing to limited scope of investment adjustments (see Fazzazi et al., 1988; Hoshi et al., 1991; Gilchrist, 1991; Whited, 1992; Lamont, 1993; Schaller, 1993; Gross, 1994; Bond and Meghir, 1994; Gilchrist and Himmelberg, 1994; Hubbard et al., 1995; Gilchrist and Zakrajsek, 1995; among others). Only Kaplan and Zingales (1997) and Cleary (1999) corroborate that firms with less financial constraints are more sensitive to cash flow. But the former paper is criticized as unreliable in that their conclusions rest on a small sample, that their *a priori* classification of financial constraint is problematic, and that their highly constrained subsample contains too many negative cash flow firms which could bias cash flow sensitivities downward. Our Table 7 results differ from most prior literature probably due to the way we define variables. As listed in Table 1, our cash flow variables are directly drawn from cash flow statement instead of income statement, which is very different from traditional cash flow sensitivity literature.

In change-in-cash regressions, we do not find consistent results regarding how financial constraints affect cash-cash flow sensitivities. When we use WW index and HP index to measure financial constraints, the results are consistent with Almeida et al. (2004), who find that more constrained firms have higher cash-cash flow sensitivity. But when we measure financial constraints by Cleary index, we find more constrained public firms have lower cash-cash flow sensitivity, which would be consistent with the hypothesis that private firms are more financially constrained.

In dividend regressions, we find that more constrained public firms have lower dividend-cash flow sensitivities. Actually, more constrained public firms do not allot

any cash flow as dividend payout, which is consistent with Fazzazi et al. (1988) and many papers ensuing. Most studies regard no dividend payout as a symbol for financial constraints of public firms. If more constrained private firms should also pay no dividends to shareholders, then financing frictions do not seem like the driving force of private firms' cash flow allocation.

In debt repayment regressions, we find that more constrained public companies allocate significantly less cash flow to retire debt when we measure financial constraints with HP index, but the interaction term is insignificant when we measure financial constraints with WW index or Cleary index. Thus financial constraint hypothesis cannot explain why private firms allocate more cash flow to repay debt. In equity repurchase regressions, we find that more constrained public firms assign more cash flow to repurchase shares than less constrained public firms. Thus the evidence appears parallel to previous finding that private firms allocate more cash flow to repurchase equity. To the best of knowledge, there is no comparable research on cash flow sensitivities of net reduction of external finance. A related study is Almeida and Campello (2010) who find that unconstrained public firms have lower demands for external finance due to ample internal funds, and constrained public firms' external finance is insensitive to cash flow. But their interested variable is external financing consisting of net debt issuance and net equity issuance, which is conceptually opposite to our net reduction of external finance. And our use of cash flow statement variables may lead to discrepancies between my Table 7 and previous literature.

Overall, it seems difficult to reconcile all the above evidence and literature. One possibility is that it is a wrong strategy to generalize the conclusions drawn from public firms to the comparison results between private firms and public firms. Another possibility is that financing friction hypothesis cannot explain why private firms

allocate cash flow to each use in the way shown in our baseline results.

4.2. Agency problem hypothesis

In this section, the agency problems refers to the interest misalignment between managers and shareholders. As discussed in the introduction, private firms' lower agency cost may shape their cash flow allocation. It is worth noting that this hypothesis is not mutually exclusive with financing friction hypothesis. Some existing papers explain their findings with both theories. For example, Mortal and Reisel (2014) argue that the degree of agency problems can explain why public firms hold more cash, and cost of debt financing can explain why private firms hold less cash. Harford and Haushalter (1998) find that when oil producers are left with an unexpected one time positive cash flow shock, more agency problem is associated with expansionary corporate activities, while ex ante constrained firms use the excess cash flow to reduce debt.

Therefore, similar to the logic in Section 4.1, we attempt to validate whether this channel works within public firms. To achieve this purpose, we pick out public firms with more agency problems and less agency problems. Board size is considered as a discriminating variable and a proxy for ineffective internal governance, because an overstaffing board is inefficient due to incremental cost of poor communication and decision making (Yermack, 1996; John and Senbet, 1998). G index proposed by Gompers et al. (2003) is taken as a proxy for outside governance because higher G index keeps takeover threat at bay, encroaches on shareholder rights and is value destructive. Accordingly, we classify a public firm as having more (fewer) agency problems if its board size or G index is in the top (bottom) three deciles of the year. Then dummy variables are created which take one if the public firm falls into "more

agency problems” category and zero if it falls into “fewer agency problems” category.

Table 8 shows that public firms with fewer agency problems allocate more cash flow to investments, cash savings, and less to dividends and debt reduction, compared with their high agency cost counterparts. Hence, public firms with fewer agency problems behave exactly opposite to private firms. This suggests that either private firms have bad governance and more agency problems, which seem implausible, or these agency issues cannot explain the difference between private firms and public firms, or the agency problems faced by public firms and private firms are essentially different and not comparable.

4.3. Shareholders' needs and power

It is surprising that neither financing frictions nor agency problems can perfectly explain private firms' cash flow allocation. But the higher financial constraints and fewer agency problems of private firms all stem from the special financing structure of private firms. There are a limited number of shareholders in private firms and their shares are more likely to be concentrated among a handful of investors. Blockholders of private firms are so powerful that they can make sure that managers will tilt to their advantage. Investors of public firms only require compensation for systemic risk because they can diversify their holdings and sell off any unwanted stock easily on the secondary market, but investors of private firms must demand compensation for idiosyncratic risk. There are very few opportunities that private firms' shareholders can sell out shares, so they would push the management to realize their financial gains by dividends and equity repurchases.

As we have seen in Table 3, public firms allocate one cent out of one dollar increase in cash flow to dividends, private firms allocate two cents more, and the

difference is significant. This is potentially consistent with the view that private firms' shareholders who cannot get liquidity in cash form need more dividends to meet daily consumption and diversification purposes. Zingales (1995) states that consumption and diversification concerns are motives to go public. If a private firm does not want to go public, it has to find some ways to satisfy shareholders. Firms with more powerful shareholders can pressure the management to disgorge cash as dividends, which will be otherwise diverted for managerial perquisites or unprofitable projects. In a similar vein, private firms also allocate more cash flow to repurchase equity. Private firms generally have more concentrated ownership and more powerful shareholders, who are able to get the amount of cash rewards which maximize shareholders' utility.

Concentrated ownership and powerful shareholders also have other influences. As argued by Jensen and Meckling (1976), when managers and major shareholders have aligned interests and the proportion of outside equity is small, the agency cost associated with debts increases, or rather, conflicts between the firm and creditors are more likely to occur, which would lead to higher cost of debt finance. Thus, a more sensible way to interpret evidence in Section 4.1 is that financing frictions do exist for private firms and are generally more severe than public firms, but the results in Table 7 cannot be generalized to private firms. Because financially constrained public firms and private firms employ different strategies to maintain financial slack: the former usually prefers to hoard more cash as a precautionary measure, the latter prefers to pay off existing debts as soon as possible even at the expense of reduced cash holding and investment activities.

Therefore, Section 4.2 only implies that the agency problems which matter to private firms more are between shareholders and debtholders, and the analysis of how

agency problems between managers and shareholders affect public firms' cash flow allocation cannot be extended to the comparison between private firms and public firms. Moreover, the agency problem hypothesis in Section 4.2 might not be entirely implausible, since private firms' conservative allocation of cash flow into investment and cash holding might be explained by value maximizing behaviors. But the optimal amount of investment and cash holding for each private company would be a complicated issue and demands much work ahead.

5. Conclusion

In this paper, we use a comprehensive sample drawn from Capital IQ, and put private firms and public firms in a unified model, to examine how differently they allocate cash flow. The new evidence shows that private firms allocate less cash flow to investments and cash saving, and more to dividends and reduction of external finance, especially reducing debts, compared with public firms. Since my sample is composed of large private firms and public firms, one caveat is that the results might not be generalized to small private firms.

To examine how private firms respond to exogenous cash flow shock, we disentangle the trend part and transitory part of cash flow by Butterworth filter. We find that for the transitory part of cash flow, private firms display a similar allocation pattern as the overall cash flow.

We also explore the possible mechanisms for private firms' cash flow allocation style by looking for analogies among public firms. We find that the allocation difference between private firms and public firms cannot be replicated by the comparison between more constrained public firms and less constrained public firms, or the comparison between better governed public firms and poorly governed public

firms. This raises doubts about applying conclusions from public firms mechanically to private firms. Theories inform us the most plausible driving force behind private firms' cash flow allocation should be concentrated ownership and related issues. Future studies on private firms should pay more attention to shareholders' desire for liquidity and agency costs associated with debts.

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Table 1: Definitions of main variables

For both private and public firms, we define variables in this table using data items under the tabs of three basic financial statements in Capital IQ database. Variables in Panel A include investment (*Inv*), change in cash holdings (*ΔCash*), dividends (*Div*), net debt repayment (*nDREP*), and net equity repurchase (*nEREP*). They are defined in arithmetic expressions with Capital IQ cash flow statement items, and the cash flow identity shown in Equation (2) automatically hold. Each financial item in Capital IQ is coded with a number, we also include those numbers in brackets. The sign of items under cash flow statement tab is aligned with positive net change in cash. For example, investing drains cash, so Item 2005 Cash from Investing is mostly a negative number in the database, and that is why we put a minus sign before it to get the variable we want. Although not shown in the arithmetic expressions, these variables are further deflated by lagged total assets. Panel B include major control variables: *Market to book*, *Sales growth*, *Size*, *Leverage* and *Tangibility*. *Market to book* ratios for private firms are filled by median *Market to book* of public firms in the same 3 digit SIC industry. All control variables are defined with Capital IQ balance sheet items and income statement items, and we do not include item numbers for brevity.

Panel A: Cash flow statement variables

Variable	Definition
<i>Inv</i>	- Cash from Investing [2005]
<i>ΔCash</i>	Net Change in Cash [2093]
<i>Div</i>	- Total Dividends Paid [2022]
<i>nDREP</i>	- (Total Debt Issued [2161] + Total Debt Repaid [2166])
<i>nEREP</i>	- (Issuance of Common Stock [2169] + Repurchase of Common Stock [2164] + Issuance of Preferred Stock [2181] + Repurchase of Preferred Stock [2172])
<i>CF</i>	Cash from Operations [2006] + Foreign Exchange Rate Adjustments [2144] + Miscellaneous Cash Flow Adjustments [2149] + Other Financing Activities [2175]

Panel B: Control variables

Variable	Definition
<i>Market to book</i>	3 digit SIC industry median of the market value of assets divided by the book value of total assets
<i>Sales growth</i>	The change in total revenues scaled by lagged total revenues
<i>Size</i>	The natural log of total assets
<i>Leverage</i>	Total debt over total assets
<i>Tangibility</i>	Net PPE over total assets

Table 2: Summary statistics

Our sample contains 9575 observations for private firms and 54782 observations for public firms from 1993 to 2012 obtained from Capital IQ database. Variables in this table have been defined in Table 1, except that *Inv*, Δ *Cash*, *div*, *nDREP*, and *nEREP* here have been deflated by lagged assets. All variables are winsorized at the top and bottom 1% of their distribution. We only keep observations which deviate from cash flow identity Equation (2) by less than 0.01. Dollar values are adjusted to the third quarter of 2009 value using the gross domestic product (GDP) deflator.

Panel A describes the mean, median, and standard deviation of each variable in our full sample. In this paper, we use a variety of methods to match private firms with public firms, and in Panel B we summarize one of the matched samples, which follows the most standard procedure of matching on 2 digit SIC industry and firm size. Using this procedure we find control firms for 9014 unique private firm observations with replacement. In each panel, *T*-tests based on unequal variance are conducted to test for differences in mean values between private firms and public firms; Wilcoxon rank-sum tests are conducted to test for differences in median values between private firms and public firms. The symbols ***, **, and * indicate that their means or medians are significantly different at the 1%, 5%, and 10% levels, respectively.

