Government Subsidies and Corporate Fraud

Aneesh Raghunandan*

Stanford University

January 5, 2016

Abstract

I study the relation between firms' receipt of significant subsidies and their subsequent propensities to engage in – and be caught engaging in – financial fraud. Firms who receive subsidies are likely to have greater influence over the politicians who award these subsidies (regulatory capture), but are also more likely to be subject to external scrutiny. Consistent with the idea of regulatory capture, I find that firms that receive tax breaks (governmental revenue decreases) tend to engage more frequently in fraudulent activity, and are less likely to be caught engaging in fraud by regulators and third parties conditional on engaging. However, such firms are less likely to engage as the magnitude of the tax break received increases. Conversely, firms that receive direct cash grants (governmental spending increases) or below-market-rate access to resources do not on average engage in fraud more or less frequently than those who do not, although these firms are also less likely to be caught when they do engage.

*I thank Lisa De Simone for advising me on this project; Phil Mattera of Good Jobs First for providing me with data; Cindy Lu for providing research assistance; and Shai Bernstein, Beth Blankespoor, Ed deHaan, Kurt Gee, Rebecca Lester, Ken Li, Ivan Marinovic, Charles McClure, Maureen McNichols, Trung Nguyen, and Christina Zhu for helpful comments and suggestions. All errors are my own.

1 Introduction

Billions of dollars in subsidies are awarded by all levels of government each year. The stated reason for virtually all of these subsidies is to promote some form of "economic development" or "economic growth" and there is a significant literature on whether or not these subsidies achieve their stated purpose (see, for example, Cohen et al (2011)). Little has been said, however, about what these subsidies mean in terms of firms' relationships with regulators and the resulting effects on firm behavior. In this paper I study the relation between receiving government subsidies (e.g., tax breaks, no- or low-interest loans, and cash grants) and firms' willingness to engage in misconduct. Is receiving government aid associated with a higher likelihood of firms (mis)behaving?

There is an argument to be made for an effect in either direction. Firms could employ better governance because these tax breaks and other subsidies are often heavily publicized, with the amount of publicity increasing in the magnitude of the subsidy.[1] Under public scrutiny, a firm may find it beneficial to "lie low" as detection of wrongdoing is more likely, even if not by government officials. This explanation is supported by literature suggesting that firms facing public scrutiny face more pressure to be "good corporate citizens" (e.g., Hanlon and Slemrod (2009), in the context of tax shelters). Third parties such as analysts, the media, and investors also have incentives to detect cheating when it occurs, for reputational as well as financial reasons. This scrutiny results in several sources being responsible for detection; Dyck, Morse, and Zingales (2010) analyze the types of entities that detect corporate fraud, and show that a substantial portion of externally detected fraud is caught by non-regulatory parties. This literature suggests that the threat of adverse effects to

[1] For example, searching for Boeing's 10 largest tax breaks of the past ten years on Google News gives a nearly monotonically increasing relationship between the number of hits and dollar value of the subsidy

reputational costs may keep a firm in line, even if regulatory capture is present.

On the other hand, politicians have their own incentives. Legislators responsible for granting subsidies do not want their names tarnished by scandal, as this is likely to adversely affect their chances of reelection. This is especially true in the case of large subsidies, when positive externalities of the deal to regional economies are heavily publicized.[2] A subsequent fallout from such a deal could lead to adverse publicity for the candidate and even corruption charges, and to this end politicians who award subsidies may push for enforcement agencies or the media to turn a blind eye to a subsidized firm engaging in fraud.[3] Subsidy recipients could use this low enforcement as a signal of a weak or reluctant regulator and conclude they have more free reign to engage in questionable actions such as earnings management or business practices, due to a reduced likelihood of being caught.

While I expect that subsidy receipt will be associated with firms engaging in misconduct more frequently, in light of the competing arguments above there is no ex-ante obvious effect of a corporate subsidy on firm behavior. To empirically test the relation, I obtain a dataset on corporate subsidies from the nonprofit watchdog group Good Jobs First. Good Jobs First describes itself as[4]

[2]Note that the magnitude of the subsidy and related costs are frequently ignored. For example, when Hankook Tire Co. built a new plant in Clarksville, TN, an article in USA Today wrote: "Hankook will invest over $800 million for the new state-of-the-art plant, its first in the United States. The new plant will provide additional capacity for Hankook's growing business in the U.S. market and create approximately 1,800 full-time jobs for the region." The article goes on to describe Hankook Tire and praise the parties involved in completing the deal, especially Gov. Bill Haslam and Mayor Kim Millan, without any mention of the $150.6 million tax break Hankook received as an incentive. Source: `http://www.usatoday.com/story/money/cars/2013/10/14/hankook-tire-manufacturing-facility-clarksville-tennessee/2980689/`

[3]Although state legislators are not directly responsible for detecting fraud, state attorneys general play a significant role in the prosecution of fraud as well as in aiding the SEC in its detection process (Coffee and Sale (2009)). A captured state government, therefore, would be less willing to aid the SEC and more willing to dismiss a lawsuit against the firm.

[4]From the homepage of `www.goodjobsfirst.org`

[T]he nation's leading resource for grass roots groups and public officials seeking to make economic development subsidies more accountable and effective.

The dataset, called Subsidy Tracker, is an extensive source on federal, state, and local subsidies awarded to individual firms. I match subsidies awarded to the roughly 2000 largest publicly traded firms to financial and other data.

To test my hypotheses, I then estimate via a partially-observed bivariate probit model (1) the effects of receiving a subsidy on firms' willingness to engage in fraudulent activity as well as (2) the likelihood they will be caught engaging in fraudulent activity. For the purposes of this paper, I define fraudulent activity as either receiving an Accounting and Auditing Enforcement Release (AAER) from the SEC or paying a nontrivial settlement[5] in a shareholder lawsuit. Further details on these data are provided in Section 5.

Because governmental spending increases and revenue cuts are often treated differently in the media and can come with differing political implications, I construct the subsidy variable in multiple ways. I initially study the set of all types of subsidies without differentiating by type, and subsequently partition my subsidy data into tax breaks (revenue decreases) and non-tax-based subsidies (spending increases). I find that firms that receive any kind of subsidy have a probability of 2.1% higher to engage in fraudulent activity.[6] The average subsidy recipient also enjoys a conditional probability of being caught that is lower by 11.9% than a similar unsubsidized firm. I do not make a claim as to the direction of causality (subsidies leading to capture as compared to capture resulting in subsidies). As it is not my goal in this paper to study the determinants of regulatory capture, my

[5] Firms are almost never found guilty in investor lawsuits, generally agreeing to a monetary settlement instead; therefore, nontrivial settlements provide evidence that material wrongdoing very likely occurred

[6] That is, assume the average firm were initially non-subsidized, and that it would engage in fraud with probability q. If that firm received a subsidy, it would then engage on average with probability $q + 0.021$.

findings are consistent with either direction.

My findings provide insight into the relation between firms on firm behavior beyond investment decisions. Recent GASB regulations (GASB Statement No. 77) require state and local governments to provide increased disclosures on corporate tax breaks[7], with the goal of increasing to increase politicians' accountability in the wake of recent regional budgetary issues. My study informs this policy debate by shedding light on additional potential consequences of these subsidies. This information could provide evidence in favor of GASB No. 77 and other similar regulations, or even suggest that these regulations should be strengthened. Further, while it is unlikely that corporate subsidies will significantly lessen, a greater understanding of subsidies' potential indirect consequences could also lead to more well-designed tax abatements and grants. In particular, these could include stronger provisions for negating awards if wrongdoing is detected. Finally, most studies relating money in politics to fraud consider the flow of money from firms to politicians. In contrast, the current study relates fraud to the flow of money from politicians to firms.

2 Background

Awarding subsidies to specific private-sector firms is a commonly employed tool by all levels of government with the intent of stimulating economic growth. State and local governments frequently use these subsidies as incentives for companies to relocate operations. State and local governments also have more freedom to award these subsidies than the federal government: 73.5% of all subsidies in my dataset by dollar value are non-federal, and the top 24 subsidies are all awarded by state or local governments. For example, the

[7] http://www.gasb.org/cs/ContentServer?c=GASBContent_C&pagename=GASB%2FGASBContent_C%2FGASBNewsPage&cid=1176166284793

largest federal subsidy is worth $590 million, whereas the largest state subsidy is worth nearly 15 times as much at $8.7 billion.

These subsidies are awarded in several ways. Many of the largest subsidies are tax breaks, including property tax exemptions or sales tax exemptions. Other methods of subsidizing companies involve direct cash payments or reimbursements for approved activities, as well as discounted access to resources. For example, Alcoa struck a deal with New York State that allowed it to pay 25% of the going rate for electricity for 30 years, a savings valued at roughly $5.6 billion. Many of the largest subsidies are package deals consisting of multiple types of subsidies, as in the case of Boeing's 2013 tax breaks from the state of Washington, the single largest subsidy package ever awarded in the US at $8.7 billion. In Boeing's case, the largest parts of the subsidy package consisted of property tax exemptions as well as a reduction of the business-and-occupation tax rate. I provide further examples of substantial subsidy packages in the appendix. I consider two classes of subsidies: reductions to governmental revenue (tax breaks) and increases in governmental spending (non-tax-breaks, hereafter referred to as "grants"). I consider total subsidy value rather than per-year subsidy value as these are the numbers used by politicians and the media when discussing subsidies. In the case of Alcoa, for example[8], major news outlets reported the $5.6 billion figure but not yearly figures (either broken down by year, or a simple yearly average). This is also why I allocate the entirety of the subsidy to the year it was awarded, rather than to the years in which the firms actually receive the monetary benefit. My goal is to study the impact of *receiving* a subsidy rather than the direct impact of the subsidy itself.

[8] `http://www.forbes.com/pictures/emeh45mfmk/no-2-alcoa-new-york-state-5-6-billion-3/`

3 Prior Literature

My study relates to two areas of the literature; (1) the causes and effects of financial fraud and (2) the role of money in politics.

3.1 Corporate Fraud

I draw upon the work of Dyck, Morse and Zingales (2010, 2013) as well as Wang, Winton, and Yu (2010) and Wang (2013). Dyck et al (2010, 2013) use securities lawsuits as their fraud indicator, while Wang et al (2010) and Wang (2013) use both securities lawsuits and Accounting and Auditing Enforcement Actions (AAERs) issued by the SEC as their fraud indicator. I follow the approach of Wang et al (2010) and Wang (2013) in order to obtain greater coverage of fraudulent activity. This approach mitigates some of the limitations raised by Karpoff, Koester, Lee, and Martin (2014) of only using one type of fraud indicator.