Panel A: Full sample						
	Private firms (N = 9575)			Public firms (N = 54782)		
	Mean	Median	Std.Dev.	Mean	Median	Std.Dev.
<i>Inv</i>	0.092	0.051	0.141	0.083***	0.057**	0.123
Δ <i>Cash</i>	0.006	0.000	0.062	0.006	0.001	0.085
<i>div</i>	0.011	0.000	0.033	0.007***	0.000***	0.013
<i>nDREP</i>	-0.034	0.000	0.129	-0.010***	0.000***	0.090
<i>nEREP</i>	-0.020	0.000	0.093	-0.019	-0.001***	0.102
<i>CF</i>	0.056	0.057	0.123	0.067***	0.079***	0.128
<i>Market to book</i>	1.450	1.351	0.430	1.568***	1.467***	0.457
<i>Sales growth</i>	0.242	0.050	0.850	0.188***	0.073***	0.600
<i>Size</i>	6.417	6.388	1.807	5.781***	5.637***	1.888
<i>Leverage</i>	0.493	0.466	0.349	0.225***	0.185***	0.218
<i>Tangibility</i>	0.344	0.283	0.254	0.264***	0.197***	0.222

Panel B: Industry-size matched sample						
	Private firms (N = 9014)			Matched public firms (N = 9014)		
	Mean	Median	Std.Dev.	Mean	Median	Std.Dev.
<i>Inv</i>	0.092	0.050	0.143	0.090	0.066***	0.116
Δ <i>Cash</i>	0.007	0.000	0.063	0.007	0.002**	0.068
<i>div</i>	0.011	0.000	0.033	0.008***	0.000***	0.013
<i>nDREP</i>	-0.034	0.000	0.131	-0.014***	0.000***	0.090
<i>nEREP</i>	-0.020	0.000	0.095	-0.009***	-0.000	0.081
<i>CF</i>	0.054	0.056	0.124	0.083***	0.088***	0.102
<i>Market to book</i>	1.461	1.361	0.434	1.467	1.371**	0.406
<i>Sales growth</i>	0.249	0.052	0.860	0.123***	0.063	0.404
<i>Size</i>	6.361	6.358	1.755	6.360	6.354	1.755
<i>Leverage</i>	0.493	0.465	0.353	0.265***	0.233***	0.228
<i>Tangibility</i>	0.331	0.270	0.247	0.323**	0.270	0.235

Table 3: Private firms' cash flow allocation

The data are from Capital IQ for 1993-2012. Panel A presents Model (3) implemented on the full sample which contains 9575 private firm-year observations and 54782 public firm-year observations. Panel B presents Model (3) implemented on the 9014 pairs of private and public firm-year observations matched by 2 digit SIC industry and size. The matching procedures are introduced in Section 2.2. $Private_t$ is a dummy variable which takes 1 if the firm is not listed in year t and 0 otherwise. Other variables are defined in Table 1. Regressions are all estimated with firm fixed effects and year fixed effects. Standard errors are heteroskedasticity-consistent and clustered at the firm level. T statistics are reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Panel A: Full sample					Panel B: Industry-size matched sample				
	Inv_t	$\Delta Cash_t$	Div_t	$nDREP_t$	$nERP_t$	Inv_t	$\Delta Cash_t$	Div_t	$nDREP_t$	$nERP_t$
$Private_t$	0.02*** (6.4)	0.01*** (4.1)	0.00*** (5.9)	-0.05*** (-15.9)	0.02*** (5.7)	0.02** (2.1)	0.01*** (2.6)	0.00*** (3.4)	-0.03*** (-4.7)	0.00 (0.4)
$Private_t \times CF_t$	-0.09*** (-4.7)	-0.14*** (-9.4)	0.02*** (4.7)	0.16*** (7.5)	0.05** (2.0)	-0.11*** (-3.4)	-0.14*** (-5.4)	0.02*** (3.7)	0.16*** (4.4)	0.07** (2.1)
CF_t	0.29*** (38.3)	0.29*** (44.0)	0.01*** (10.0)	0.22*** (33.8)	0.19*** (23.7)	0.31*** (12.4)	0.28*** (13.4)	0.01*** (3.7)	0.29*** (12.2)	0.12*** (6.0)
$Market\ to\ book_{t-1}$	0.02*** (10.1)	-0.00** (-2.4)	0.00** (2.4)	-0.01*** (-5.8)	-0.01*** (-5.5)	0.03*** (5.3)	0.00 (0.5)	0.00 (0.4)	-0.02*** (-5.0)	-0.01** (-2.3)
$Sales\ growth_{t-1}$	0.01*** (10.6)	0.00 (1.6)	-0.00** (-2.0)	-0.00*** (-4.0)	-0.01*** (-9.1)	0.01*** (2.9)	0.00 (0.5)	-0.00** (-2.0)	-0.00 (-0.9)	-0.01*** (-3.0)
$Size_{t-1}$	-0.03*** (-19.5)	-0.01*** (-17.3)	0.00 (0.5)	0.01*** (9.1)	0.03*** (27.0)	-0.05*** (-12.7)	-0.01*** (-4.1)	-0.00 (-0.3)	0.03*** (9.9)	0.02*** (7.9)
$Leverage_{t-1}$	-0.11*** (-22.0)	0.01*** (5.2)	-0.01*** (-11.4)	0.16*** (31.9)	-0.06*** (-15.9)	-0.12*** (-10.3)	0.00 (0.2)	-0.01*** (-5.9)	0.17*** (15.1)	-0.04*** (-6.7)
$Tangibility_{t-1}$	0.02** (2.0)	0.09*** (16.5)	0.00 (0.1)	-0.07*** (-10.4)	-0.03*** (-4.9)	-0.01 (-0.4)	0.07*** (7.2)	-0.00 (-0.3)	-0.07*** (-3.9)	0.01 (0.5)
# of Obs.	64357	64357	64357	64357	64357	18028	18028	18028	18028	18028
# of Firms	9058	9058	9058	9058	9058	4676	4676	4676	4676	4676
Within R ²	0.12	0.11	0.03	0.14	0.10	0.12	0.08	0.02	0.17	0.07
F test	148.2	107.1	19.2	121.4	67.5	28.1	15.1	4.9	31.5	9.1

Table 4: Private firms' allocation of sustainable and transitory cash flow

This table presents Model (5) implemented on the industry-size matched sample. The data are from Capital IQ for 1993-2012. $Private_t$ is a dummy variable which takes 1 if the firm is not listed in year t and 0 otherwise. We use Butterworth time series filter to decompose the cash flow variable into trend part and cycle part, CF_Trend_t and CF_Cycle_t , respectively. In this decomposition procedure, firms with only one year or two years of data are dropped. Other variables are defined in Table 1. Regressions are all estimated with firm fixed effects and year fixed effects. Standard errors are heteroskedasticity-consistent and clustered at the firm level. T statistics are reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Inv_t	$\Delta Cash_t$	Div_t	$nDREP_t$	$nEREP_t$
$Private_t$	0.02** (2.1)	0.01 (1.3)	0.00*** (2.8)	-0.03*** (-3.9)	0.00 (0.2)
$Private_t \times CF_Trend_t$	-0.15*** (-2.8)	-0.08*** (-2.6)	0.04*** (3.5)	0.14*** (3.1)	0.04 (0.9)
CF_Trend_t	0.38*** (10.5)	0.21*** (7.7)	0.02*** (3.3)	0.22*** (7.1)	0.17*** (5.6)
$Private_t \times CF_Cycle_t$	-0.06 (-1.2)	-0.21*** (-4.9)	0.00 (0.4)	0.18*** (3.2)	0.08* (1.9)
CF_Cycle_t	0.23*** (6.9)	0.36*** (11.2)	0.00 (0.5)	0.36*** (9.8)	0.05* (1.9)
$Market\ to\ book_{t-1}$	0.03*** (4.8)	0.00 (0.5)	0.00 (0.4)	-0.02*** (-4.5)	-0.01** (-2.6)
$Sales\ growth_{t-1}$	0.01*** (3.4)	0.00 (1.1)	-0.00** (-2.0)	-0.00 (-1.4)	-0.01*** (-3.4)
$Size_{t-1}$	-0.05*** (-12.1)	-0.01*** (-3.8)	-0.00 (-0.3)	0.03*** (9.5)	0.02*** (7.3)
$Leverage_{t-1}$	-0.11*** (-9.1)	-0.00 (-0.7)	-0.01*** (-5.4)	0.16*** (13.7)	-0.04*** (-5.6)
$Tangibility_{t-1}$	-0.02 (-0.9)	0.07*** (7.6)	-0.00 (-0.7)	-0.05*** (-2.9)	0.00 (0.2)
# of Obs.	13233	13233	13233	13233	13233
# of Firms	2190	2190	2190	2190	2190
Within R^2	0.12	0.09	0.03	0.17	0.07
F test	25.7	14.3	4.4	27.6	8.3

Table 5: Match private firms with public firms by industry, size and leverage

This table presents Model (3) implemented on the industry-size-leverage matched sample. The data are from Capital IQ for 1993-2012. $Private_t$ is a dummy variable which takes 1 if the firm is not listed in year t and 0 otherwise. Other variables are defined in Table 1. Regressions are all estimated with firm fixed effects and year fixed effects. Standard errors are heteroskedasticity-consistent and clustered at the firm level. T statistics are reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Inv_t	$\Delta Cash_t$	Div_t	$nDREP_t$	$nERP_t$
$Private_t$	0.02 (1.1)	-0.00 (-0.0)	0.01** (2.4)	-0.03** (-2.0)	0.01 (0.6)
$Private_t \times CF_t$	-0.10* (-1.7)	-0.08** (-2.4)	0.01* (1.7)	0.14** (2.2)	0.03 (0.6)
CF_t	0.37*** (8.4)	0.16*** (6.7)	0.01*** (3.0)	0.36*** (8.3)	0.11*** (3.5)
$Market\ to\ book_{t-1}$	0.04*** (4.4)	0.01 (1.6)	0.00 (0.9)	-0.03*** (-4.6)	-0.01** (-2.6)
$Sales\ growth_{t-1}$	0.00 (1.0)	-0.00** (-2.1)	-0.00 (-1.2)	0.00 (0.9)	-0.00 (-1.0)
$Size_{t-1}$	-0.07*** (-7.8)	-0.01*** (-3.2)	0.00 (0.0)	0.05*** (7.7)	0.02*** (4.6)
$Leverage_{t-1}$	-0.20*** (-8.4)	0.00 (0.4)	-0.01*** (-3.9)	0.26*** (11.9)	-0.05*** (-4.8)
$Tangibility_{t-1}$	0.03 (0.8)	0.06*** (4.1)	0.00 (0.2)	-0.09*** (-3.1)	-0.00 (-0.2)
# of Obs.	7476	7476	7476	7476	7476
# of Firms	1813	1813	1813	1813	1813
Within R^2	0.15	0.06	0.02	0.22	0.06
F test	13.0	6.6	2.6	19.1	4.8

Table 6: Alternative matching schemes

This table presents Model (3) results implemented on four matched samples employing alternative matching schemes. Our full sample contains 9575 observations for private firms and 54782 observations for public firms from 1993 to 2012 obtained from Capital IQ database. Panel A matches each private firm to a public firm by estimated propensity score of being unlisted. Panel B matches private firms and public firms by industry and size too, but the industry categories are based on Fama-French 48 industries instead of 2 digit SIC. Panel C also matches private firms and public firms by industry and size, but following Gao et al. (2013), the matched public firm for a specific private firm can change if the prior matched public firm drops out of sample at a point of time. Panel D keeps only those firms which have variations in listing status, so each private firm in this sample can use its own cash flow allocation style when it is publicly listed as a control. Detailed matching procedures are introduced in Section 3.4. In each panel, we use the same regression specification as in Table 3, but only the two key variables are tabulated for brevity. Regressions are all estimated with firm fixed effects and year fixed effects. Standard errors are heteroskedasticity-consistent and clustered at the firm level. T statistics are reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Inv_t	$\Delta Cash_t$	Div_t	$nDREP_t$	$nEREP_t$
Panel A: Propensity score matched sample					
$Private_t \times CF_t$	-0.06* (-1.8)	-0.08*** (-3.5)	0.02*** (3.5)	0.12*** (3.3)	0.00 (0.0)
CF_t	0.29*** (11.3)	0.21*** (11.2)	0.01*** (4.6)	0.30*** (11.2)	0.19*** (7.5)
Panel B: FF48 industry-size matched sample					
$Private_t \times CF_t$	-0.11*** (-3.2)	-0.15*** (-6.1)	0.02** (2.5)	0.14*** (4.2)	0.10*** (3.1)
CF_t	0.30*** (12.5)	0.29*** (14.2)	0.01*** (3.9)	0.29*** (12.8)	0.11*** (6.4)
Panel C: Industry-size matched sample following Gao et al. (2013)					
$Private_t \times CF_t$	-0.10*** (-3.0)	-0.13*** (-5.1)	0.02** (2.4)	0.17*** (4.8)	0.05 (1.4)
CF_t	0.31*** (11.7)	0.26*** (12.7)	0.01*** (5.2)	0.28*** (11.8)	0.14*** (5.8)
Panel D: Each firm matched by itself					
$Private_t \times CF_t$	-0.09*** (-3.1)	-0.11*** (-4.9)	0.01* (1.9)	0.13*** (4.6)	0.06* (1.7)
CF_t	0.27*** (14.9)	0.27*** (16.6)	0.01*** (4.8)	0.22*** (12.7)	0.24*** (10.9)

Table 7: Financial constraints and public firms' cash flow allocation

This table only contains public firms from 1993 to 2012 obtained from Capital IQ database, and it is aimed at comparing the cash flow allocation between more financially constrained public firms and the less constrained counterparts. Each year a firm is classified as more financially constrained if its Cleary Index, WW Index, or HP Index is in the top 3 deciles. A firm is classified as less financially constrained if its Cleary Index, WW Index, or HP Index is in the bottom 3 deciles. For each index, a dummy variable is created, which takes 1 if the firm-year is classified as more constrained and 0 if the firm-year is classified as less constrained. Cash flow allocation is estimated separately for more and less constrained subsamples. In each panel, we use the same regression specification as in Table 3, but only the two key variables are tabulated for brevity. Regressions are all estimated with firm fixed effects and year fixed effects. Standard errors are heteroskedasticity-consistent and clustered at the firm level. T statistics are reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	Inv_t	$\Delta Cash_t$	Div_t	$nDREP_t$	$nEREP_t$
Panel A: WW Index					
$High\ WW_t \times CF_t$	-0.18*** (-7.8)	0.08*** (4.6)	-0.02*** (-9.6)	0.00 (0.1)	0.11*** (5.3)
CF_t	0.38*** (19.9)	0.22*** (17.3)	0.02*** (10.1)	0.22*** (14.1)	0.16*** (10.1)
Panel B: HP Index					
$High\ HP_t \times CF_t$	-0.14*** (-6.8)	0.07*** (3.9)	-0.01*** (-6.0)	-0.07*** (-4.0)	0.16*** (9.4)
CF_t	0.37*** (20.7)	0.25*** (20.6)	0.01*** (7.9)	0.26*** (16.8)	0.10*** (9.9)
Panel C: Cleary Index					
$High\ Cleary_t \times CF_t$	-0.04** (-2.2)	-0.04*** (-2.5)	-0.00 (-0.7)	0.02 (1.1)	0.07*** (3.1)
CF_t	0.28*** (19.7)	0.30*** (25.6)	0.00*** (5.3)	0.22*** (19.2)	0.19*** (12.4)

Table 8: Agency Problem and public firms' cash flow allocation

This table only contains public firms from 1993 to 2012 obtained from Capital IQ database, and it is aimed at comparing the cash flow allocation between public firms with less agency problems and their counterparts. Each year a firm is classified as having fewer agency problems if its *Board Size* or *G Index* is in the bottom 3 deciles. A firm is classified as having more agency problems if its *Board Size* or *G Index* is in the top 3 deciles. For each of the two variables, a dummy variable is created, which takes 1 if the firm-year is classified as having fewer agency problems, and 0 if the firm-year is classified as having more agency problems. Cash flow allocation is estimated separately for each subsample. All control variables in Table 3 are included in the regressions. Only the coefficient of *CF* is reported for brevity. Regressions are all estimated with firm fixed effects and year fixed effects. Standard errors are heteroskedasticity-consistent and clustered at the firm level. T statistics are reported in parentheses. Coefficients significant at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

	<i>Inv_t</i>	$\Delta Cash_t$	<i>Div_t</i>	<i>nDREP_t</i>	<i>nEREP_t</i>
Panel A: Board Size					
<i>Small Board_t × CF_t</i>	0.02 (0.5)	0.15*** (4.6)	-0.02*** (-4.6)	-0.05 (-1.2)	-0.10*** (-3.4)
<i>CF_t</i>	0.40*** (10.3)	0.19*** (7.5)	0.02*** (5.1)	0.24*** (6.7)	0.15*** (6.6)
Panel B: G Index					
<i>Small G Index_t × CF_t</i>	0.07 (1.3)	0.05 (1.3)	-0.02*** (-2.8)	-0.10** (-2.2)	-0.00 (-0.1)
<i>CF_t</i>	0.35*** (7.4)	0.24*** (8.1)	0.02*** (4.0)	0.30*** (8.1)	0.09*** (2.8)

Essay Three

The Impact of 2012 STOCK Act on Congressional Insider Trading

Abstract

The U.S. Congress members are privy to valuable and confidential information that is beneficial for their financial investments. After many years' debates inside the congress, on April 4, 2012, the Stop Trading on Congressional Knowledge (STOCK) Act was finally signed into law by President Obama. This paper investigates the impact of the STOCK Act on congressional stock trading. Using hand-collected congressional financial disclosure data, we find that the frequency, dollar volume, timing ability, and profitability of congress trading have been reduced much after the STOCK Act, and especially so in purchase transactions which better reflect the activeness of potentially informed trading. A parallel study using CEO insider trading data does not exhibit such changes around the passage of this law, which points to the causality relationship flowing from the STOCK Act to changing congressional trading patterns.

JEL Classification: G18, G38, K22

Keywords: STOCK Act; Insider Trading; U.S. Senate; U.S. House of Representatives; U.S. Congress; CEO

"Members of Congress and their staffs are also privy to valuable confidential information not yet made public. They have information about the likely outcome of various votes, committee proceedings, and investigations. Such information can be extremely valuable to investors. Those who buy and sell stock on the basis of such non-public information will have an obvious advantage over those who lack such information. This is not the sort of information that even the most savvy and sophisticated investor would be able to obtain legally. From a moral perspective, such informational advantages are indistinguishable from those enjoyed in more familiar forms of insider trading."

----- Stuart P. Green, JD, Professor of Law and Justice, Nathan L. Jacobs Scholar at Rutgers School of Law, wrote on May 13, 2008

1. Introduction

As lawmakers, U.S. congress members exercise judgment concerning legislation across the entire spectrum of business and economic endeavors and possess information about various legislative proceedings. Besides, they can easily get in touch with other authority officials, lobbyists, business executives, and powerful people within their constituency. As the excerpt which I quote from Professor Green at the beginning of this paper states, congress members should be monitored and regulated against insider trading. There are a few widespread cases about how they actually use privileged information to gain financial profits.¹ For example, on January 3, 2009, Democrat Dianne Feinstein, a California Senator, introduced legislation to

¹ See "The Congress Insider Trading Scandal Is Outrageous: Rep. Spencer Bachus Should Resign In Disgrace" on <http://www.businessinsider.com/the-congress-insider-trading-scandal-is-a-disgrace-rep-spencer-bachus-should-resign-immediately-2011-11#ixzz2cUiYOTKF> and "Pelosi and Boehner Deny Profiting from Insider Information" on <http://www.procon.org/headline.php?headlineID=005042> for more examples.

grant 25 billion dollars to the FDIC (Federal Deposit Insurance Corporation). Senator Feinstein first promised to offer the FDIC this money on October 30, 2008, days before the FDIC awarded a highly competitive and equally lucrative contract to CBRE (CB Richard Ellis Group). Senator Feinstein's husband, Richard Blum, was the chairman of the board of CBRE.² Around the same time, Blum's private investment firm purchased more than 10 million new shares in CBRE. Despite these substantial conflicts of interest, Senator Feinstein remains a Senator and her husband was never investigated for insider trading related to CBRE.

However, before April 4, 2012, the U.S. congress appeared to have very few restraints preventing them from trading stocks based on nonpublic information that they acquire on the job. The Senate Ethics Manual even expressly defends insider trading in its "Financial Disclosure" chapter (p. 124) by asserting that "Members should not 'be expected to fully strip themselves of worldly goods'— even a selective divestiture of potentially conflicting assets is not required". While the U.S. House of Representatives Ethics Manual states that its members should "never use any information coming to him confidentially in the performance of governmental duties as a means for making private profit," it is not a binding legal restraint. The laxer requirements on congressmen's trading behavior appear to be unjust given the much stricter restrictions on other groups of interested people.³

On April 4, 2012, the Stop Trading on Congressional Knowledge (STOCK) Act

² See <http://www.washingtontimes.com/news/2009/apr/21/senate-husbands-firm-cashes-in-on-crisis/?page=all>

³ Insider trading for corporate executives and key employees has been banned since Securities Exchange Act (SEA) of 1934. The Insider Trading Sanctions Act of 1984 and the Insider Trading and Securities Fraud Enforcement Act of 1988 even increased the penalties for illegal insider trading to three times the profit gained or the loss avoided. SEC also regulates corporate insider trading through regulation FD ("Fair Disclosure") and Williams Act. Administrative agencies, such as FDA (Food and Drug Administration) and EPA (Environmental Protection Agency), which have access to information about the likely outcome of regulatory proceedings, are all prohibited by law from using such confidential information in the purchase and sale of publicly traded stocks.

was signed into law by President Obama. This act affirms and makes explicit the fact that there is no exemption from the “insider trading” laws and regulations for members of congress, congressional employees, or any federal officials. The law also expressly affirms that all federal officials have a “duty” of trust and confidentiality with respect to nonpublic, material information which they may receive in the course of their official duties, and a duty not to use such information to make a private profit. Will this act have some binding or deterring effects or just a mere scrap of paper?

STOCK Act requires expedited, periodic public disclosure of covered “financial transactions” by all officials in the executive and legislative branches of the federal government who are covered by the public reporting provisions of the Ethics in Government Act of 1978, as amended. Apart from the original requirement of annual financial disclosure (which is due by May 15 of each year) since 1978, it also now requires public reporting within 30 days of receipt of a notice of a covered financial transaction (but in no event more than 45 days after such transaction).

The express statements in STOCK Act that members of congress are not exempt from the insider trading prohibitions arising under the Securities Exchange Act of 1934 are supposed to eliminate the informational advantage for congressional trading. However, a couple of complications may undermine the impact of STOCK Act. First, the type of information that members of Congress receive is markedly different from corporate insiders, and virtually limitless in its breadth, rather than just related to one company. In other words, it is difficult to decide the occasions where a congress member can be called an insider. So congressmen may get away from an accusation due to lack of proof in the connectedness between their job and trading activities.

Another reason that STOCK Act may not exert immediate and notable effect is that it places an additional burden on the SEC to formulate rules with respect to

Congressional trading. This added responsibility came at a time when the SEC is still trying to comply with the demands of the Dodd Frank Act and Jumpstart Our Business Startups Act. Furthermore, SEC might be simply unwilling to enforce federal securities laws on members of congress, who are such a small subset of traders and at the same time who control their budget⁴.

Therefore, it remains an empirical question whether the STOCK Act prevents congress members from seeking financial profits by taking advantage of their access to significant and confidential information at their workplace, and the following is what we do in this paper to address this question. First, we hand-collect the common stock transaction records of congress members from their online financial disclosure reports between April 4, 2011 and April 3, 2013, and pool all 14653 congressional transactions together by quarters, and find that the frequency and dollar volume of congressional purchases and sales both keep dropping before the STOCK Act became law. After the passage of STOCK Act, the frequency and volume of congressional purchases remain flat at a very low level, congressional sales somewhat pick up, which might be in relation to market condition, their liquidity needs, or fire sales of existing equity holdings. In other words, the slight reversal of congressional sales may not indicate that congress members are taking advantage of their information in selling. Stock purchases should be a better indicator of informed trading activeness than sales.