My econometric approach also closely follows Dyck et al (2013), Wang et al (2010), and Wang (2013). These studies use a partial observability bivariate probit approach in order to disentangle firms' incentive to cheat from the conditional probability of being detected. To satisfy the exclusion restriction of the bivariate probit, i.e., to choose variables that are likely to affect the probability of cheating but not the probability of detection, I draw from a wide literature linking managerial compensation to financial fraud (Burns and Kedia (2006); Efendi et al (2007); Armstrong et al (2010); Dyck et al (2010, 2013)). These variables will be discussed in further detail in Section 4.

3.2 Money in Politics

I also draw upon a substantial literature on the effects of the politician-to-firm monetary flow. Faccio et al (2006) demonstrate a positive relation between firms' political connections and the likelihood of receiving a substantial government bailout across a small but multinational sample between 1997 and 2002. Tahoun and van Lent (2013) also examine bailouts, but consider financial institutions in the United States in a more recent time period; they find that financial institutions with greater levels of holdings by politicians have a higher probability of receiving government bailouts under the 2008 Emergency Economic Stabilization Act. Tahoun (2014) subsequently constructs a measure of the interconnectedness between firms and politicians and uses this to show that firms with stronger ties to politicians (measured by politicians' personal financial portfolios) receive more government contracts. Collectively, these papers suggest a substantial return on investment that firms make in politicians. I study one other major component of this ROI, in the form of subsidies and the potential effects of regulatory capture.

Previous literature on government subsidies has primarily studied whether and how governmental spending competes with private-sector investment. Cohen et al (2011) use a dataset of federal subsidies, finding that government spending, to a large extent, crowds out private sector investment and employment. Relatedly, they provide evidence that while there are only modest linkages between congressional representation and the geographic distribution of spending, there is a much stronger association between congressional committee representation and the distribution of spending (also see Aghion et al (2009)). Under the assumption that the state and local subsidy process works similarly, these findings suggest that the locations of subsidy-offering areas are somewhat exogenous relative to the business environments of these counties. That is, whether a city/county/state offers a subsidy is more related to its politicians' incentives than to other aspects of its business environment. This allows me to compare firms operating in different geographic areas and to use instrumental variables based on political events.

A few papers study the intersection of these two subjects. Perhaps the most similar study to this one is Correia (2014), who studies the effect of corporate lobbying expenditures on the likelihood of SEC enforcement actions and magnitude of penalties conditional on receiving an enforcement action. She finds that firms that spend more on lobbying, and thus enjoy cozier relationships with regulators, are less likely to receive SEC enforcement actions; and when these firms do receive SEC enforcement actions, the financial penalties are lower than they otherwise would be. However, Correia's (2014) study – along with most papers relating money in politics to misconduct – study consider the firm-to-politician monetary flow, in part due to the availability of comprehensive data on corporate lobbying expenditures. The Good Jobs First data allows me to study the effect of a different type of monetary linkage between firms and politicians. To the best of my knowledge, mine is one of the first studies that attempts to link the politician-to-firm money flow with corporate fraud.

4 Hypotheses and Research Design

The literature on corporate fraud discussed in the previous section leads me to the following hypothesis (written in the alternative) about the subsidy effect:

Hypothesis 1. *Receipt of a subsidy is positively associated with the propensity to engage in fraud.*

I do not have a prediction as to whether larger, more noteworthy subsidies are more or less positively associated than medium-sized subsidies with firms' propensities to engage in fraud. It is unclear ex ante whether regulatory capture dominates or whether the presence of external monitoring overrides capture. Put another way, I do not have a prediction for the sign or significance of the coefficient on the subsidy-magnitude term.

There is also a potential distinction between state and federal subsidies. Given that most enforcement agencies are at the federal level (e.g., the SEC), one might expect a difference between the effects of the two; specifically, one might expect that federal subsidies are more indicative of regulatory capture. However, there are significant political links between regional politicians and federal regulatory agencies. The US Congress has substantial influence over the SEC, and of the 100 voting members of the US Senate in 2014, 10 were former state governors while another 4 were former lieutenant governors.[9] Furthermore, while regional politicians are unlikely to have as much of a direct influence on the SEC's Washington, D.C. office as federal politicians, it is plausible that they may have influence over regional offices of enforcement agencies. These politicians could therefore influence the likelihood of detecting fraud; Kedia and Rajgopal (2011) provide evidence that the location of SEC regional offices plays a role in the enforcement process, and DeFond et al (2015) show that auditors located closer to SEC regional offices issue far more going concern opinions.

External monitoring of subsidies likely also has a substantial local and state component. Malloy (2005) finds that local analysts tend to be more accurate while Ivkovic and Weisbenner (2005) find that investors are likely better informed about local investments. Finally, Galvin (2008) describes the significant role of state-level securities regulators in initiating enforcement action, while Coffee and Sale (2009) find that state attorneys general play a substantial role in prosecuting fraud and in aiding the SEC. Coffee and Sale (2009), however, detail the susceptibility of state-level securities regulators to capture. A captured

[9] `https://www.fas.org/sgp/crs/misc/R42964.pdf`

state government could lead to a lower likelihood of punishment, whether via less cooperation with the SEC or via lighter prosecution in state-level courts. Given these linkages, I therefore do not distinguish between state-funded and federally-funded subsidies.

4.1 Tax Breaks

As mentioned previously, I do distinguish between tax breaks and non-tax breaks (hereafter referred to as grants or cash grants). Tax breaks are a method of reducing governmental revenue without directly altering other spending, while grants increase governmental spending without directly altering government revenue. Both popular press and academic research consider altering spending and altering revenue to be distinct actions.[10] The distinction between tax cuts (increases) and spending increases (reductions) is often a highly political issue as well, and hence such a distinction may be important. This leads to the following hypothesis:

Hypothesis 2. *Tthe magnitude of tax breaks' effect on firms' decisions to engage in fraud will be stronger than the magnitude of the effects of grants.*

Consistent with this hypothesis is the idea that tax breaks are associated with higher regulatory capture. Many cash grants are the result of a competitive application process, while tax breaks tend not to be. This could make tax breaks more likely to be viewed with skepticism. Tax breaks also tend to last for a longer duration, meaning that on average awarding a tax break to a firm creates a direct linkage between lawmakers awarding a subsidy and the recipient firm for a longer period of time. I test this hypothesis by conducting

[10] For an example of academic research, see Alesina and Ardagna (2010). For a few examples of popular press, see an op-ed by Christina Romer at `http://www.nytimes.com/2011/07/03/business/economy/03view.html?_r=0`, or a USA Today op-ed contrasting the two at `http://www.usatoday.com/story/opinion/2012/11/15/tea-party-fiscal-cliff-cut-spending/1707961/`

tests separately for each of these two types of subsidies and comparing coefficients on the subsidy-based variables. My econometric approach is detailed below.

4.2 Partial Observability Bivariate Probit

It is generally not possible to observe a firm that cheats but does not get caught. The investor lawsuit and AAER data only include observations on firms who were caught cheating, as indicated by either a nontrivial lawsuit settlement amount or the receipt of an AAER. Therefore, to try to disentangle the effect of subsidies on firms' incentives to cheat from the effect of subsidies on firms' likelihood of being caught, I use a partially observed bivariate probit framework (see Poirier (1980) for technical details). This framework is applied to a corporate fraud setting in Dyck et al (2010), Wang et al (2010), and Wang (2013). Standard probit and logit models cannot distinguish a firm that is likely to cheat – but unlikely to be caught – from a firm that is unlikely to cheat but likely to be caught. However, these two types of firms are likely different. A partial observability bivariate probit allows me to distinguish between these two types of firms.

The partial observability bivariate probit is a method to estimate two equations with binary dependent variables when it is only possible to observe the product of the two binary dependent variables. In this case, the two binary variables are (1) whether the firm decides to cheat and (2) whether the firm is caught cheating conditional on deciding to engage. This does not directly take into account the magnitude of cheating. However, estimating a bivariate probit with one of the latent variables being continuous is much more difficult, and estimating the probability that a firm will engage would likely lead to similar insights to those from a procedure attempting to estimate a latent level of cheating; the only difference is in the interpretation (the determinants of "more cheating" as compared to the determinants of "more probable to cheat").

Estimating the two equations of a partial-observability bivariate probit requires a set of variables likely to affect both equations, as well as a set of variables that satisfy an exclusion restriction in the sense that they are likely to affect the probability of one of the two outcome variables but not the other. More specifically, I simultaneously estimate the following two equations:

$$\mathbb{P}(\text{cheat}) = X\beta_1 + Z_1\gamma_1 + \varepsilon \tag{1}$$

$$\mathbb{P}(\text{caught}|\text{cheat}) = X\beta_2 + Z_2\gamma_2 + u \tag{2}$$

where Z_1 and Z_2 are mutually exclusive and nonempty. The error terms ε and u must be normally distributed per the assumptions of Poirier (1980), but do not need to be independent of one another.

4.3 Variables

Prior literature suggests using executive compensation metrics as Z_1 to proxy for managerial incentives. Following Dyck et al (2010, 2013) I use managerial incentive pay as well as unexercised exercisable options. I rely on the temporal dynamic of required managerial compensation disclosures to make this a feasible Z_1. While firms are required to disclose the compensation of top executives, this disclosure occurs after the opportunity for executives to earn bonuses or additional options. Further, previous literature finds that incentive-based executive compensation varies over time rather than remaining sticky (Core and Guay (1999)). Thus, the incentive pay and option based compensation that would influence a manager's decision making is not a figure known publicly at the time of decision making. There are some exceptions to this rule; for example, ex ante disclosure of the structure of a longer term contract would provide additional public information on managerial incentives. However, in general I assume that current monetary incentives are not a primary piece of information used by external parties.

For Z_2 in the "caught" equation I use variables observed by investors, regulators, and other parties outside the firm. All of these parties have their own incentives to detect (and announce) cheating when it occurs. Investors who fail to proactively announce misconduct will incur a larger financial loss if the misbehavior is announced by another party. While there may be some desire by investors to keep misconduct quiet to maintain a high stock price, a rational investor would be unlikely to quietly hold stock that he knows is overvalued. Analysts and regulators who miss instances of misconduct will suffer adverse reputational consequences. Based on previous literature (e.g., Dyck et al (2010), Wang (2011)) I use abnormal ROA, abnormal returns, and abnormal leverage as Z_2. As discussed in Wang (2011), there are caveats associated with the use of these variables. However, the temporal aspect of these variables makes them reasonably suitable for use as Z_2: when deciding to engage or not engage in fraud, managers do not know what the realizations of abnormal variables will be. In general, I rely on temporal information asymmetry to satisfy Poirier's exclusion restrictions for Z_1 and Z_2.