Second, we gather all transactions made by the same congress member during

⁴ For example, in June 2011, House Appropriations Committee cut the SEC's fiscal 2012 budget request by \$222.5 million, limiting the SEC's budget to the same amount as 2011 even though the SEC's responsibilities were vastly expanded under the Dodd-Frank. The SEC is not financed by tax revenue, but rather by fees levied on those it regulates. And Dodd-Frank act mandates that the fees cannot exceed the SEC's budget. So cutting SEC's budget is equal to throwing those money back to Wall Street and other securities law breakers. http://www.nytimes.com/2011/07/16/business/budget-cuts-to-sec-reduce-its-effectiveness.html?pagewanted=all&_r=0

each of the two sample years, and examine whether an average congress member has changed his or her trading behavior around the passage of STOCK Act. The results show that an average congress member's trading frequency (volume) falls 40% (30%) after STOCK Act. Especially when it comes to purchases which is directly linked to congressional trading motivation, both frequency and volume declines by 50%.

Third, we pile up all congressional transactions on the same stock in the same year regardless of which congress member made the transaction, and examine how this firm-year level trading activeness is affected by the STOCK Act. Using an OLS regression framework controlling for variable and invariable firm characteristics which might influence congress members' investment choice, we find that after the passage of the STOCK Act, the overall trading frequency (volume) diminishes by 24% (18%), the purchase trading frequency (volume) diminishes by 31% (22%), and the sale trading frequency (volume) diminishes by 18% (2%). All these declines are statistically significant except the sale trading volume which might be caused by congressional bulk sales and the concurrent bull stock market.

Fourth, we use an event study methodology to measure congress members' ability of timing financial transactions. The events in our analysis are each purchase and sale by congress members. We measure 10-day buy-and-hold abnormal returns (BHARs) both before and after the event, and then calculate the difference as a proxy for timing ability. Other window lengths (20-day and 5-day BHARs before and after the event date) are also tried and yield similar results. Our findings are that congress members have positive (good) timing ability before the STOCK Act and negative (poor) timing ability after the STOCK Act for purchases, and the decline is statistically significant. Congressional sales' timing is neither good nor poor before the Act, but it turns to be very poor after the Act, and the change is significant with a similar magnitude of the

change for purchases. Regression results confirm the significant decline in congress members' timing ability after controlling for firm characteristics and invariant personal investment skills.

Fifth, I also follow Ziobrowski et al. (2004) to conduct the calendar time portfolio analysis. On each calendar day, I construct a portfolio containing stocks traded by congress members in last month, and calculate the trade value weighted average return of today. Then at the beginning of each month, I apply the Fama French three factor model on daily portfolio returns during the next three months. So I get a series of portfolio alphas for the three factor model, which represent the time series of mimicking portfolios' abnormal returns. The results show that before the STOCK Act, the portfolios mimicking congressional stock purchases generally have positive abnormal returns, but the abnormal returns turn negative after the STOCK Act.

Finally, we do a parallel study using CEO insider trading data which can serve as placebo tests, because CEOs' trading behaviors only reflect their knowledge of the market and their own company, but not affected by the STOCK Act. If CEOs' trading is also reduced after the STOCK Act, it can jeopardize the validity of our previous findings. On the contrast, if CEO trading is not reduced after the STOCK Act, then the deterrence effect of the new law is specific to congress members. We extract the CEO insider trading data from Thomson Reuters Insider Trading Database using the same set of firms traded by congress members, and find that CEO purchases remain stable throughout the sample years, and CEO sales on average increases after the STOCK Act. The person-level statistics show that for an average CEO, the trading frequency does not change around the Act, but the overall trading volume increases by 13%. Firm-year level regressions show that CEO trading frequency and volume increase by 9% and 26% respectively after the passage of STOCK Act. Event studies show that

even though CEOs have very good timing ability both before and after the STOCK Act, their timing ability does not change. All these evidence does not align with what we find in the congress member sample, indicating that the reduction of trading activeness and timing ability for congress members is the consequence of passing the STOCK Act.

My study contributes to the extant literature in two ways. First, my findings add to the literature on insider trading and regulation effect. Prior studies have shown the profitability of corporate insider trading (e.g., Jaffe, 1974; Jeng et al., 2003) and congressional insider trading (Ziobrowski et al., 2004, 2011), and some papers focus on the impact of regulation change on corporate insider trading (e.g., Jaffe, 1974; Allen, 1990; Ashardi, 1998; Bhattacharya and Daouk, 2002; Bushman et al., 2005), but the regulation effect on congressional insider trading has never been examined. Our paper bridges this gap by looking at the recent STOCK Act. Second, our paper also contributes to the burgeoning research area of political ownership in corporations. Some representative papers in this strand of literature include the following. Tahoun and van Lent (2010) find that the U.S. congress members' equity ownership in financial institutions is positively associated with the likelihood, timing and amount of bailout under the Troubled Asset Relief Program (TARP). Tahoun (2011) depicts the various aspects of the quid pro quo relations between the U.S. congress members and firms, for example, congress members invest more in firms which contribute to their election campaigns and favor their party, and these firms receive more government contracts. Our paper studies political ownership per se and its change around the STOCK Act.

This paper is organized as follows. The next section reviews related literature. Section 3 describes congressional trading data and our main sample. Section 4

elaborates on major empirical results. Section 5 delineates our placebo tests using CEO data. Section 6 concludes.

2. Literature review

By examining the impact of STOCK Act on congressional insider trading, we bring together two different strands of literature.

First, our paper builds on prior findings of congressional informational advantage in financial trading. Ziobrowski et al. (2004) are among the first to examine senators' insider trading in the US. They compute abnormal returns from common stock investments of U.S. senators from 1993 to 1998. When regressing calendar-time trade-weighted portfolio returns on the Fama-French three factor model, they find that stocks purchased by senators significantly beat the market by 85 basis points per month, and that stocks sold by senators underperform the market by 20 basis points, although the abnormal returns after sales are not statistically significant. Hence, combining the purchase transactions with sale transactions in a hedged portfolio, senators outperform the market by around 1% per month. Ziobrowski et al. (2011) examine 16,000 common stock transactions made by approximately 300 House delegates between 1985 and 2001, and find that a portfolio mimicking the purchases of representatives beats the market by 55 basis points per month.

If this is true, congressional stock trading can beat the U.S. households and corporate insiders. Barber and Odean (2000) show that the average households underperform the market by 12 basis points per month. Jeng et al. (2003) show that corporate insiders' stock purchase performs higher than the market average by 52-68 basis points per month, although no significant abnormal returns for their stock sales. An earlier study of Jaffe (1974) draws a random sample of trading months during

1962-1968 covering 200 large firms and finds that the average return of corporate officials' trading excluding transaction costs is around 40 basis points per month.

The above stark contrast indicates that congressional investments are highly profitable and there is widespread insider trading on the Capitol Hill. Our paper is the first one to investigate their investments' pattern changes around the STOCK Act. To study regulation change, we need to draw another existing strand of literature which examines how the formulation and amendments to 10(b)5-1 of Securities Exchange Act 1934 (SEA) affect corporate insider trading behaviors. It's broadly recognized that corporate insiders' trades lead to substantial abnormal returns, although some papers attribute the high insider profits to the fact that the prompt disclosure of insider transactions spreads company information before public release (Bhattacharya et al, 2000; Bushman et al, 2005; Meulbroek, 1992) and attracts other investors to follow the trend (Givoly and Palmon, 1985). Banerjee and Eckard (2001) present a pre-SEA sample, which is the legal insider trading associated with mergers during the first merger wave (1897-1903). By comparing this sample with modern samples, they find that insider trading regulations which ensued may have had little impact on the distribution of pre-announcement and announcement returns, implying that insiders still gain while outsiders do not benefit from regulations.

Of the few who research into regulation changes on corporate insider trading, Jaffe (1974) examines the changes in volume and profitability of insider trading after three important legal decisions (two of them are involved with SEC vs. Texas Gulf Sulphur case) during 1961-1966. There was no significant change in the properties of insider trading following any one of the three events. In addition, there are no combined effects from three decisions. Contrary to Jaffe's (1974) conclusion, Allen (1990) reviews the Texas Gulf Sulphur (TGS) litigation influence by taking account

of the subsequent appellate court decision, finding that corporate insiders decreased their willingness to exploit proprietary information after the appellate court adopted SEC's liberal material fact standard. Ashardi (1998) also concludes that insider-trading regulation has been reasonably effective in deterring illegal insider trading by registered and temporary insiders. With a unique and discriminatory perspective, Bhattacharya and Daouk (2002) and Bushman et al (2005) suggest that the enactment of insider trading laws does little to curtail insider trading; only the initial enforcement of such laws can effectively suppress insider trading in cross-country samples, especially in emerging economies.

Now that the newly adopted STOCK Act is meant to restore the public confidence in the US congress by limiting congress members' discretion in using private information procured at the workplace, this second strand of papers provides both motivation and methodology for us to examine whether the STOCK Act is successful in its nascent period.

3. Data

3.1. Congressional stock transactions

After April 15, 2013, all public financial disclosure statements required by STOCK Act in the legislative branch are made in electronic form, and posted on the Internet where the public can search, sort, and download them. All data in this paper are hand-collected from the official websites of Senate Office of Public Records and House of Representatives Financial Disclosure Reports Database.⁵

We have downloaded all the files that satisfy the following: For Senate data, the

⁵ Senate Office of Public Records website:
http://www.senate.gov/pagelayout/legislative/g_three_sections_with_teasers/lobbyingdisc.htm#lobbyingdisc=fd. House of Representatives Financial Disclosure Reports Database website:
http://clerk.house.gov/public_disc/financial-search.aspx.

receiving or posting date ranges from 1 Jan 2012 to 15 May 2013; for House data, the filing year is between 2011 and 2013. Valid information mainly comes from three kinds of forms: financial disclosure report (FDR, or annual report), termination report (TR), and periodic transaction report (PTR). So we have all congress members' FDRs for years 2011 and 2012, TRs whenever a congress member terminates his or her position in the congress, and PTRs during 2012 and 2013 which are required by the STOCK Act. In these files, we can get information for each common stock transaction, whether it is a purchase or a sale, specific transaction date, and the approximate transaction value. These records include common stock transactions made by congress members, as well as their spouses, and their dependent children. We don't look at assets held in blind trusts since congress members are not required to report them. We also exclude all option transactions, preferred stock, REITs, mutual funds and foreign stocks, although this may underestimate the intensity of insider trading given the scope of congressional information. To facilitate comparison, I restrict our final dataset to the period between April 4, 2011 and April 3, 2013, so that there are one year's data both before and after the effective date of STOCK Act, April 4, 2012.

As mentioned in Ziobrowski et al (2004), congressional trading data's quality has serious limitations. Especially these files prepared and signed by congress members are not audited for accuracy by any government or non-government agency. They are found to repeatedly report the same transactions in multiple PTRs, mis-spell company names, or report purchase and sale for the same stock on the same day with same transaction value categories. We can't exclude the possibility that they also might have, mostly unintentionally, forgotten to report some transactions or mis-reported the transaction date or value. Some of their reports are handwritten and not easy to decipher.

Even though several congress members in their FDRs have posted brokerage reports as appendixes about realized profits (losses) and unrealized profits (losses) which can tell us detailed trading volume and lead us to track each closed transaction and open position at the end of year, the majority of congress members only report year-end holdings and daily transactions, the amount of which is put in broad categories: \$1,001 to \$15,000, \$15,001 to \$50,000, \$50,001 to \$100,000, \$100,001 to \$250,000, \$250,000 to \$500,000, \$500,001 to \$1,000,000 and over \$1,000,000. I also include transactions below \$1,000 whenever the congress member indicates such transactions in the report. And then I define each transaction value as the midpoint of each band. For transactions over \$1,000,000, I uniformly define the transaction value to be \$1,000,000 exactly. Since trades on the same stock by the same person on the same day should reflect same information, so we combine these daily trades for each person, so that each person appears to trade each stock only once on the same day in our final sample. Then we exclude daily transactions valued less than \$100 on a stock.

Finally we are left with 14653 transactions by 144 congress members (40 senators and 104 representatives). Table 1 shows the transaction distributions. The majority of congressional stock transactions are valued under \$50,000. Transactions valued over \$500,000 are much less common. For each value category, there are generally a larger number of sale transactions than purchases, but the difference in counts is small. In the second column, we explicitly denote the midpoint values for each transaction category, and these values are to be used in later statistics and regressions.

To get an idea of whether the numbers of congressional transactions have changed around the passage of the STOCK Act, we count the number of transactions for each of the eight quarters from April 4, 2011 to April 3, 2013, and plot these numbers by dots in Figure 1. We also connect these dots to show its time trend. Figure 1 suggests

that before the passage of STOCK Act, the total number of congressional trading has been declining all the way. This is consistent with the fact that passing STOCK Act was a long story.⁶ The most recent proposal on STOCK Act was brought up by Tim Walz on March 17, 2011, and this proposal finally gained one co-sponsor and was referred to various committees. Hence, since the beginning of our sample, April 4, 2011, there must be some expectations on the passage of this act, and congress members have been reducing their trading frequency. Eight more co-sponsors joined the proposal by November 4, 2011. The most crucial event is that on November 13, 2011, *60 Minutes* reported that several members of Congress allegedly used insider information for personal gains. The STOCK Act received 84 additional House co-sponsors in the five days following the report and also got positive response from Senate. Untabulated statistics show that there are 5537 congressional transactions from April 4, 2011 to October 3, 2011, but only 3114 transactions from October 4, 2011 to April 3, 2012, which verifies that November 2011 as a milestone point has amplified the anticipation of STOCK Act passage and shrunk congressional trading.

Figure 1 also shows the total number of congressional trading picks up after April 4, 2012, especially in Quarter 2 and Quarter 3. By dissecting the total number into number of purchase transactions and number of sale transactions, Figure 1 shows that the higher trading frequency after STOCK Act passage is mainly driven by the sale side. Congress members are inclined to reduce equity holdings to avoid regulatory scrutiny. They are very likely, though not verified, to put the money into blind trusts which is not subject to disclosure according to the law. Figure 1 shows that the frequency of purchase transactions remains very low in the quarters after STOCK Act.

⁶ The earliest introduction of STOCK Act dates back to March 28, 2006, when Congressman Brian Baird and Congresswoman Louise Slaughter brought it up in the 109th session of the House of Representatives. However, it quickly died in committee. Baird and Slaughter re-introduced this legislation on May 16, 2007 in the 110th House session, and on January 26, 2009 in the 111th House session, but both bills failed to make it out of committee.

We also sum up the absolute value of transaction amount in each quarter and plot them into Figure 2. It displays a similar time trend as in Figure 1.

Besides the above transaction-level analyses, we also calculate person-level statistics and look at whether an average congress member has changed his or her trading behavior around the passage of STOCK Act. Table 2 Panel A shows that there are 111 congress members who have reported stock trading in the pre-STOCK Act period, from April 4, 2011 to April 3, 2012. These people on average make 78 stock transactions during this year, their annual transaction value are about 2.2 million dollars. There are 127 congress members who have reported trading in the post-STOCK Act period, from April 4, 2012 to April 3, 2013, so the number of people trading does not change much. However, on average each congress member trades 47 times in the year following the STOCK Act, which is a 40% drop from the year before. The total transaction volume for an average congress member also diminishes by 30% to 1.5 million dollars. If we separate purchase transactions from sale transactions, we see that the number and value of purchases both decreases by about 50%, while for sales, the number and value just decrease by less than 30% and less than 10% respectively. This is consistent with transaction-level statistics. The smaller drop for stock sales should be due to fire sales of uneasy congress members after the STOCK Act. To make sure the reduced transaction is driven by STOCK Act, rather than differing congressional make-up in these two years, among the 144 congress members in our sample, we only keep 94 members who stay in the congress during the two years and have trading records in both pre- and post-STOCK Act periods. These people tend to trade with higher frequency and higher value compared to Panel A, but the pre- and post-STOCK Act comparison results in Panel B resembles those in Panel A, which confirms that congress members do trade less especially purchase less after

the STOCK Act.

3.2. Firm-year level variables

In the multivariate analysis, we form a firm-year level sample by summing up all congressional transactions on each stock in each of the two sample years. So each firm only has one observation in the pre- or post-STOCK Act period, and not every stock is traded by congress members in both periods. We employ the following framework to test the impact of STOCK Act on congressional trading.

$$\text{Congress trading}_{i,t} = \alpha + \beta D(\text{STOCK Act})_{i,t} + \gamma X_{i,t} + \delta_i + \varepsilon_{i,t},$$

where *Congress trading*_{*i,t*} is congressional stock trading for stock *i* and year *t*, which can be either *Ln(Frequency)*, the natural log of total number of transactions, or *Ln(Value)*, the natural log of total volume of transactions. *D(STOCK Act)* is a dummy variable indicating the post-STOCK Act period, from April 4, 2012 to April 3, 2013. *X* represents a set of firm characteristics which may affect congress members' investment choice. These characteristics include firm size *Ln(assets)*, *ROA*, *R&D* expenses, *Annual return*, and *Return volatility*. The detailed definition of each variable is given in Table 3. Company financials are from Compustat and return related data are from CRSP. Firm fixed effects (δ_i) are also included in this OLS regression. Standard errors are heteroskedastic-consistent Huber/White/Sandwich estimates and clustered by firm. After excluding observations with missing variables, our final sample consists of 2290 observations for 1469 firms.

Table 3 Panel A describes this sample. The mean (median) frequency that a stock is traded by congress members is 6 (3), while the mean (median) volume that a stock is traded by congress members is \$182,203 (\$40,500). These two proxies for congressional trading are very skewed, so we use the natural log of these two variables. The statistics of

$\ln(\text{Frequency})$ and $\ln(\text{Value})$ show that the skewness problem has been well solved. The dummy variable $D(\text{STOCK Act})$ has a mean of 0.486, because 1178 firms are traded by congress members in the pre-STOCK Act year, and 1112 firms are traded by congress members in the post-STOCK Act year (48.6% out of 2290 firm-year observations). Among the firm characteristics, the most notable one is that firms traded by congress members tend to be larger than the Compustat universe average.

We also form firm-year level samples for congressional stock purchases and sales separately in a similar way. From Panel B and Panel C of Table 3, we can see that the frequency and volume of congressional purchases and sales do not differ materially. Other variables are also similar to the sample in Panel A and thus omitted.

4. STOCK Act and congressional stock trading

4.1. Multivariate analysis of congressional stock trading frequency and volume

Prior transaction-level and person-level analysis has shed some light on the deterrence effect of the STOCK Act on congressional stock trading. To make sure the reduction of transaction frequency and volume is not due to evolving attractiveness of the stock per se proxied by its characteristics, we employ the regression framework described in Section 3.2 and report the results in Table 4. The negative coefficients of $D(\text{STOCK Act})$ suggest that the deterrence effects remain after considering firm characteristics. For a typical stock traded by congress members, the trading frequency has fallen by 24% ($1 - \exp(-0.303)$), and the trading volume has dropped by 18% ($1 - \exp(-0.198)$).

Since in the transaction-level analysis (Figure 1 and Figure 2) and person-level analysis (Table 2), we both find that the pattern of congressional stock purchases and sales differ greatly. The overall level of congressional stock sales does not decrease

much after the STOCK Act, because stock selling can signal either trading activeness or inactiveness. When congress members do not want to continue stock trading, they may clear the current holdings first. In comparison, stock purchases should be a better vane for trading activeness. Thus we separately form firm-year level samples based on purchase transactions and sale transactions, and look at how the STOCK Act may affect them after controlling for firm characteristics.

Table 5 shows that the frequency (value) of congressional stock purchases drops by 31% (22%), which is the best proof that congress members are retreating from the stock market. The frequency of congressional stock sales drops by 18%, while the decrease in the value of sales is not statistically significant. This is largely consistent with the idea that congress members are conducting bulk sales. Another reason why the decrease in value is smaller than decrease in frequency for both purchases and sales is that the stock market displays an upward trend during our sample period. Figure 3 shows that the average level of S&P index is higher in the post-STOCK Act period, which should increase the valuation of stock transactions after the passage of STOCK Act. Untabulated results show that S&P 500 index and Dow Jones Industrial Average index both increase by around 3% from April 4, 2011 to April 3, 2012, and 11% from April 4, 2012, to April 3, 2013. Therefore, the reduction effect of STOCK Act on congressional trading value may be offset by this valuation effect to some extent.

4.2. Congress members versus all investors

Previously we find that congressional transactions, especially purchases, have decreased after the STOCK Act. A potential concern is that maybe other investors also reduced trading on these stocks as well. To investigate whether this is the case, I do a

stock level summary in Table 6 Panel A. We find that for periods both before and after the STOCK Act, congressional trading only accounts for a negligible part of total trading volume. For example, from 4 April, 2011 to 3 April, 2012, there are 1178 stocks ever traded by congress members. For these 1178 stocks, the average aggregate trading volume by all investors is about 30,000 million dollars, but the average volume of aggregate congressional trading is 0.2 million dollars. In Panel A, we also see that all investors' aggregate trading volume has dropped by 10% on average after the STOCK Act, but congress members' aggregate trading volume has dropped 15% on average. The contrast is especially paramount for the stocks purchased by congress members. For these stocks, all investors reduced their trading volume by only 4%, suggesting that an average investor on the stock market would be 4% less likely to purchase these stocks, but congress members reduced their purchasing volume by 31%. The situation is somewhat reversed for stocks sold by congress members. For these stocks, congress members increased selling by 13%, while all investors reduce trading on them by 10%. The abnormally large volume of congressional sales compared with an average investor may be due to their liquidity needs, market conditions, or their desire to dispose of existing holdings.

To show how differently congressional trading responds to the STOCK Act compared with other investors' trading after controlling for firm characteristics, I replace $\ln(\text{Value})$ in Table 4 and Table 5 with the natural log of all investors' aggregate transaction value, and run similar regressions. Table 6 Panel B confirms that for stocks traded by or purchased by congressional members, even though there is a drop of trading volume for both congressional and non-congressional investors, but the magnitude of decrease for congressional investors is much larger. For stocks sold by congress members, other investors also reduced trading, but congress member did

not reduce trading as much.

In Panel 3, I rerun the $\ln(\text{Value})$ regressions in Table 4 and Table 5, just adding *All investors' trading* as an additional control. I find that congressional trading is not 100% correlated with aggregate trading by all investors; the connection is rather weak with stocks congress members chose to buy. And after controlling for other investors' trading, congressional purchases still decreased significantly after the STOCK Act.

4.3. Congress members' timing ability

Besides the trading activeness measured by transaction frequency and volume, we also want to examine whether congress members' short-term timing ability changes around the passage of STOCK Act. Here timing ability is measured according to the “buy low, sell high” logic. If congress members can use their information to purchase a stock at a relatively low price, then the buy-and-hold abnormal return after the purchase should be greater than the buy-and-hold abnormal return before the purchase. Similarly, if they can sell a stock at a high price, the buy-and-hold abnormal return after the sale should be lower than before the sale. Thus, we use the difference in buy-and-hold abnormal return before and after the transaction date to proxy for timing ability. The procedure to calculate buy-and-hold abnormal returns (BHARs) and timing ability is detailed in Table 7.

Table 7 lists three measures of timing ability with different window lengths. The first measure has the longest window, which involves Trading Day -20 to Trading Day +20 relative to the transaction date. Before the STOCK Act, congressional stock transactions are only reported once a year, so the public can't know these transactions immediately after the transaction date. The new STOCK Act requires PTR disclosure of transactions no later than 45 calendar days. If a congress member files the PTR

within 20 trading days, there is a chance that the market might incorporate this information and follow the congress member's trading direction, which would amplify the timing ability of the congress member. But this disclosure effect should be very limited for our sample because PTRs are not regularly posted online before April 15, 2013. Thus, our reported timing ability measures would likely result from the events which motivated congress members' trading, rather than market reaction to their transaction records.