I define the abnormal variables as follows. Letting x_{it} denote one of ROA, returns, or leverage, I estimate the following regression:

$$x_{ijt} = \gamma_0 + \gamma_1 x_{it-1} + \gamma_2 \overline{x_{jt}} + \varepsilon_{ijt} \tag{3}$$

where i denotes firm, t denotes year, j denotes industry, and $\overline{x_{jt}}$ denotes the industry-year average of the quantity x. This approach attempts to remove serial correlation and industry effects. The residuals from the regression in Equation (3) above are serve as my abnormal ROA, abnormal returns, and abnormal leverage. Previous work (Jones and Weingram (1996), Dyck et al (2010, 2013)) suggests that abnormal ROA and abnormal returns are negatively associated with the likelihood that firms are caught engaging in fraud (since they are perceived as less likely to need to misbehave or misreport). These papers also find that higher abnormal leverage is associated with firms being caught more often conditional on engaging, due to the perception of higher risk.

The primary variables in both the cheat and caught equations are firm size (measured by log assets), log R&D, and a pair of variables related to subsidies. I use as my subsidy variables a binary indicator for subsidy receipt as well as the dollar magnitude of a subsidy. I do not have a prediction for the effects of firm size. While a larger firm may have more political influence, it is also likely to be subject to more external monitoring (media, analysts, etc.) and as such may not find it optimal to engage in misbehavior. In the caught equation, I include lagged rather than current values of assets and R&D (as well as for most of the other control variables), as these lagged values represent information available to investors and regulators in a given year. I use non-lagged variables, however, in the cheat equation to represent a firm's expectation of its own performance; a firm likely decides to engage in fraud primarily based on its expected performance for the current and upcoming periods.

High levels of R&D can introduce opacity about a firm's financials (Wang (2006)), suggesting that firms with higher levels of R&D may be more willing to engage in fraud. Consistent with this, Wang (2011) uses a bivariate probit approach and finds that R&D intensity is negatively associated with the likelihood of detection but positively associated with the likelihood of engaging. However, the effect of R&D for subsidy recipients specifically is less clear. There is evidence in my data that high-R&D firms tend to receive subsidies more frequently and that the subsidies they receive tend to be higher. As a result, their heightened dependence on these subsidies for research and development purposes may serve as a deterrent from misbehavior – even though the higher level of opacity of the fundamentals of high-R&D firms may make this misbehavior more difficult to detect. To test this, I include in my regression specification an interaction term between abnormal R&D and the subsidy indicator in addition to the main R&D term. I define abnormal R&D using the same approach as in Equation (3).

In addition to these main variables I use a number of control variables in both equations, based on previous literature (Yu (2008), Jones and Weingram (1996)). These include firm leverage, an indicator for whether a firm is in a regulated industry (finance, utilities, healthcare, etc.), an indicator for whether a firm is in an industry in which qui tam[11] lawsuits are possible, and the number of analysts following a firm.

My primary subsidy variable is the sum of the company's previous three years of subsidies received. I use a three-year historical sum in order to take a long-term view of the effect of government spending and political favors. This is based on the approach by Snyder (1992) and Kroszner and Stratmann (2005).

4.4 Instrumental Variables Approach

Many types of subsidies vary systematically with certain firm attributes. For example, the total (estimated) dollar value of a tax break is directly related to the expected profitability

[11]Informally, a qui tam lawsuit is a whistleblower lawsuit. Included industries are defense and healthcare. See `http://dictionary.law.com/default.aspx?selected=1709` for further information.

of the firm being awarded the tax break, which is in turn related to the firm's size. Because I am interested in the increased levels of visibility generated by receipt of a subsidy rather than the pure monetary effect of the subsidy, scaling by size is not an option to get around this issue. Similarly, many subsidies' dedicated purpose is research and development. Firms engaging in higher levels of R&D likely seek subsidies far more frequently than those that do not. As might be expected, both assets and R&D are substantially correlated with subsidies received in my dataset.

Because several of the control variables are correlated with the subsidy variable, I instrument for the log of subsidies as well as the received-subsidy indicator. This approach is not without precedent. Cohen et al (2011), who study federal subsidies, use ascension to various House of Representatives and Senate committees by politicians to instrument for subsidies given to firms based in those politicians' jurisdictions. Because much of my subsidy data is at the state level, however, I cannot use the Cohen et al (2011) instrument.

It would be difficult to create a direct analog of the Cohen et al (2011) instrument. Data on state-level House and Senate committees is murkier, and unlike the U.S. Congress it is more difficult to determine how much say state politicians have in a given bill. I therefore instrument with readily available data on gubernatorial elections. Besley and Case (1993) find that incumbent governors constrained by term limits often substantially raise governmental spending, and Wolfers (2002) finds that voters irrationally attribute much of a state's economic successes and failures to the governor. I therefore use gubernatorial elections rather than local elections to the state House or Senate. Gubernatorial elections have previously been studied in the accounting literature, with results suggesting that election-year behavior is systematically different (Kido et al (2012)).

One concern with this approach is that it may be irrelevant with respect to local subsidies. However, while gubernatorial elections are likely a better instrument for state subsidies than for local subsidies, they likely still have substantial impact on state policy. Ansolabehere and Snyder (2006) provide evidence on the redistribution of state funds to various municipalities following gubernatorial elections. Even though 12% of subsidies worth over $1 million in my dataset are local, many of them are either drawn from state funds (i.e., the state provides discretionary funds to the county/city, which the county/city then chooses to use part of for the subsidy in question) or are subject to oversight by state bodies.[12] As such, a change in the state government could make it more difficult for municipalities to award subsidies if new administrations wish to impose more restrictive conditions on proposed subsidies or redistribute state funds.

In particular, I instrument with dummy variables indicating election years, whether the incumbent governor reruns for election[13], and whether the incumbent governor loses. I also instrument for what can be thought of as a measure of perceived uncertainty for a given election: a dummy variable indicating whether the margin of victory in the gubernatorial race was less than four percent. The latter figure comes from Politico. Politico tends to designate swing states as those with a less than roughly four percent differential in pre-election polls[14], while the New York Times uses a roughly five percent differential. I use

[12]For example, in exchange for a $2 billion dollar tax break from Sandoval County, New Mexico, Intel agreed to a variety of conditions to be monitored by state authorities. One of these stipulated that more than 100 layoffs in Sandoval County triggered mandatory reporting requirements to the state's Department of Workforce Solutions; see `http://www.koat.com/news/intel-employees-laid-off-in-rio-rancho-sources-say/33671810` for further details

[13]Several states have term limits for governors. I do not differentiate those governors facing term limits from those governors who voluntarily choose not to run for reelection, for example due to other political pursuits (e.g., becoming a U.S. Senator) or private sector pursuits.

[14]See, e.g., `http://www.politico.com/2012-election/swing-state/`

the more conservative of the two. This latter variable captures what are likely to be tight races and allows me to study whether an incumbent's behavior changes in the year before running for reelection when he may perceive a tough challenge. I use media definitions of swing states in order to capture public perception surrounding around an election. While many of my subsidies are at the local (i.e., city or county) level, the largest subsidies in my dataset are almost all granted by state governments and are much larger than the federal subsidies I observe in the data. Further, the supply function of available state subsidies is driven in part by the political desires or ideology of state officials (for example, Keynesians vs. non-Keynesians in office, with markedly different views on government spending). Thus, changes in state politics are likely to have more substantial effects on subsidies and firm behavior than changes in federal or municipal politics. I expect that during election years, subsidies will become less generous when the race is not expected to be close. This is because the marginal political gains, assuming reelection to be the ultimate motive, of becoming more generous are small; while potential fallout increases in the amount of subsidies granted. By contrast, when the race is a tight one, the marginal political gains to generous subsidies are likely to be much higher. I therefore expect that the coefficient on the election-year dummy will be negative while the coefficient on the tight race dummy will be positive; and that the sum of these two coefficients will be positive (i.e., the overall effect is negative for non-tight races and positive for tight races).

I also construct instruments based on competitors' subsidies received.[15] Specifically, when instrumenting for dollar amount of subsidies received for a given firm-year, I use the log of the sum of all other subsidies received by other firms within the same industry-time groupings as well as the log of the sum of all other subsidies received by other firms head-

[15] For a discussion of these types of instruments, see Larcker and Rusticus (2010) as well as the structural industrial organization literature (e.g., Nevo (2000))

quartered within the same state during the same time period. This latter variable accounts for the fact that, as mentioned above, the majority of subsidy dollars in my dataset come from the state and local levels, with almost all of the largest subsidies being state-funded tax breaks. As such, the state level is a natural marketplace in which firms headquartered within a state compete for available tax dollars. I construct similar instruments for the received-subsidy dummy variable, but instead of using the total dollar amount of subsidies received within the past three years, I use the total number of subsidies received by other firms within the same state-year and industry-year as instruments. I expect that the coefficients on the state competitor variables will be positive in the first-stage regression, since greater availability of subsidy dollars on the whole should translate to a higher likelihood of receiving a subsidy as well as a higher dollar amount received.

In addition, most of my specifications involve interaction terms between the subsidy indicator and at least one other variable (abnormal R&D, incentive pay, book-tax differences, or a financial crisis indicator). Let $\widehat{subs}$ denote the predicted value of the subsidy dummy after carrying out the first stage of the IV approach and let x be the variable to be interacted with the subsidy indicator. As described in Wooldridge (2005), I instrument for the product $\widehat{subs \cdot x}$ rather than instrumenting for $\widehat{subs}$ then multiplying by x. I do so by multiplying all variables in the first-stage prediction for $\widehat{subs}$ by x and use these as regressors. That is, if my first-stage regression is

$$subs = \sum_i z_i \cdot \beta_i + \varepsilon$$

then I estimate

$$subs \cdot x = \sum_i (z_i \cdot x) \cdot \delta_i + \mu$$

as my first-stage equation to instrument for the interaction term.

5 Data

I use data from a variety of sources. My data falls into three categories: (1) subsidy data, (2) fraud data, and (3) financial data and other variables used as controls or instruments.

5.1 Subsidy Data

I obtain subsidy and tax break data from the nonprofit corporate watchdog Good Jobs First (GJF), which collects detailed data on national, state, and local-level economic development subsidies. Their database consists of 441,061 total subsidies between 1983 and 2014 (with more per year in more recent years). Each observation in the dataset provides the recipient name, name of the awarding regulatory body and the specific subsidy program, year of award, dollar value of the subsidy, and type of subsidy (grant, low-interest loan, tax credit, enterprise zone, etc.), as well as several other attributes (location, funding agency, etc.). Importantly, GJF provides a source for the vast majority of data entries, so any potential outliers can be checked for data errors.

Considering only subsidies that are matched to publicly traded parent companies, 53.6% (57.1%) of subsidies are tax breaks by total number of subsidies (by dollar value). The latter figure is 62.9% in my final dataset (Appendix B provides additional details on subsidy classifications).

GJF does not provide any firm identifier beyond company name (and parent company name), but the presence of a firm's parent company name in the database is a rough proxy for the firm being publicly traded. By hand-matching GJF data to Compustat data, I obtain a total of 46,921 individual subsidy observations for 1,893 firms between 2004 and 2011. I aggregate this to the firm-year level, using the sum of all subsidies received.

GJF only identifies a subsidy recipient's parent company when that parent company is (roughly) in the largest 2000 US publicly traded firms. Specifically, the largest 900 firms are all in my dataset, but the 993 remaining identified parent companies do not map 1-to-1 to the next 993 largest firms. I am therefore working with truncated data in that there are many subsidies in the dataset pertaining to companies whose parent companies are publicly traded but not labeled in the dataset. As such, as well as for reasons pertaining to the lawsuit data described later, I limit my sample to firms with assets greater than $750 million. This matches the cutoff used in Dyck et al (2013). I verified the largest unmatched subsidies by hand and determined that no unaccounted-for subsidy above $25 million was given to a publicly traded company. It is therefore appropriate to use a Tobit-like approach of a dummy plus an interaction term. My "subsidy amount" variable can actually be thought of as an interaction term between a received-subsidy dummy and the subsidy amount. This also means that my instrumental variables approach should be thought of as instrumenting for the subsidy dummy and for the interaction term.

After a series of data cuts based on data availability and firm size, I have 1,213 distinct firms in my final sample. Of these 1,213 firms, 509 received a subsidy at least once between 2004 and 2011, while the remaining 704 firms do not have any subsidy observations in the dataset. Given the nature of the subsidies in my dataset, it is likely that any estimated effect I find likely understates the true magnitude of a subsidy effect. My dataset contains data on subsidies given to a particular firm but does not contain, for example, tax breaks that benefit an entire industry as a result of lobbying. Subsidy summary statistics at the firm-year level are presented in Figure 1.

5.2 Fraud Data

I use data from two sources to construct the corporate fraud variable, combining shareholder lawsuit data with SEC enforcement action data as in Wang et al (2010) and Wang (2013). This approach is largely additive rather than duplicative. In the full dataset there are 435 enforcement actions and 701 nontrivial lawsuits, with only 61 (or $\frac{61}{779+435-61} = 4.5\%$) overlap, while in the final regression sample there are 115 enforcement actions and 242 nontrivial lawsuits, with only 14 (or $\frac{14}{115+242-14} = 4.1\%$) overlap. While other indicators of fraud exist, combining multiple sources of accounting and financial fraud alleviates some of the issues described in Karpoff et al (2013) on the scope limitations of any one source of data on financial misconduct. I use two of Karpoff et al's four sources, while a third (the Government Accountability Office database) does not cover my sample period. I also do not use Karpoff's fourth source, AuditAnalytics, because a database of restatements is likely to overestimate the instances of meaningful fraud (Dyck et al (2013)). A similar issue exists for the SSCAC lawsuit data if I only considered whether or not firms were sued (Dechow et al (2011)). However, because I observe the eventual settlement as well, I can separate meaningful lawsuits from unimportant or spurious ones.

5.2.1 Lawsuits

My first fraud variable comes from the Stanford Securities Class Action Clearinghouse (SSCAC) database. SSCAC data contains information on securities lawsuits filed from 1995 onward, including date, status (settled/dismissed/ongoing), and settlement amount when the case's status is "settled". For a detailed discussion on why SSCAC data are a good proxy for fraud and do not overstate the number of potential frauds, see Dyck et al (2010). On average, from the date that a class action lawsuit is brought, its status is updated (dismissed or settled) in 577.1 days, with dismissed cases taking on average 574.1 days and settlements taking on average 579.8 days. As such, most SSCAC data from 2013 onward, and in fact many of the SSCAC lawsuits from 2012 onward, are still ongoing. I therefore limit my sample to end in 2011 to be able to work primarily with cases that have ended.

I construct as part of my dependent variable an indicator for whether a lawsuit is settled with a settlement amount at least equal to $1.5 million. This figure is based on work in the legal literature by Choi (2007) and Choi, Nelson, and Pritchard (2009) among others. Settlement amounts less than $1.5 million are often frequently incurred just to make a nuisance suit go away and pay any lawyers' fees. As such, to limit the possibility of false positives in my data, I consider only those lawsuits where the settlement exceeded this amount.

5.2.2 AAER

I also use SEC Accounting and Auditing Enforcement Release (AAER) data to proxy for fraud. Firms that receive AAERs have been caught by the SEC committing some sort of accounting fraud, and as such these serve as additional data points on corporate misbehavior (for details on the AAER data, see Dechow, Ge, Larson, and Sloan (2011)). Because AAERs are subject to the SEC's budget constraints, they primarily represent the most serious of financial frauds, making them a high-quality proxy for fraud; it is extremely unlikely that an AAER recipient did *not* engage. However, due to their limited scope, I use AAERs in conjunction with the SSCAC data rather than as a standalone proxy for fraud.

5.3 Financial and Other Data

5.3.1 Financial Data

I obtain remaining variables primarily from Compustat and CRSP. In order to proxy for managerial incentives, I follow Dyck, Morse, and Zingales (2010, 2013) and consider two types of variables from Execucomp and Equilar: incentive pay and total value of exercisable unexercised options (both summed over all executives). In order to obtain broader sample coverage, I also compute the same two variables using the Equilar database. When data on a firm is available from both Execucomp and Equilar, I use the Execucomp data values. Because these variables are highly correlated, I do not include both in the same regression. I compute incentive pay differently from Dyck et al (2013). While they define incentive pay as the ratio of restricted stock grants to total compensation, data unavailability cuts my final regression sample almost in half when I include incentive pay calculated this way. I therefore define incentive pay as total compensation minus base salary, and the proportion of incentive pay as $\frac{\text{Total Compensation} - \text{Base Salary}}{\text{Total Compensation}}$. My total compensation variable is TDC1 in Compustat, which in addition to salary, bonus, and stock grants, includes the value of options granted rather than the value of options exercised. The inclusion of options granted rather than options exercised (TDC2 in Compustat) allows me to proxy for both a manager's short-term incentives (via the unexercised exercisable options variable) and longer-term incentives (since TDC1 includes options that will be exercised at a future date). I primarily use the log of total incentive pay rather than incentive pay as a fraction of total compensation. The median CEO obtains 74.1 percent of his total compensation from incentive pay, and the 25th and 75th percentile obtain 59.4 and 83.0 percent, respectively. As such the magnitude of incentive pay is almost always substantial. However, when I rerun my regression specifications with incentive pay percentage in place of log incentive pay, the results are virtually unchanged.

Using compensation data from Execucomp and Equilar limits my sample to larger firms, as Execucomp provides coverage only for the S&P 1500 while Equilar provides data for Russell 3000 firms. Because of the nature of the SSCAC lawsuit data, this is not overly limiting. As argued in Dyck, Morse, and Zingales (2010), the incentive structure for class action law firms leads them to take on primarily larger clients, meaning that the estimated probability that a small firm has engaged in wrongdoing will be understated based on class action law firms' selection bias; I therefore limit my final sample to larger firms in order to attenuate this bias.

The remainder of the variables in my cheat and caught equations are various financial proxies. Returns and CAPM residuals are computed using CRSP monthly returns data; ROA, assets, sales, R&D, and leverage are computed using data from Compustat. I proxy for analyst attention via the number of distinct analysts making forecasts in each firm-year, taken from I/B/E/S. In the appendix I provide a table of data cuts, outlining how the Compustat universe from 2004 to 2011 is ultimately reduced to the various final sample sizes.

5.3.2 Political Data

In order to construct the first-stage equation in my instrumental variables approach, I collect data on state gubernatorial elections from the CQ Voting and Elections Collection. Between 2003 and 2012, there were 114 such elections. The majority of these elections occur on the same four-year cycle, as the years 2006 and 2010 alone account for 71 of the 114 elections. In these 114 elections, there were 5 instances where an incumbent governor ran and lost and 59 instances where an incumbent chose not to run, meaning that in total

I observe 64 gubernatorial changes and 50 cases of incumbents keeping power. Of the 114 elections, I also create a dummy variable for particularly close elections. Based on definitions of swing states by Politico and the New York Times, I call an election a "tight race" if it is decided by a margin of 4 percentage points or less. This cutoff yields 17 tight races out of the 114 total elections. Of these 17 tight races, 3 involved the incumbent losing; 11 involved the incumbent not running for reelection; and the remaining 3 involved the incumbent winning by a narrow margin. That is, in the 55 cases where the incumbent chose to run for reelection, (s)he lost by a substantial margin 2 times, lost by a narrow margin 3 times, won by a narrow margin 3 times, and won by a substantial margin the remaining 47 times. As such, I do not have enough variation to include both a close-race dummy and an incumbent-lost dummy; because my aim is to capture uncertainty and the effects that uncertainty may have on abnormal subsidies, I use a close-race dummy variable as one of my instruments.

I match election data to the rest of the dataset using the lagged year, e.g., I match variables pertaining to 2010 election results to a company's 2009 financials and 2009 subsidies. Doing this allows me to model the potential effect of politicians behaving differently in the years leading up to potential reelection, whether to build reputational capital or to make a play for campaign contributions or high-profile endorsements. My data corresponds to a timeframe before the *Citizens United* US Supreme Court decision, however, meaning that direct political contributions by the companies at hand are limited.

6 Main Results

6.1 Descriptive Statistics

Because I obtain data from several different sources and drop firms with under \$750 million in assets, the intersection of the set of available data from all of these sources limits me to 5,243 data points between 2004 and 2011. Figure 2 presents summary statistics on the key variables in my subsidy dataset for the final regression sample, at the firm-year level between 2004 and 2011. Assets and R&D data are in millions of dollars, while incentive pay and the value of unexercised exercisable options are in thousands of dollars.

6.2 First Stage IV

As noted earlier, I first instrument for the log-subsidy variable. I use the previous three years of subsidy data, i.e., if a firm receives subsidy s_t in year t I consider $\log\left(\sum_{k=0}^{3} s_{t-k}\right)$. I instrument for this using log assets, log R&D, leverage, two variables based on competitors' subsidies, and three political variables. The competitor variables are the log of all subsidies received by other firms either in the same industry or in the same state in the past three years. The three columns in Table 3 correspond to all subsidies received, the subset of tax breaks, and the subset of grants, respectively.