Table 7 Panel A shows that congress members have positive mean timing ability with their purchases before the STOCK Act, but this timing ability becomes significantly negative after the STOCK Act. The deterioration of timing ability measured by the "Difference" column is all significant at 1% level. This suggests that due to more frequent, more transparent mandatory financial disclosure, congress members are less willing to use informational advantage to pick stocks. And most of them have limited spare time, so the stocks they choose experience negative returns just like ordinary people documented by Barber and Odean (2000). Panel B shows that in the pre-STOCK Act period, congress members do not "sell high" and their average timing ability in sales is neither good nor bad. But in the post-STOCK Act period, they time their sales much worse, maybe due to the fire sales of current equity holdings regardless of the price. But the changes in timing ability of congressional sales are significant and comparable to that of congressional purchases in magnitude.

We also examine how the STOCK ACT influences the transaction-level timing ability after controlling for firm-year level characteristics. We only tabulate the OLS regression results when the dependent variable is $BHAR[1, 10] - BHAR[-10, -1]$ for the purchase sample and $BHAR[-10, -1] - BHAR[1, 10]$ for the sale sample in Table 8. Person fixed effects are included to control for the inherent financial trading ability of

each congress member. The coefficients on $D(STOCK\ Act)$ are both significantly negative for purchases and sales, with a magnitude slightly smaller than the “Difference” column in Table 7. The implication is that for a specific congress member, his timing ability in purchase and sale transactions has decreased after considering the effects of stock related features. Changing the dependent variable into other two versions of timing ability proxies does not materially alter our conclusion.

4.4. Calendar time portfolio analysis

I also follow Ziobrowski et al. (2004) to employ the calendar time portfolio approach to examine the profitability of congressional trading before and after the STOCK Act. Due to limited time span of my sample, I cannot completely replicate their methodology. I conduct my analysis by the following procedures.

For each calendar day (I call it as the benchmark day) from 4 May, 2011 to 3 January, 2013, a calendar-time portfolio is constructed including all those stocks that have been bought or sold since one month ago till the last day. Using the transaction value when the stock was bought or sold as the weight, I calculate the value weighted portfolio return for the current calendar day. After getting a series of daily portfolio returns, I run a daily Fama French three factor model regression for the next three months, and roll over this regression after a one month gap. Thus, for the purchase portfolio, sale portfolio and hedged portfolio, I run 21 rolling regressions and get a series of alphas. These alphas represent the abnormal return of a portfolio which mimics congress members’ transactions.

From Figure 3a, we can see that before April 2012, most alphas of congressional purchase portfolios are positive, except the portfolios formed from 4 November, 2011 to 3 February, 2012. These portfolios have negative abnormal return because during

this period, the anticipation of STOCK Act passage greatly intensified as argued in Section 3.1. After the actual passage of STOCK Act, negative abnormal returns of congressional purchase portfolios become more common. Hence, the profitability of congressional purchases is lower after the STOCK Act. Even though we do not see there is a decrease of congressional sales' profitability, which might be due to booming stock market, the profitability of the hedged portfolio generally becomes negative since September 2011. This shows the anticipation effect of STOCK Act. A prolonged sample period would better assess its influence.

5. Placebo tests using CEO insider trading data

Since we can't completely eliminate the possibility that the reduction of congressional trading after the STOCK Act might be related to the market condition or information inside the associated company, we do some placebo tests using CEO insider trading data, because CEOs know the market condition and their own firm better than anyone and their trading is not affected by the STOCK Act. If CEOs' trading is also reduced after the STOCK Act, it can jeopardize the validity of our findings in previous sections. On the contrast, if CEO trading is not reduced after the STOCK Act, then the deterrence effect of the new law is specific to congress members.

First, we extract all firm IDs which are ever traded by congress members during the sample years, and then we pull out all CEO transactions of these firms from Thomson Reuters Insider Trading Database. Like Figure 1, we also plot the quarterly counts of CEO insider trading in Figure 4. We find that CEO purchase frequency (the green line) remain low and steady, the shape of overall trading frequency (the blue line) is dominated by sales (the red line) which is quite irregular. But the average

trading frequency in the second sample year is higher than that in the first sample year. The pattern of flat purchases and rising sales becomes clearer if we look at the trading volume in Figure 5. It seems that around the passage of the STOCK Act, there are no salient factors which would likely reduce stock trading.

Like Table 2, Table 9 tabulates the person-level statistics for CEO trading. Panel A shows there are 830 CEOs who have traded their own companies' stock in the year from April 4, 2011 to April 3, 2012, and 800 CEOs who have done such transactions in the year after. Their average trading frequency in both years is 4, which does not change. Even though an average CEO does not make transactions frequently, their total transaction value involved in a year has a mean of around ten million dollars, which is much larger than an average congress member. Also different from a congress member, a CEO's annual transaction value increases by 13% after the STOCK Act. We also group CEOs who have made purchases or sales separately, and find that only less than 200 CEOs have ever made insider purchases in these two years, but over 600 CEOs made sales, and they sell more than twice often than they purchase. This is because CEOs get most of the stock through incentive compensations instead of voluntary purchases, and they are more willing to sell when they have liquidity needs or the stock price is high. In either the purchase or the sale sample, CEO mean trading frequency does not change and their mean annual transaction values have increased by 3% and 9% respectively. Panel B using CEOs who trade both before and after the STOCK Act confirms the findings in Panel A, except in the purchase sample whose size is too small to produce any valid conclusion.

We also replicate the firm-year level analysis discussed in Section 3.2 and 4.1 using CEO trading data and report the results in Table 10 and Table 11. Table 9 summarizes firm-year variables in the CEO sample just as in Table 3. The most

obvious difference between Table 10 and Table 3 is that CEOs trade less frequently but with a larger amount, and they have asymmetric patterns for purchases and sales. Table 11 shows that CEO trading frequency and volume increase by 9% and 26% respectively after the passage of STOCK Act when controlling for firm characteristics. Especially given the significantly intensified selling frequency (increases by 12%) and volume (increases by 34%) of CEOs after April 2012 related to the bull market, the reduction of congressional sales (even if the reduction in trading value is tiny) manifests the effectiveness of the STOCK Act.

As a parallel study to Table 7 and Table 8, we show the timing ability of CEOs in Table 12. Because CEOs are required to report their trading within two days, their timing ability measures can be amplified by the disclosure effect. Panel A shows that for both purchases and sales, CEOs have positive timing ability both before and after the passage of STOCK Act with a magnitude much larger than congress members. But different from congress members, their timing ability does not change significantly with the advent of new law. Panel B also confirms this in a multivariate regression framework. Therefore, the changing congressional trading behavior is a consequence of the STOCK Act, rather than due to sporadic correlation.

I have also done the calendar time portfolio analysis with CEO data and report the results in Figure 5. There is not a changing trend of CEO trading profitability after the STOCK Act.

6. Conclusion

This paper intends to find out the impact of newly enacted STOCK Act, and substantiates that STOCK Act has successfully discouraged congress members to engage in intensive financial transactions. Congressional trading frequency and

volume have decreased significantly after the Act. The decrease of congressional trading volume is not merely because congress members happen to choose stocks with reduced turnover. Both event studies and regressions support the conclusion that their timing ability is less advanced than before. The abnormal returns of congressional purchase also turned negative after STOCK Act. A parallel study using CEO insider trading data does not reveal the same phenomenon, suggesting the causality flowing from the STOCK Act to the reduction of congressional trading. Admittedly, this paper lacks data several years before the act to solve anticipation effects due to limited time. Future work should assemble a longer sample period to better assess the influence of STOCK Act.

Appendix: Major Variables Definition

<i>Frequency</i>	The total number of all/purchase/sale transactions by congress members on one stock during one year (from April 4, 2011 to April 3, 2012, or from April 4, 2012 to April 3, 2013).
<i>Value</i>	The total dollar volume of all/purchase/sale transactions by congress members on one stock during one year (from April 4, 2011 to April 3, 2012, or from April 4, 2012 to April 3, 2013).
<i>Ln(Frequency)</i>	The natural log of <i>Frequency</i> .
<i>Ln(Value)</i>	The natural log of <i>Value</i> .
<i>D(STOCK Act)</i>	A dummy variable indicating the post-STOCK Act period, from April 4, 2012 to April 3, 2013.
<i>Ln(assets)</i>	The natural log of total assets at the beginning of fiscal year.
<i>ROA</i>	Net income divided by beginning-of-year total assets.
<i>R&D</i>	Research and development expenses divided by beginning-of-year total assets.
<i>Annual return</i>	Annual stock return calculated as the sum of monthly returns in the fiscal year.
<i>Return volatility</i>	Stock return volatility calculated as the standard deviation of 60 monthly returns prior to the fiscal year end (at least 24 monthly returns are required).

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Figure 1: Quarterly summary of congressional transactions

Our sample includes 14653 congressional stock transactions from April 4, 2011 to April 3, 2013. Figure 1a (1b) shows the number (value) of transactions totaled for each of the eight quarters in this period. The blue line stands for all transactions. The green line stands for purchase transactions. The red line stands for sale transactions. The horizontal axis denotes the respective quarters, with quarter zero representing the effective date of the STOCK Act, April 4, 2012.

1a. Total number of transactions

1b. Total dollar value of transactions

Figure 2: S&P 500 index from April 2011 to April 2013

This is a snapshot from Yahoo Finance website, showing the daily price of S&P 500 index during our sample period, from April 2011 to April 2013.

Figure 3: Trend of calendar time portfolios' alphas

Monthly calendar time portfolios are created based on congressional transactions in the prior month. Then I apply Fama and French (1993) three factor model on the portfolio's value weighted return in the next three months to get the alpha. Finally I plot the trend of alphas in the following figures. Figure 3a (3b) plots the alphas for portfolios based on congressional purchases (sales). Figure 3c plots the alphas for the hedged portfolio.

3a. Congressional purchases

3b. Congressional sales

3c. Hedged portfolio

Figure 4: Quarterly summary of total number of CEO transactions

Our sample includes 7155 CEO stock transactions from April 4, 2011 to April 3, 2013. Figure 1a (1b) shows the number (value) of transactions totaled for each of the eight quarters in this period. The blue line stands for all transactions. The green line stands for purchase transactions. The red line stands for sale transactions. The horizontal axis denotes the respective quarters, with quarter zero representing the effective date of the STOCK Act, April 4, 2012.

4a. Total number of transactions

4b. Total value of transactions

Figure 5: Trend of calendar time portfolios' alphas

Monthly calendar time portfolios are created based on CEO transactions in the prior month. Then I apply Fama and French (1993) three factor model on the portfolio' value weighted return in the next three months to get the alpha. Finally I plot the trend of alphas in the following figures. Figure 5a (5b) plots the alphas for portfolios based on CEO purchases (sales). Figure 5c plots the alphas for the hedged portfolio.

5a. CEO purchases

5b. CEO sales

5c. Hedged portfolio

Table 1: Frequency distribution of transactions by congress members

This table tabulates the numbers of common stock transactions made by congress members from April 4, 2011 to April 3, 2013. Transactions on the same stock by the same person on the same day have been combined to one transaction, and transactions valued at less than \$100 have been eliminated. See section 3.1 for more details about the data source and processing procedures. The first column lists the transaction value categories printed on FDRs, TRs, and PTRs for congress members to choose from. The second column is the midpoint dollar value in each category, which will be used in subsequent analyses in this paper. The third column is the number of purchase transactions which are valued within each category. The fourth column is the number of sale transactions which are valued within each category. The fifth column is the total number of purchase or sale transactions which are valued within each category.