Unsurprisingly, the first-stage regressions suggest that assets and R&D are significant predictors of both the receipt of a subsidy as well as the magnitude of a subsidy if received. The fact that larger firms are more likely to receive any subsidy even when controlling for R&D suggests either some level of political influence or a belief that subsidizing a larger firm may lead to more economic development. These variables are similar across the three subsidy sets.

The estimated coefficient on the election-year indicator variable is negative and significant for all subsidy sets in all types of regressions. Put another way, in the year before an election, the likelihood of receiving a subsidy and the expected magnitude of subsidies received both decrease. There are political risks to providing subsidies that a political opponent can take advantage of as part of a campaign; as such, we might expect that politicians are less likely to provide subsidies in the year before an election unless there is a particularly compelling reason (political or otherwise) to do so. This reasoning is suggested by the strong positive and significant indicator on the tight race indicator variable. Again, across all subsets of subsidies, I find that subsidy receipt is more likely in the year before a tight race. The combined effect of the coefficients on the election-year and tight-race dummies, since by construction the tight-race indicator is not switched on if there is no election to speak of, is statistically significantly positive.

Other first-stage regressors vary across the subsets of subsidies. As discussed previously, many states' governors are constrained by term limits. Recall that in 59 of 114 elections, the incumbent did not run for reelection. When controlling for these elections, tax breaks received in the years before these elections are on average significantly higher (but not grants or subsidies as a whole). The reason for this is unclear; it could represent a revolving door, or it could represent promises made by existing lower-level politicians who hope to become governor.

When considering the set of all subsidies or the set of tax breaks, I also find that the industry-level competitor subsidies are a significant predictor of receiving a subsidy. This is not the case when considering only grants, however. This result may be due to the nature of tax breaks (cutting revenue) versus grants (increasing spending) from a political standpoint. The competitor variable in this case is the number of subsidies rather than the dollar amount received. As such, it may be the case that when a certain type of tax break becomes "industry standard", state or local governments may give a similar tax break to a company in their own state for competitive reasons, even if the actual magnitude of the tax break is not particularly large.

6.3 Bivariate Probit Estimations

I turn now to the partially observed bivariate probit estimation in Table 4. For tax breaks and the whole sample, the subsidy indicator is a significantly positive predictor of the probability that firms choose to engage in financial misconduct (i.e., firms that receive subsidies are more willing to engage), and a significantly negative predictor of the conditional probability that they will be caught when doing so. However, the subsidy indicator is insignificant in the cheat equation for grants. This supports the idea that tax breaks and grants are distinct classes of subsidies that lead to different behavior by firms. My results also suggest that subsidies are associated with a higher degree of regulatory capture, as the coefficient on the subsidy indicator is negative and significant for all three choices of subsidy variable. Computing marginal effects, I find that the average firm is 2.3% (2.0%) more likely to cheat when considering tax breaks (all subsidies) and 11.9% less likely to be caught both when considering tax breaks and all subsidies. These findings suggest that regulatory capture exists to some extent across different types of subsidies, but that the effect may be more powerful for tax breaks.

The coefficient on the log subsidy amount is significant and negative in the cheat equation for all subsidies and in the case of tax breaks. This may suggest that above a certain level third-party watchdogs have a nontrivial effect, supported by Dyck et al (2013). It could also suggest that regulatory capture has its limits in the sense that politicians are willing to look the other way when cheating occurs after a smallish subsidy in order to maintain support and donations; but the political cost after a large tax break may be too high. The notion of a threshold is also supported in alternative specifications (untabulated) in which I add an interaction term between the log-subsidy amount and an indicator variable that switches on when the log-subsidy amount is within the top 10, 15, or 25 percent of nonzero subsidy amounts received; results are essentially the same whether I use 10, 15, or 25. In all three cases, all coefficients maintain their signs and significance at the 5% level and the coefficient on the interaction term for tax breaks and all subsidies is negative, significant at the 5% level, and larger in magnitude than the coefficient on the log-subsidy amount. Similar to the main specifications, this interaction term is also insignificant for the grants-only estimation. Obtaining significant results only for tax breaks and for the whole sample suggests that the effect of grants and other non-tax subsidies is not the same as the effect of tax breaks, which may in part be due to the issue of perception. Grants in my sample are sometimes the result of a competitive process[16], while tax breaks are almost never the result of such a process. Thus tax breaks might be considered more notable subsidies by investors and the media, for reasons good and bad.

In all cases, the coefficient on R&D is positive and significant in the caught equation and negative in the cheat equation, although only significant in the case of grants. As R&D is sometimes used as a measure of the opacity of a firm's fundamentals (Wang (2006)), this suggests that firms with more ability to hide manipulation may be the target of in-

[16]This is frequently the case for, e.g., healthcare-related subsidies from the National Institutes of Health; any company can apply, even though only one company will receive the grant

creased scrutiny. There is also a negative and significant coefficient on the interaction term between abnormal R&D and subsidies in both the cheat and detection equations. This is consistent with the notion of increased scrutiny for high-R&D firms, suggested by the positive significant coefficient in the caught equation. Given that high-R&D firms know they will be more carefully scrutinized, those high R&D firms receiving higher subsidies may find it beneficial to not engage in fraud due to the increased risk of being caught. This in turn may lead to regulators focusing some attention away from these firms, explaining the negative coefficient on the interaction term in the detection equation as well.

I turn now to the variables hypothesized to affect only the decision to cheat. Recall that unexercised exercisable options capture short-term gain while the incentive pay variable captures a mix of short-term incentives (e.g., bonuses) and long-term incentives (stock grants or options with maturity date years in the future). The coefficient on option-based compensation is positive and significant in the full sample and in the case of tax breaks, while it is insignificant when considering only grants. Conversely, the coefficients on incentive pay are insignificant regardless of the choice of subsidy variable. The option variable represents unexercised but currently exercisable options, and as such it is unsurprising that executives might be more willing to cheat if they believe doing so will bolster near-term compensation. However, incentive pay captures both short-term and long-term incentives. Negotiating for tax breaks may be thought of as a form of tax avoidance; given the long-term risks of tax avoidance detailed in Hanlon and Slemrod (2009), it may be the case that executives do not wish to risk depressing future stock price which would in turn depress the value of future options and stock grants.

Coefficients on the other control variables are consistent with previous literature. Firms in regulated industries are caught engaging in fraud less frequently, while firms in qui tam industries are conditionally caught more frequently; neither of these attributes, however, is significantly related to firms' decisions to engage in fraud. Firms exhibiting better performance are less likely to engage in fraud, but are more likely to be caught engaging in fraud when they do. This may be because these "better performance" numbers are the result of fraudulent accounting; Dechow et al (2011) find that firms that receive AAERs demonstrated strong financial performance prior to manipulation.

Analyst coverage, measured by the number of analysts making estimates about a firm in a given year, is not significantly related to a firm's decision to engage in fraud, but higher analyst coverage does lead to firms being caught more frequently conditional on engaging. This is consistent with Dyck et al (2010), who note the importance of analysts in fraud detection; in their sample of 216 significant frauds, analysts caught 17% of 142 externally-detected frauds and 11% when considering both internally and externally detected frauds.

7 Additional Tests

I now consider alternate specifications of the tests in the previous section. Given the difference in the results surrounding managerial compensation when firms receive tax breaks versus grants, I test whether there is an interaction effect between subsidies received and managerial incentives in the cheat equation. Given the demonstrated differences between tax breaks and non-tax-breaks, I then also consider a tax-based potential indicator of riskiness in the detection equation. Finally, because the 2008-09 financial crisis occurs during my sample period, I test whether the effect of subsidies is different prior to the crisis by both including a crisis indicator and rerunning the same specifications from the previous section while excluding the crisis years from my data.

7.1 Managerial Compensation

I now consider specifications that include an interaction term between the subsidy indicator and managerial compensation. I hypothesize that the coefficient on such a term will be significant and positive. Such a result would indicate that a subsidized firm tends to take advantage of its political pull more when the firm's executives have more at stake financially. Because the managerial compensation variable affects only the cheat equation, I include the interaction term only in the cheat equation; the detection ("caught") equation remains unchanged.

I first run specifications that include an interaction term between the subsidy indicator and log option compensation; however, the coefficient on this variable is statistically insignificant for all three choices of subsidy variable, and the coefficients on all other variables are largely the same. I therefore do not tabulate these results. I then run specifications that include an interaction term between the subsidy indicator and incentive pay and obtain significant results. These results are presented in Table 5. The subsidy dummy variable is no longer significant for the set of all subsidies and for the set of tax breaks, while it is now significant and negative in the case of grants. As predicted, the interaction term between the subsidy dummy and incentive pay is positive and significant for all choices of subsidy variable. This does not contradict the results in the previous section, as the total marginal effect of receiving a subsidy on the probability of cheating, evaluated at the means of the variables in question, is significant and positive for the set of all subsidies and for tax breaks but insignificant for grants. The main effect for incentive pay is now also significant and negative. This suggests that in the absence of subsidies, managers with higher incentive pay considerations may take fewer risks, while executives of firms who receive subsidies may feel more strongly that their misbehavior will not be detected, in the process diluting future compensation. This is supported by the significant negative effect of receiving a subsidy on the probability of detection for all three choices of subsidy variable.

7.2 Tax-Based Red Flags

Given the association between tax breaks and both the likelihood of engaging in fraud and of being caught conditional on engaging, in this section I consider other tax-based variables. Hanlon et al (2012) suggest that a firm's book-tax difference is related to firms' propensity to engage in earnings management. Given this, I consider specifications that include the firm's book-tax difference. The correlation coefficient between the book-tax difference and firms' tax breaks received is 0.036 – and 0.025 when only considering observations with nonzero tax break amounts at some point within the previous three years. As such it is unlikely that introducing book-tax differences leads to multicollinearity issues.

Much of the literature on book-tax differences discusses its use (or lack thereof) to investors and regulators, and so I include the book-tax difference in the detection equation only. Results from the tax break specifications are presented in Table 6; for brevity I do not tabulate the all-subsidies or grants-only specifications. The book-tax term is statistically insignificant when using only grants, and results for the case of all subsidies are similar to results when using tax breaks only. I find that the book-tax difference is significant and positively related to the probability of detecting fraud conditional on it having occurred. This is in line with previous research on the book-tax difference (Hanlon et al (2012), Desai and Dharmapala (2009)), although neither of these two authors comments on corporate fraud specifically. However, the inclusion of the book-tax difference does not alter any of my main results in either the cheat or caught equation. Importantly, receiving a subsidy is still a significant positive predictor of the propensity to engage in fraud and a negative predictor of the conditional probability of detection. Including an interaction term between the subsidy indicator and the book-tax difference to test whether the book-tax difference matters more for subsidized firms does not alter the main takeaways from Table 6; the interaction term comes in insignificant, and the main effects of the book-tax difference and subsidy indicator remain significant and positive.

7.3 Financial Crisis

My sample covers the years 2004 to 2011. However, it is reasonable to expect that the effects of subsidies may be different during a crisis when compared to subsidy effects during more "normal" economic cycles. To test this idea, I run two additional specifications to study the effects the crisis might have had on my results. I use the NBER definition of the dates of the financial crisis[17] , i.e., as having occurred between December 2007 and June 2009. First, I attempt to control for the financial crisis with an indicator variable that switches on for the years 2008 and 2009. I then re-run the main analyses in Tables 4 excluding the years 2008 and 2009 (e.g., using a discontinuous sample consisting of data from the years 2004-2007, 2010, and 2011). In both cases my results remain substantially unchanged, and so I do not tabulate results.

8 Conclusion and Extensions

Using a large dataset of state, local, and federal corporate subsidies, I estimate the impact of these subsidies on firms' decisions to engage in fraud as well as the conditional likelihood that cheating firms will be caught. I find that the receipt of a government subsidy leads firms to engage in fraud more frequently and to be caught less often. Drawing upon academic literature and popular press, I partition my subsidy dataset into tax breaks (revenue

[17]`http://www.nber.org/cycles.html`

cuts) and grants (increased spending). I find that the magnitude of the subsidy effect as well as the effects of the dollar values of subsidies differ significantly. Large grants or discounts are associated with firms engaging in fraudulent activity more frequently, while firms that receive large tax breaks engage less frequently than firms that receive smaller tax breaks. These results suggest that regulatory agencies or external watchdogs may find it prudent to more closely monitor firms in the years after they receive a significant favor from state, local, or federal governments.

My study's main contribution is to document the relation between the politician-to-firm monetary flow and corporate fraud. However, some caveats apply. While federal lobbying data is rich and widely available, data at the state level is much less so.[18] Because the majority of my subsidies are granted by state politicians, this means I cannot identify the effect of lobbying on the likelihood of receiving a subsidy or its magnitude. Further, for data availability reasons, I have not yet been able to include proxies for media coverage (Dyck et al (2013)) in my analyses. Other possible extensions of this paper could also study the effects of non-monetary politician-to-firm flows such as politician-sponsored industry regulations. This could include variables such as fines paid to regulatory agencies. While this would shift the focus away from financial fraud and investor lawsuits, it could allow for more general insight on how the politician-to-firm monetary flow affects' firm behavior.

[18] http://sunlightfoundation.com/blog/2015/08/12/how-transparent-is-your-states-lobbying-disclosure/

References

[1] Alesina, Alberto, and Silvia Ardagna, 2010. *Large Changes in Fiscal Policy: Taxes vs. Spending.* Tax Policy and the Economy 24, 35-68.

[2] Armstrong, Christopher, Alan D. Jagolinzer, and David F. Larcker, 2010. *Chief Executive Officer Equity Incentives and Accounting Irregularities.* Journal of Accounting Research 48(2), 225-271.

[3] Atlas, Cary, Thomas Gilligan, Robert Hendershott, and Mark Zupan, 1995. *Slicing the Federal Government Net Spending Pie: Who Wins, Who Loses, and Why.* American Economic Review 85, 624-629.

[4] Bergstresser, Daniel and Thomas Philippon, 2006. *CEO Incentives and Earnings Management.* Journal of Financial Economics 80, 511-529.

[5] Burns, Natasha, and Simi Kedia, 2006. *The Impact of Performance-Based Compensation on Misreporting.* Journal of Financial Economics 79, 3567.

[6] Choi, Stephen J., Karen K. Nelson, and A.C. Pritchard, 2009. *The Screening Effect of the Private Securities Litigation Reform Act.* Journal of Empirical Legal Studies 6(1), 35-68.

[7] Choi, Stephen J., 2007. *Do the Merits Matter Less After the Private Securities Litigation Reform Act?* Journal of Law, Economics, and Organization 23(3), 598-626.

[8] Cohen, Lauren, Joshua Coval, and Christopher Malloy, 2011. *Do Powerful Politicians Cause Corporate Downsizing?* Journal of Political Economy 119(6), 1015-1060.

[9] Coffee, John C., and Hillary A. Sale, 2009. *Redesigning the SEC: Does the Treasury Have a Better Idea?* Virginia Law Review 95(4), 707783.

[10] Core, John and Wayne Guay, 1999. *The Use of Equity Grants to Manage Optimal Equity Incentive Levels.* Journal of Accounting and Economics 28(2), 151-184.

[11] Correia, Maria, 2014. *Political Connections and SEC Enforcement.* Journal of Accounting and Economics 57, 241-262.

[12] Dechow, Patricia, Weili Ge, Chad Larson, and Richard Sloan, 2011. *Predicting Material Accounting Misstatements* Contemporary Accounting Research 28(1), 17-82.

[13] Dechow, Patricia, Amy Hutton, Jung Hoon Kim, and Richard Sloan, 2012. *Detecting Earnings Management: A New Approach.* Journal of Accounting Research 50(2), 275-334.

[14] Dechow, Patricia, Richard Sloan, and Amy Sweeney, 1995. *Detecting Earnings Management*. The Accounting Review 70(2), 193-225.

[15] deHaan, Ed, Simi Kedia, Kevin Koh, and Shivaram Rajgopal, 2014. *Does the Revolving Door Affect the SEC's Enforcement Outcomes?* Rock Center for Corporate Governance Working Paper No. 187. Available at http://ssrn.com/abstract=2125560.

[16] Dal Bo, Ernesto, 2006. *Regulatory Capture: A Review*. Oxford Review of Economic Policy 22(2), 203-225.

[17] Desai, Mihir, and Dhammika Dharmapala. *Corporate Tax Avoidance and Firm Value*. Review of Economics and Statistics 91, 537-546.

[18] DeFond, Mark, Jere Francis, and Nicholas Hallman. *Auditor Reporting Decisions and Proximity to SEC Regional Offices*. Working paper, available at http://wp.unil.ch/earnet2015/files/2014/10/Auditor-Reporting-Decisions-and-Proximity-to-SEC-Regional-Offices.pdf

[19] Dyck, Alexander, Adair Morse, and Luigi Zingales, 2010. *Who Blows the Whistle on Corporate Fraud?* Journal of Finance 65(6), 2213-2253.

[20] Dyck, Alexander, Adair Morse, and Luigi Zingales, 2013. *How Pervasive is Corporate Fraud?* Working paper, available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2222608

[21] Efendi, Jap, Anup Srivastava, and Edward P. Swanson, 2007. *Why Do Corporate Managers Misstate Financial Statements? The Role of Option Compensation and Other Factors*. Journal of Financial Economics 85, 667-708.

[22] Faccio, Mara, Ronald W. Masulis, and John J. McConnell, 2006. *Political Connections and Corporate Bailouts*. Journal of Finance 51(6), 2597-2635.

[23] Galvin, William, 2008. *States' Demonstrated Record of Effectiveness in Their Investor Protection Efforts Underscores the need to Avoid Further Preemption of State Enforcement Authority*. White paper, available at http://www.sec.state.ma.us/sct/sctwhitepaper/Secretary_Galvin_Enforcement_White_Paper.pdf

[24] Good Jobs First Foundation, 2014. *Subsidy Tracker User Guide*. Available online at http://www.goodjobsfirst.org/subsidy-tracker-user-guide.

[25] Hanlon, Michelle, Gopal V. Krishnan, and Lillian F. Mills, 2012. *Audit Fees and Book-Tax Differences*. Journal of the American Taxation Association 34(1), 55-86.

[26] Hanlon, Michelle, and Joel Slemrod, 2009. *What Does Tax Aggressiveness Signal? Evidence from Stock Price Reactions to News About Tax Shelter Involvement*. Journal of Public Economics 93, 126-141.

[27] Ivkovic, Zoran, and Scott Weisbenner, 2005. *Local Does as Local Is: Information Content of the Geography of Individual Investors Common Stock Investments.* Journal of Finance 50 (1), 267-306.

[28] Jones, Christopher L., and Seth E. Weingram, 1996. *The Determinants of 10b-5 Litigation Risk.* Stanford Law School working paper, available at `http://ssrn.com/abstract=2684`.

[29] Karpoff, Jonathan M., Allison Koester, D. Scott Lee, and Gerald S. Martin, 2014. *Database Challenges in Financial Misconduct Research.* Georgetown McDonough School of Business Research paper no. 2012-15.

[30] Kedia, Simi, and Shivaram Rajgopal, 2011. *Do the SEC Enforcement Preferences Affect Corporate Misconduct?* Journal of Accounting and Economics 51(3), 259-278.

[31] Kido, Nolan, Reining Petacchi, and Joseph Weber, 2012. *The Influence of Elections on the Accounting Choices of Governmental Entities.* Journal of Accounting Research 50(2), 443-476.

[32] Krozner, Randall and Thomas Stratmann, 2005. *Corporate Campaign Contributions, Repeat Giving, and the Rewards to Legislator Reputation.* Journal of Law and Economics 48(1), 41-71.

[33] Larcker, David F., and Tjomme O. Rusticus, 2010. *On the Use of Instrumental Variables in Accounting Research.* Journal of Accounting and Economics 49(3), 186-205.

[34] Leal, David L., 2006. *Electing America's Governors: The Politics of Executive Elections.* Palgrave Macmillan.

[35] Malloy, Christopher J., 2005. *The Geography of Equity Analysis.* Journal of Finance 60 (2), 719-755.

[36] Miller, Gregory S., 2006. *The Press as a Watchdog for Accounting Fraud.* Journal of Accounting Research 44(5), 1001-1033.

[37] Nevo, Aviv, 2000. *A Practitioner's Guide to Estimating Random-Coefficient Logit Models of Demand.* Journal of Economics and Management Strategy 9(4), 513-548.

[38] Ramanna, Karthik and Sugata Roychowdhury, 2010. *Elections and Discretionary Accruals: Evidence from 2004.* Journal of Accounting Research 48(2),445-475.

[39] Shleifer, Andrei and Robert W. Vishny, 1994. *Politicians and Firms.* Quarterly Journal of Economics 109(4), 995-1025.