Transaction Value Category	Assigned Value	Purchase	Sale	All
Within \$1,000	\$500	503	969	1,472
\$1,001 to \$15,000	\$8,000	4,085	4,029	8,114
\$15,001 to \$50,000	\$32,500	1,594	1,795	3,389
\$50,001 to \$100,000	\$75,000	425	454	879
\$100,001 to \$250,000	\$175,000	258	328	586
\$250,000 to \$500,000	\$375,000	70	72	142
\$500,001 to \$1,000,000	\$750,000	14	52	66
over \$1,000,000	\$1,000,000	3	2	5
Total		6,952	7,701	14,653

Table 2: Congressional trading frequency and volume before and after STOCK Act

This table tabulates the average frequency of congressional stock transactions under the “Frequency” header, and the average volume of congressional stock transactions under the “Value” header. The “Before” columns represent pre-STOCK Act period, which is the whole year from April 4, 2011 to April 3, 2012. The “After” columns represent the post-STOCK Act period, which is the whole year from April 4, 2012 to April 3, 2013. To fill the cells under “Frequency” and “Value”, we first sum up the annual number or value of transactions for each congress member in the pre- or post-STOCK Act period, and then take the average across congress members. Panel A includes 144 congress members with trading records, among whom 117 with purchase records and 139 with sale records. In Panel A, the first number (on the left of slash) under the “No. of Individuals” column is the number of congress members who have trading records in the pre-STOCK Act period, and the second number (on the right of slash) under the “No. of Individuals” column is the number of congress members who have trading records in the post-STOCK Act period. Panel B includes 94 congress members who stay in the congress during the two years and have trading records in both pre- and post-STOCK Act periods. In Panel B, the number under the “No. of Individuals” column denotes how many congress members satisfy the above selection criteria.

	No. of Individuals	Frequency		Value	
		Before	After	Before	After
Panel A: All congress members with trading records					
All	111/127	78	47	\$2,157,144	\$1,509,000
Purchases	82/95	53	27	\$1,481,994	\$728,079
Sales	96/108	45	32	\$1,228,328	\$1,134,032
Panel B: Congress members who report trading/purchase/sell in both periods					
All	94	90	59	\$2,517,117	\$1,820,782
Purchases	64	67	37	\$1,756,664	\$947,703
Sales	75	55	41	\$1,528,627	\$1,412,807

Table 3: Firm-year level variables for congressional trading

This table tabulates the basic statistics for variables used in firm-year level analysis. In Panel A, the firm-year sample is formed by summing up all congressional transactions on each stock in each of the two sample years. So each firm only has one observation in the pre- or post-STOCK Act period. Panel A sample consists of 2290 observations for 1469 firms. *Frequency* is the total number of transactions by congress members on one stock during one year. *Value* is the total trading volume of congress members on one stock during one year. $\ln(\text{Frequency})$ and $\ln(\text{Value})$ are the natural log of *Frequency* and *Value*. $D(\text{STOCK Act})$ is a dummy variable indicating the post-STOCK Act period, from April 4, 2012 to April 3, 2013. The following firm characteristics variables are based on the most recent prior fiscal year end for each firm. $\ln(\text{assets})$ is the natural log of total assets. *ROA* is net income divided by total assets. *R&D* is research and development expenses divided by total assets. *Annual return* is annual stock return calculated as the sum of monthly returns in the fiscal year. *Return volatility* is stock return volatility calculated as the standard deviation of 60 monthly returns prior to the end of the fiscal year end (at least 24 monthly returns are required). Panel B (Panel C) is similarly constructed by summing up all congressional purchase (sale) transactions on each stock in each of the two sample years. Firm characteristics for Panel B (Panel C) are suppressed to save space.

Variable	Mean	Std.Dev.	Q1	Median	Q3
Panel A: All stocks traded by congress members (2290 observations for 1469 firms)					
<i>Frequency</i>	6.128	8.421	1	3	7
<i>Value</i>	182203	443766	16000	40500	175000
$\ln(\text{Frequency})$	1.245	1.017	0	1.099	1.946
$\ln(\text{Value})$	10.802	1.667	9.680	10.609	12.073
$D(\text{STOCK Act})$	0.486	0.500	0	0	1
$\ln(\text{assets})$	8.595	1.895	7.289	8.526	9.797
<i>ROA</i>	0.058	0.085	0.022	0.055	0.095
<i>R&D</i>	0.025	0.052	0	0	0.025
<i>Annual return</i>	0.190	0.319	0.012	0.177	0.359
<i>Return volatility</i>	0.123	0.056	0.084	0.113	0.145
Panel B: All stocks purchased by congress members (1659 observations for 1172 firms)					
<i>Frequency</i>	4.001	5.058	1	2	4
<i>Value</i>	110246	276306	8000	32000	97500
$\ln(\text{Frequency})$	0.933	0.880	0	0.693	1.386
$\ln(\text{Value})$	10.425	1.564	8.987	10.373	11.488
Panel C: All stocks sold by congress members (1921 observations for 1316 firms)					
<i>Frequency</i>	3.851	4.656	1	2	4
<i>Value</i>	121991	282308	8000	32000	105500
$\ln(\text{Frequency})$	0.908	0.875	0	0.693	1.386
$\ln(\text{Value})$	10.407	1.663	8.987	10.373	11.566

Table 4: Firm-year level analysis of the STOCK Act's impact on congressional transactions

This table tabulates the OLS regressions of congressional trading on the post-STOCK Act period dummy, with the dependent variables listed in the top row of the table. This firm-year level sample consists of 2290 observations for 1469 firms which are traded by congress members from April 4, 2011 to April 3, 2013. All the variables are defined in Table 3. Firm fixed effects are included. Standard errors are controlled for heteroskedasticity and clustered at firm level, shown in parentheses. ***, **, and * represent that the coefficient is significant at 1%, 5%, or 10% level.

	<i>Ln(Frequency)</i>	<i>Ln(Value)</i>
<i>D(STOCK Act)</i>	-0.303*** (0.043)	-0.198*** (0.071)
<i>Ln(assets)</i>	0.144 (0.222)	-0.224 (0.337)
<i>ROA</i>	0.446 (0.702)	0.719 (1.142)
<i>R&D</i>	1.846 (1.923)	-0.430 (2.443)
<i>Annual return</i>	0.236*** (0.091)	0.336** (0.154)
<i>Return volatility</i>	-8.102* (4.220)	-1.215 (7.100)
<i>Constant</i>	1.034 (2.052)	12.874*** (3.069)
Firm FE	Yes	Yes
No. of Obs.	2290	2290
Within R ²	0.1331	0.0405

Table 5: Firm-year level analysis of the STOCK Act's impact on congressional transactions – purchases and sales separately

This table tabulates the OLS regressions of congressional trading frequency and volume on the post-STOCK Act period dummy, classified by transaction directions. Purchase sample consists of 1659 firm-year level observations for 1172 stocks which are purchased by congress members from April 4, 2011 to April 3, 2013. Sale sample consists of 1921 firm-year level observations for 1316 stocks which are sold by congress members from April 4, 2011 to April 3, 2013. All the variables are defined in Table 3. Firm fixed effects are included. Standard errors are controlled for heteroskedasticity and clustered at firm level, shown in parentheses. ***, **, and * represent that the coefficient is significant at 1%, 5%, or 10% level.

	Purchase sample		Sale sample	
	<i>Ln(Frequency)</i>	<i>Ln(Value)</i>	<i>Ln(Frequency)</i>	<i>Ln(Value)</i>
<i>D(STOCK Act)</i>	-0.378*** (0.056)	-0.244** (0.102)	-0.193*** (0.044)	-0.024 (0.099)
<i>Ln(assets)</i>	-0.093 (0.316)	-0.693 (0.453)	0.188 (0.189)	0.105 (0.528)
<i>ROA</i>	1.246 (1.097)	0.691 (1.874)	-0.336 (0.756)	0.290 (1.516)
<i>R&D</i>	0.175 (1.910)	-3.879 (4.016)	7.522*** (3.722)	5.647 (6.278)
<i>Annual return</i>	0.236** (0.119)	0.727*** (0.201)	0.275*** (0.094)	0.114 (0.194)
<i>Return volatility</i>	-6.925 (5.441)	1.285 (9.169)	-5.182 (5.065)	4.055 (11.636)
<i>Constant</i>	2.622 (2.975)	16.346*** (4.148)	-0.246 (1.882)	8.831 (5.252)
Firm FE	Yes	Yes	Yes	Yes
No. of Obs.	1659	1659	1921	1921
Within R ²	0.1949	0.1087	0.0888	0.0015

Table 6: STOCK Act's impact on trading volume – congress members versus all investors

This table is aimed to compare congressional trading and all investor's trading pattern. Panel A summarizes the trading volume by all investors of those stocks that are ever traded by congress members either before or after the passage of STOCK Act. In Panel B, columns headed by "Congress members" display the coefficients of STOCK Act dummy taken from the columns headed by $Ln(Value)$ in Table 4 and Table 5. Columns headed by "All investors" in Panel B apply the same set of models but replace the dependent variables with log trading value of all investors for the same set of stocks. Other control variables are not shown in Panel B for brevity. Panel C runs the regressions similar to the columns headed by $Ln(Value)$ in Table 4 and Table 5, but adds *All investors' trading* to the original set of control variables. In Panel B and Panel C, standard errors are controlled for heteroskedasticity and clustered at firm level, shown in parentheses. ***, **, and * represent that the coefficient is significant at 1%, 5%, or 10% level.

Panel A: Stock level summary – total trading volume by congress members only and all investors							
		All		Purchase		Sale	
		N	Mean	N	Mean	N	Mean
Before	Congress members	1178	\$196,315	915	\$127,996	995	114,716
	All investors	1178	\$29,385,842,089	915	\$34,852,518,406	995	33013204306
After	Congress members	1112	\$167,253	744	\$88,417	926	129809
	All investors	1112	\$26,451,194,428	744	\$33,410,375,550	926	29776756139

Panel B: Comparison of total trading volume in a multivariate regression						
	Congress members			All investors		
	All	Purchase	Sale	All	Purchase	Sale
$D(STOCK Act)$	-0.198*** (0.071)	-0.244** (0.102)	-0.024 (0.099)	-0.088*** (0.017)	-0.096*** (0.019)	-0.093*** (0.021)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	2290	1659	1921	2290	1659	1921

Panel C: Regression of congressional trading on STOCK Act dummy after controlling for total trading volume			
	All	Purchase	Sale
$D(STOCK Act)$	-0.152** (0.071)	-0.228** (0.106)	0.036 (0.098)
$Ln(assets)$	-0.299 (0.341)	-0.697 (0.458)	0.027 (0.508)
ROA	0.470 (1.132)	0.628 (1.868)	-0.025 (1.496)
R&D	1.651 (2.561)	-3.218 (4.256)	6.360 (6.470)
Annual return	0.261* (0.153)	0.706*** (0.206)	0.022 (0.190)
Return volatility	1.341 (6.976)	2.081 (9.508)	7.302 (11.005)
<i>All investors' trading</i>	0.516*** (0.149)	0.165 (0.293)	0.652*** (0.208)
Constant	1.294 (4.155)	12.449 (8.287)	-5.990 (6.847)
Firm FE	Yes	Yes	Yes
No. of Obs.	2290	1659	1921
Within R ²	0.0526	0.1095	0.0181

Table 7: Contrasting congressional trading timing ability before and after STOCK Act

This table employs the event study method to keep track of congressional trading timing ability both pre- and post-STOCK Act. Each purchase or sale transaction by a congress member is treated as an event, and the transaction date is the event day. This event study is conducted under a market model framework, and CRSP value weighted return is used as the market return. We mainly focus on the buy-and-hold abnormal returns (BHAR) within a broad window of $[-20, 20]$. Hence, a 240-day estimation window of $[-260, -21]$ is chosen, with 60 as the minimal acceptable length. Our timing ability is proxied by the difference in BHARs before and after the transaction date. The “Timing” column presents three different window lengths. The “Before” column shows the congressional timing ability before STOCK Act. The “After” column shows the congressional timing ability after STOCK Act. In each cell of the “Before” and “After” columns, the top number is the mean timing ability of all associated transactions, the first number in the parentheses below is the number of associated transactions, and the second number in the parentheses is the t-statistic testing whether the mean timing ability is significantly different from 0. The “Difference” column equals the “After” column minus the “Before” column, and t-statistic testing whether the difference is significant is reported in the parentheses below. Considering the distinct features for transactions in opposite directions, we define the timing ability for stock purchase and sales differently and present them in Panel A and Panel B separately. ***, **, and * represent that the t statistic is significant at 1%, 5%, or 10% level.

Timing	Before	After	Difference
Panel A: Purchase			
$BHAR[1, 20] - BHAR[-20, -1]$	0.50%*** (4229, 3.3)	-0.32%* (2515, -1.7)	-0.82%*** (-3.4)
$BHAR[1, 10] - BHAR[-10, -1]$	0.36%*** (4229, 3.2)	-0.22%* (2515, -1.7)	-0.59%*** (-3.3)
$BHAR[1, 5] - BHAR[-5, -1]$	0.05% (4229, 0.6)	-0.27*** (2515, -2.9)	-0.32%*** (-2.6)
Panel B: Sale			
$BHAR[-20, -1] - BHAR[1, 20]$	0.10% (4137, 0.6)	-0.49%*** (3306, -3.0)	-0.59%*** (-2.6)
$BHAR[-10, -1] - BHAR[1, 10]$	0.02% (4137, 0.2)	-0.52%*** (3306, -4.5)	-0.55%*** (-3.4)
$BHAR[-5, -1] - BHAR[1, 5]$	-0.01% (4137, -0.11)	-0.37%*** (3306, -4.6)	-0.36%*** (-3.2)

Table 8: STOCK Act's impact on congressional timing ability – purchases and sales separately

This table tabulates the OLS regression results of congressional timing ability on the STOCK Act dummy, classified by transaction direction. The purchase sample consists of 6445 purchase transactions done by 106 congress members from April 4, 2011 to April 3, 2013, and the dependent variable is $BHAR[1, 10] - BHAR[-10, -1]$ defined in Table 6. The sale sample consists of 7159 sale transactions done by 125 congress members from April 4, 2011 to April 3, 2013, and the dependent variable is $BHAR[-10, -1] - BHAR[1, 10]$ defined in Table 6. All independent variables are defined in Table 3. Congress member fixed effects are included. Standard errors are controlled for heteroskedasticity and clustered at firm level, shown in parentheses. ***, **, and * represent that the coefficient is significant at 1%, 5%, or 10% level.

	Purchase	Sale
<i>D(STOCK Act)</i>	-0.004* (0.002)	-0.004** (0.002)
<i>Ln(assets)</i>	0.000 (0.001)	-0.000 (0.001)
<i>ROA</i>	-0.009 (0.012)	-0.017 (0.013)
<i>R&D</i>	0.014 (0.023)	0.005 (0.024)
<i>Annual return</i>	0.005 (0.004)	0.003 (0.004)
<i>Return volatility</i>	-0.037 (0.024)	-0.014 (0.022)
<i>Constant</i>	0.007 (0.013)	-0.029*** (0.007)
Congress Member FE	Yes	Yes
No. of Obs.	6445	7159
Within R ²	0.0389	0.0226

Table 9: CEO trading frequency and volume before and after STOCK Act

This table tabulates the average frequency of CEO stock transactions under the “Frequency” header, and the average volume of CEO stock transactions under the “Value” header. The “Before” columns represent pre-STOCK Act period, which is the whole year from April 4, 2011 to April 3, 2012. The “After” columns represent the post-STOCK Act period, which is the whole year from April 4, 2012 to April 3, 2013. To fill the cells under “Frequency” and “Value”, we first sum up the annual number or value of transactions for each CEO in the pre- or post-STOCK Act period, and then take the average across CEOs. Panel A includes 1108 CEOs with trading records, among whom 272 with purchase records and 909 with sale records. In Panel A, the first number (on the left of slash) under the “No. of Individuals” column is the number of CEOs who have trading records in the pre-STOCK Act period, and the second number (on the right of slash) under the “No. of Individuals” column is the number of CEOs who have trading records in the post-STOCK Act period. Panel B includes 522 CEOs who have trading records in both pre- and post-STOCK Act periods. In Panel B, the number under the “No. of Individuals” column denotes how many CEOs traded in both periods. Note that all associated firms in this table have also ever been traded by congress members during the sample period.

	No. of individuals	Frequency		Value	
		Before	After	Before	After
Panel A: All CEOs with trading records					
All	830/800	4	4	\$9,512,800	\$10,760,799
Purchases	190/135	2	2	\$756,391	\$777,894
Sales	672/677	5	5	\$11,535,580	\$12,560,744
Panel B: CEOs who report trading/purchase/sell in both periods					
All	522	5	5	\$10,285,099	\$10,993,667
Purchases	53	3	3	\$1,766,632	\$68,168
Sales	440	5	5	\$11,823,552	\$12,618,173

Table 10: Firm-year level variables for CEO trading

This table tabulates the basic statistics for variables used in firm-year level analysis. In Panel A, the firm-year sample is formed by summing up all CEO transactions on each stock in each of the two sample years. So each firm only has one observation in the pre- or post-STOCK Act period. Panel A sample consists of 1470 observations for 961 firms. *Frequency* is the total number of transactions by CEOs on one stock during one year. *Value* is the total trading volume of CEOs on one stock during one year. $\ln(\text{Frequency})$ and $\ln(\text{Value})$ are the natural log of *Frequency* and *Value*. *D(STOCK Act)* is a dummy variable indicating the post-STOCK Act period, from April 4, 2012 to April 3, 2013. The following firm characteristics variables are based on the most recent prior fiscal year end for each firm. $\ln(\text{assets})$ is the natural log of total assets. *ROA* is net income divided by total assets. *R&D* is research and development expenses divided by total assets. *Annual return* is annual stock return calculated as the sum of monthly returns in the fiscal year. *Return volatility* is stock return volatility calculated as the standard deviation of 60 monthly returns prior to the end of the fiscal year end (at least 24 monthly returns are required). Panel B (Panel C) is similarly constructed by summing up all CEO purchase (sale) transactions on each stock in each of the two sample years. Firm characteristics for Panel B (Panel C) are suppressed to save space. Note that all associated firms in this table have also ever been traded by congress members during the sample period.

Variable	Mean	Std.Dev.	Q1	Median	Q3
Panel A: All stocks traded by CEOs (1470 observations for 961 firms)					
<i>Frequency</i>	4.416	5.611	1	2	5
<i>Value</i>	10251853	22401463	861443	3487878	10540770
$\ln(\text{Frequency})$	0.984	0.938	0.000	0.693	1.609
$\ln(\text{Value})$	14.791	1.920	13.666	15.065	16.171
<i>D(STOCK Act)</i>	0.493	0.500	0	0	1
$\ln(\text{assets})$	8.097	1.678	6.920	8.004	9.170
<i>ROA</i>	0.058	0.086	0.022	0.058	0.098
<i>R&D</i>	0.030	0.059	0	0	0.033
<i>Annual return</i>	0.219	0.324	0.028	0.198	0.397
<i>Return volatility</i>	0.128	0.058	0.089	0.117	0.151
Panel B: All stocks purchased by CEOs (278 observations for 236 firms)					
<i>Frequency</i>	2.076	2.623	1	1	2
<i>Value</i>	671892	1657589	82417	199145	538280
$\ln(\text{Frequency})$	0.411	0.658	0	0	0.693
$\ln(\text{Value})$	12.226	1.634	11.320	12.202	13.196
Panel C: All stocks sold by CEOs (1235 observations for 801 firms)					
<i>Frequency</i>	4.789	5.884	1	3	5
<i>Value</i>	12051366	24006609	1594701	4672026	12789121
$\ln(\text{Frequency})$	1.071	0.947	0	1.099	1.609
$\ln(\text{Value})$	15.249	1.613	14.282	15.357	16.364

Table 11: Firm-year level analysis of the STOCK Act's impact on CEO transactions

This table tabulates the OLS regressions of CEO trading on post-STOCK Act dummy, with dependent variables listed in the top row. Panel A consists of 1470 firm-year level observations for 961 firms which are ever traded by CEOs from April 4, 2011 to April 3, 2013. Panel B consists of 278 firm-year level observations for 236 firms which are ever purchased by CEOs from April 4, 2011 to April 3, 2013. Panel C consists of 1235 firm-year level observations for 801 firms which are ever sold by CEOs from April 4, 2011 to April 3, 2013. Note that all associated firms in this table have also ever been traded by congress members during the sample period. All variables are defined in Table 9. Firm characteristics are controlled for but suppressed to save space. Firm fixed effects are included. Standard errors are controlled for heteroskedasticity and clustered at firm level, shown in parentheses. ***, **, and * represent that the coefficient is significant at 1%, 5%, or 10% level.

	<i>Ln(Frequency)</i>	<i>Ln(Value)</i>
Panel A: All		
<i>D(STOCK Act)</i>	0.083 (0.056)	0.228** (0.103)
Firm Characteristics (Omitted)		
Firm FE	Yes	Yes
No. of Obs.	1470	1470
Within R ²	0.0159	0.0151
Panel B: Purchases		
<i>D(STOCK Act)</i>	-0.019 (0.174)	0.116 (0.349)
Firm Characteristics (Omitted)		
Firm FE	Yes	Yes
No. of Obs.	278	278
Within R ²	0.0737	0.1265
Panel C: Sales		
<i>D(STOCK Act)</i>	0.117* (0.063)	0.292*** (0.100)
Firm Characteristics (Omitted)		
Firm FE	Yes	Yes
No. of Obs.	1235	1235
Within R ²	0.0217	0.0277

Table 12: CEO trading timing ability and the STOCK Act

Panel A employs the event study method to keep track of CEO trading timing ability both pre- and post-STOCK Act. Each purchase or sale transaction by a CEO is treated as an event, and the transaction date is the event day. The calculation of BHARs is the same as in Table 6. The “Before” column shows CEO timing ability before STOCK Act. The “After” column shows CEO timing ability after STOCK Act. In each cell of the “Before” and “After” columns, the top number is the mean timing ability of all associated transactions, the first number in the parentheses below is the number of associated transactions, and the second number in the parentheses is the t-statistic testing whether the mean timing ability is significantly different from 0. The “Difference” column equals the “After” column minus the “Before” column, and t-statistic testing whether the difference is significant is reported in the parentheses below. Considering the distinct features for transactions in opposite directions, we define the timing ability for stock purchase and sales differently.

Panel B tabulates the OLS regression results of CEO timing ability on post-STOCK Act dummy classified by transaction direction. The purchase sample consists of 576 purchase transactions done by 237 CEOs from April 4, 2011 to April 3, 2013, and the dependent variable is $BHAR[1, 10] - BHAR[-10, -1]$ defined in Panel A. The sale sample consists of 5894 sale transactions done by 848 CEOs from April 4, 2011 to April 3, 2013, and the dependent variable is $BHAR[-10, -1] - BHAR[1, 10]$ defined in Panel A. All independent variables are defined in Table 9. Firm characteristics are controlled for but suppressed to save space. CEO fixed effects are included. Standard errors are controlled for heteroskedasticity and clustered at firm level, shown in parentheses.

Note that all associated firms in this table have also ever been traded by congress members during the sample period. ***, **, and * represent that the t statistic is significant at 1%, 5%, or 10% level.

Panel A: Contrasting CEO trading timing ability before and after STOCK Act			
Timing	Before	After	Difference
Purchase: $BHAR[1, 10] - BHAR[-10, -1]$	5.21%*** (372, 9.9)	5.14%*** (288, 9.4)	-0.07% (-0.1)
Sale: $BHAR[-10, -1] - BHAR[1, 10]$	1.70%*** (3186, 11.6)	1.53%*** (3266, 11.7)	-0.17% (-0.9)

Panel B: STOCK Act's impact on CEO timing ability – purchases and sales separately		
	Purchase	Sale
$D(STOCK Act)$	-0.012 (0.027)	-0.000 (0.004)
Firm Characteristics (Omitted)		
CEO FE	Yes	Yes
No. of Obs.	576	5894
Within R ²	0.5987	0.2234