[40] Spiller, Pablo T., 1990. *Politicians, Interest Groups, and Regulators: A Multiple-Principals Agency Theory of Regulation, or "Let Them Be Bribed"*. Journal of Law and Economics 33 (1), 65-101.

[41] Tahoun, Ahmed, 2014. *The Role of Stock Ownership by US Members of Congress on the Market for Political Favors*. Journal of Financial Economics 111, 86-110.

[42] Tahoun, Ahmed, and Laurence van Lent, 2013. *The Personal Wealth Interests of Politicians and Government Intervention in the Economy*. Working paper, available at `http://ssrn.com/abstract=1570219`.

[43] Wang, Tracy Y., 2006. *Securities Fraud: An Economic Analysis*. Working Paper, University of Minnesota.

[44] Wang, Tracy Y., 2013. *Corporate Securities Fraud: Insights From a New Empirical Framework*. Journal of Law, Economics and Organization 29, 535-568.

[45] Wang, Tracy Y., Andrew Winton, and Xiaoyun Yu, 2010. *Corporate Fraud and Business Conditions: Evidence from IPOs*. Journal of Finance 65(6), 2255-2292.

[46] Yu, Frank, 2008. *Analyst Coverage and Earnings Management*. Journal of Financial Economics 88(2), 245-271.

[47] Yu,Frank and Xiaoyun Yu, 2011. *Corporate Lobbying and Fraud Detection*. Journal of Financial and Quantitative Analysis 46, 1865-1891.

[48] Yu, Xiaoyun, 2013. *Securities Fraud and Corporate Finance: Recent Developments*. Managerial and Decision Economics 34, 439-450.

Appendix A Large Subsidies

To provide some examples of the content of my subsidy dataset, I provide below the ten largest subsidies along with a brief description of each, citing the Good Jobs First description as well as web links where necessary.

A.1 Boeing, Washington, 2013: $8.7 billion

These tax breaks were given to Boeing in order for production of the new 777X airplane to occur in the state of Washington. From GJF:

> *The main portion of the package was a 16-year extension of the tax breaks that Boeing had been awarded in 2003. The tax benefits will be available to Boeing's suppliers as well. There was no reported breakdown but it was assumed that most of the benefits would accrue to Boeing itself.*

A news article[19] provides further details about the tax break, namely that

> *The biggest single piece is giving a "preferential rate" on the business-and-occupation tax for aerospace companies that build the 777X and other commercial airplanes.*

Boeing now enjoys a B&O tax rate of 0.9 percent, as compared to 1.5 percent for other service industries. Another substantial piece of the tax breaks includes a sales tax exemption on materials and services related to aircraft construction (originally implemented in 2003, and extended in this bill)[20].

A.2 Alcoa, New York, 2007: $5.6 billion

This is the largest direct subsidy I observe in my dataset. In this case, the New York Power Authority gave the company heavily discounted electricity in exchange for Alcoa investing in a plant and promising an upper bound on the number of jobs it would eliminate from that plant.

From GJF:

> *The state-owned New York Power Authority agreed to provide the company with electricity at about one-quarter of the standard rate, saving it an amount estimated by the Buffalo News at* $185 *million per year, or* $5.6 *billion over the 30-year life of the agreement. Good Jobs First contacted the Power Authority to confirm the estimate, but the agency did not provide a substantive*

[19] http://bbjtoday.com/blog/boeings-tax-break-how-8-7-billion-adds-up/26977
[20] http://budgetandpolicy.org/schmudget/proposed-boeing-tax-breaks-should-include-accountability-measures

response. In exchange for the discount, Alcoa agreed to invest \$600 *million in its Massena facility and not to eliminate more than 165 of its 1,065 workers there. Finalization of the deal was delayed until 2013 while Alcoa obtained approval from the U.S. Environmental Protection Agency of its plan to clean up PCB contamination in a portion of the Grasse River.*

A.3 Boeing, Washington, 2003: \$3.244 billion

This deal, also to Boeing, was in exchange for guarantees that Boeing would develop and assemble the 787 airplane (at the time referred to as the "7E7") in Washington State. This came in the form of a 10-year tax credit that included the following (taken directly from former Governor Gary Locke's webpage[21])

- B and O rate reduction for the aerospace industry;
- B and O tax credit for research and development;
- Sales tax exemption for computer hardware and software used in design and engineering of airplanes and their components;
- Sales tax exemption on any new construction or improvement either in Everett or Moses Lake; and
- Property tax relief on new facilities and equipment for Everett or Moses Lake.

A.4 Sempra Energy, Louisiana, 2013: \$2.195 billion

This subsidy consists of a ten-year property tax exemption in exchange for Sempra building a new \$6 billion export facility in Louisiana. I was unable to find much press coverage about this subsidy; most articles about the new facility did not mention the property tax exemption. Governor Jindal's website only briefly mentions Sempra, stating that the company " is expected to utilize Louisiana's Quality Jobs and Industrial Tax Exemption Program incentives on the project".

A.5 Nike, Oregon, 2012: \$2.021 billion

This roughly \$2 billion dollar tax break was given to Nike in order to assure that it would maintain its home operations (and expand further) in the state of Oregon. From GJF:

[21] `http://www.digitalarchives.wa.gov/governorlocke/press/press-view.asp?pressRelease=1375&newsType=1`

> *In December 2012, the Oregon legislature passed the so-called "Nike bill," which allowed the company to calculate its Oregon taxes based on the single sales factor formula for 30 years. The subsidy estimate is conservative, given that it does not take into account future increases in the company's profits.*

In terms of specifics, Nike agreed[22] to the following terms:

- Nike must invest $150 million in a capital project that will produce 500 jobs. The $150 million must be invested by January 1, 2017, and Nike must provide written notice to the governor when it reaches this threshold.
- Nike will not utilize the state's Strategic Investment Program for an incentive greater than $5 million.
- The state's current tax law, where corporate tax is calculated by the "single-sales factor" will apply to Nike during the 30-year term of the contract.

The exact agreement is also available online[23].

A.6 Intel, New Mexico, 2004: $2 billion

Intel has chip-manufacturing plants in Sandoval County, NM, where this tax break occurred. This specific deal was a $16 billion dollar industrial revenue bond, which was tax-exempt until 2034; the $2 billion figure comes from the estimated taxes that Intel would have paid were it not exempt. From Good Jobs First:

> *As with Intel's 1993 deal in New Mexico, the subsidy was expected to take the form of PILOT tax abatements associated with a* $16 *billion industrial revenue bond issued by Sandoval County.*

A.7 Intel, Oregon, 2014: $2 billion

This was a tax break in exchange for Intel building and maintaining a semiconductor manufacturing facility in Hillsboro, OR. The $2 billion is estimated, and the subsidy itself is in the form of SIP property tax exemptions on tools and equipment.
From GJF:

> *The value of the subsidy package -* $2 *billion over 30 years - was estimated by The Oregonian. The subsidy will come as SIP property tax exemptions on Intel's tools and equipment. The subsidy is investment-based and, according to the Oregonian, "the deal won't create many [direct] jobs."*

[22] http://www.oregonlive.com/politics/index.ssf/2012/12/kitzhaber_signs_30-year_tax_de.html

[23] http://media.oregonlive.com/politics_impact/other/0601_001.pdf

Details of the actual deal, after terms were announced, are available online[24].

A.8 Cheniere, Louisiana, 2010: $1.689 billion

This was a series of tax breaks given by the state of Louisiana to Cheniere Energy to build a natural gas liquification plant. This utilized the state's Industrial Tax Exemption Program, which abates local property taxes on manufacturers' new investments for up to 10 years. The Qualify Jobs program described below "provides a cash rebate to companies that create well-paid jobs"[25].
From GJF:

> *The total value includes:* $126,098,145 *in property tax abatements the company received in 2010 for assets placed in service in 2009; two 2011 awards from the Industrial Tax Exemption program valued at* $1,447,200,000 *and* $530,728*; Quality Jobs cash rebates worth about* $115.5 *million; and an unspecified amount from the FastStart job training program. The job, wage and investment projections refer to the 2011 expansion.*

A.9 Royal Dutch Shell, Pennsylvania, 2010: $1.65 billion

This package was for a Shell chemical processing plant in western Pennsylvania that would aid with fracking. The package consisted of state corporate income tax credits[26] worth $66 million per year, for 25 years; Governor Tom Corbett claimed that the package would add 10000-20000 jobs to the Pennsylvania economy. This deal got a lot of media coverage, as it roughly coincided with Corbett slashing state funding for education and other social services. This deal was an add-on to a previous (few months earlier) deal that created a special economic opportunity zone, already providing property tax credits to Shell.
From GJF:

> *The subsidy consisted of state tax credits worth* $66 *million per year over a 25-year period. As of June 2013 the company had not yet chosen a final location.*

A.10 Cerner, Missouri, 2013: $1.635 billion

This subsidy used tax increment financing in order to subsidize Cerner in its construction of a mixed-use office campus in Kansas City. This campus would be primarily for health-care companies, and 97 percent of the employees were to be office, technical, and medical

[24] http://www.oregonlive.com/silicon-forest/index.ssf/2014/08/intels_new_tax_deal_is_a_whopp.html

[25] http://www.opportunitylouisiana.com/page/cheniere-energy

[26] http://www.nytimes.com/interactive/2012/12/01/us/government-incentives.html#co-royaldutchshell

workers[27]. Cerner promised 16000 new jobs[28] as part of the development.

From GJF:

> *On October 11, 2013 the Kansas City Council approved tax increment financing district for a 4.5 million square foot mixed-use office campus that will be built by Cerner Corp. in 14 phases by 2024. The company will be reimbursed* $1.6 *billion for the construction costs and the reimbursement will come from various types of TIF:* $740,097,851 *from standard TIF,* $288,632,659 *from Super TIF and* $606,421,732 *from State Supplemental TIF.*

[27] http://www.bizjournals.com/kansascity/news/2014/07/09/new-cerner-campus-job-total-increase.html?page=all

[28] http://www.kansascity.com/opinion/editorials/article3725060.html

Appendix B Subsidy Classification

Below I provide a table of GJF's subsidy categories; I classify several of these as tax breaks.

Type of Subsidy	Tax Break?
cash grant	No
cost reimbursement	No
enterprise zone	Yes
federal allocated tax credit	Yes
federal grant	No
federal insurance	No
federal loan or loan guarantee	No
federal tax-exempt bond	Yes
grant/low-cost loan	No
industrial revenue bond	No
infrastructure assistance	No
MEGADEAL	*
property tax abatement	Yes
tax credit/rebate	Yes
tax credit/rebate and grant	Yes
tax credit/rebate; property tax abatement	Yes
tax increment financing	Yes
training reimbursement	No

*GJF classifies 306 of the largest subsidies as "MEGADEALS". Some of these are tax breaks while others are not; I classify these by hand. Because many megadeals have several components, I classify megadeals with greater than 50 percent tax breaks under the tax break category; and the rest under the grant category. For example, consider the following tax break given by New York State to AMD in 2006:

> AMD was originally awarded a $500 million capital grant, a $150 million R&D grant, and Empire Zone tax credits worth $250 million, with the remainder of the package going toward infrastructure improvements. (Larry Rulison, "State's Big Payout, Big Risk," Albany Times-Union, October 9, 2011).

Because only $250 million of the more than $900 million corresponds to tax breaks, I classify this observation as a grant.

Appendix C Tables

Table 1: Summary Statistics for Subsidies ($1000s) – Firms With Assets > $750M

Type of Subsidy (N)	Min	25th %ile	Median	75th %ile	Max	Mean	St. Dev.
All Subsidies (5106)	0	232	1300	6026	5621331	22103	175070
Tax Breaks (3649)	0	202	999	4511	3244000	16924	120754
Grants (3467)	0	136	688	3361	5603547	14739	169617

Table 2: Summary Statistics for Non-Subsidy Variables - Final Regression Dataset

Statistic	Mean	25th %ile	Median	75th %ile	St. Dev.
Assets	7,476	992	2,047	5,173	22,131
R&D	212	0	25	104	782
Leverage	0.832	0.513	0.788	1.070	0.484
Qui tam industry	0.118	-	-	-	-
Regulated industry	0.188	-	-	-	-
ROA	0.058	0.010	0.060	0.116	0.347
Annual returns	0.12	-0.183	0.059	0.308	0.705
Analyst estimates	9.954	4	8	14	7.455
Options	1,192	0.7	6	28	8,825
Incentive Pay	1,036	5	11	25	4,488

Table 3: First-Stage IV Regressions

Coefficient estimates from the first-stage regressions to instrument for the received-subsidy indicator and the dollar value of subsidies. Panel A (B) presents results for the subsidy indicator (dollar value). Standard errors are in parentheses, and both regressions use 6,813 observations. I do not report the constant term or coefficients on assets, or $R\&D$. Coefficients on log assets and log $R\&D$ are positive and significant at the 0.01 level for all six specifications, while the constant is negative and significant at the 0.01 level for all six specifications. One asterisk (*) denotes significance at the 0.1 level, two (**) the 0.05 level, and three (***) the 0.01 level. $N = 5,243$ observations.

Panel A: Subsidy Indicator

	All Subsidies	Tax Breaks	Grants
Year before gubernatorial election	-0.030^{*}	-0.030^{*}	-0.029^{**}
	(0.016)	(0.015)	(0.015)
Log other industry subsidies (#)	-0.008^{*}	-0.003	-0.006
	(0.005)	(0.005)	(0.005)
Log other state subsidies (#)	0.028^{***}	0.033^{***}	0.016^{***}
	(0.006)	(0.006)	(0.006)
Incumbent didn't rerun	0.012	0.027	-0.008
	(0.019)	(0.019)	(0.018)
Tight race	0.106^{***}	0.101^{***}	0.126^{***}
	(0.027)	(0.026)	(0.024)

Panel B: Subsidy Dollar Value

	All Subsidies	Tax Breaks	Grants
Leverage	-0.942^{***}	-0.530^{***}	-0.650^{***}
	(0.150)	(0.132)	(0.139)
Log other industry subsidies ($)	-0.060^{*}	-0.139^{***}	0.043^{*}
	(0.036)	(0.019)	(0.024)
Log other state subsidies ($)	0.037	-0.035^{**}	0.068^{***}
	(0.024)	(0.016)	(0.017)
Year before gubernatorial election	-0.645^{***}	-0.431^{**}	-0.699^{***}
	(0.215)	(0.189)	(0.200)
Incumbent didn't rerun	0.453^{*}	-0.024	0.704^{***}
	(0.260)	(0.230)	(0.242)
Tight race	1.544^{***}	1.603^{***}	1.402^{***}
	(0.359)	(0.316)	(0.334)

Table 4: Main Bivariate Probit Results

Main partially observed bivariate probit specifications. The dependent variable in all cases is an indicator for AAERs and investor lawsuits. Panel A uses the set of all subsidies, Panel B considers only tax breaks, and Panel C considers only grants. In the caught equation, the following variables are lagged: log assets, (abnormal) leverage, (abnormal) ROA, (abnormal) returns, log R&D. Standard errors are in parentheses. One asterisk (*) denotes significance at the 0.1 level, two (**) the 0.05 level, and three (***) the 0.01 level.

Panel A: All Subsidies ($N = 5,243$)

	(Cheat Equation)	(Caught Equation)
Constant	-2.988***	-9.347***
	(0.661)	(2.312)
Received subsidy	4.985***	-11.672***
	(1.846)	(3.136)
Regulated firm	-0.317	-0.834*
	(0.312)	(0.327)
Qui tam industry	0.242	1.677***
	(0.325)	(0.38)
Log assets	0.271**	0.981**
	(0.128)	(0.383)
Log subsidy amount	-0.395***	0.128
	(0.124)	(0.162)
Leverage	-0.881***	1.214***
	(0.258)	(0.321)
ROA	-0.949**	1.692**
	(0.471)	(0.679)
Annual returns	-0.449***	0.778***
	(0.133)	(0.222)
Number of analyst estimates	0.019**	0.028***
	(0.009)	(0.009)
Log R&D	-0.042	0.509***
	(0.041)	(0.123)
Subsidy indicator $\times$ abnormal R&D	-1.083**	-0.142**
	(0.486)	(0.059)
Log options	0.048**	
	(0.021)	
Log incentive pay	-0.042	
	(0.029)	
Abnormal leverage		0.033*
		(0.019)
Abnormal ROA		-1.093***
		(0.372)
Abnormal returns		-2.668***
		(0.535)

Panel B: Tax Breaks ($N = 5,243$)

	(Cheat Equation)	(Caught Equation)
Constant	-2.853***	-10.044***
	(0.544)	(1.663)
Received subsidy	4.325***	-12.661***
	(1.677)	(3.201)
Regulated firm	-0.361	-0.888***
	(0.308)	(0.336)
Qui tam industry	0.174	1.836***
	(0.311)	(0.389)
Log assets	0.241**	1.054***
	(0.095)	(0.254)
Log subsidy amount	-0.372***	0.18
	(0.127)	(0.146)
Leverage	-0.788***	1.158***
	(0.218)	(0.302)
ROA	-1.255***	1.554***
	(0.397)	(0.559)
Annual returns	-0.453***	0.799***
	(0.134)	(0.199)
Number of analyst estimates	0.027***	0.022***
	(0.008)	(0.008)
Log R&D	-0.066**	0.386***
	(0.031)	(0.077)
Subsidy indicator × abnormal R&D	-0.69***	-0.156***
	(0.159)	(0.06)
Log options	0.056***	
	(0.012)	
Log incentive pay	-0.046**	
	(0.018)	
Abnormal leverage		0.042***
		(0.012)
Abnormal ROA		-0.916***
		(0.324)
Abnormal returns		-2.555***
		(0.495)

Panel C: Grants ($N = 5,243$)

	(Cheat Equation)	(Caught Equation)
Constant	-3.07***	-6.07***
	(0.445)	(1.402)
Received subsidy	-0.068	-9.199***
	(1.368)	(2.781)
Regulated firm	-0.112	-1.147***
	(0.305)	(0.366)
Qui tam industry	0.103	1.603***
	(0.313)	(0.403)
Log assets	0.293***	0.246
	(0.084)	(0.191)
Log subsidy amount	-0.09	0.205
	(0.095)	(0.136)
Leverage	-0.798***	1.672***
	(0.216)	(0.418)
ROA	-0.706*	1.039
	(0.381)	(0.639)
Annual returns	-0.484***	1.098***
	(0.143)	(0.283)
Number of analyst estimates	-0.003	0.065***
	(0.009)	(0.012)
Log R&D	0.061	0.239**
	(0.042)	(0.094)
Subsidy indicator × abnormal R&D	-0.473***	-0.282***
	(0.133)	(0.079)
Log options	0.022	
	(0.015)	
Log incentive pay	-0.013	
	(0.021)	
Abnormal leverage		0.036
		(0.023)
Abnormal ROA		-0.283
		(0.303)
Abnormal returns		-3.208***
		(0.646)

Table 5: Bivariate Probit Results with Incentive Pay Interaction

Includes interaction between incentive pay and subsidy indicator. The dependent variable in all cases is an indicator for AAERs and investor lawsuits. Panel A considers all subsidies, Panel B considers only tax breaks, and Panel C considers only grants. In the caught equation, lagged variables are: log assets, (abnormal) leverage, (abnormal) ROA, (abnormal) returns, log R&D. Standard errors are in parentheses. One asterisk (*) denotes significance at the 0.1 level, two (**) the 0.05 level, and three (***) the 0.01 level.

Panel A: All Subsidies ($N = 5,243$)

	(Cheat Equation)	(Caught Equation)
Constant	-3.253***	-9.209***
	(1.061)	(2.844)
Received subsidy	5.288**	-12.925***
	(2.072)	(3.211)
Regulated firm	-0.366	-0.964***
	(0.306)	(0.326)
Qui tam industry	0.323	1.632***
	(0.320)	(0.403)
Log assets	0.39**	0.950**
	(0.167)	(0.480)
Log subsidy amount	-0.467***	0.190
	(0.128)	(0.230)
Leverage	-1.034***	1.505***
	(0.249)	(0.428)
ROA	-1.138***	1.805**
	(0.422)	(0.782)
Annual returns	-0.457***	0.793***
	(0.138)	(0.202)
Number of analyst estimates	0.023***	0.025***
	(0.008)	(0.007)
Log R&D	-0.072	0.550***
	(0.059)	(0.138)
Subsidy indicator × abnormal R&D	-0.614***	-0.156**
	(0.169)	(0.061)
Log options	0.041*	
	(0.022)	
Log incentive pay	-0.069**	
	(0.030)	
Subsidy indicator × log incentive pay	-0.54***	
	(0.207)	
Abnormal leverage		0.054**
		(0.022)
Abnormal ROA		-0.853**
		(0.407)
Abnormal returns		-2.477***
		(0.473)

Panel B: Tax Breaks ($N = 5,243$)

	(Cheat Equation)	(Caught Equation)
Constant	-2.868***	-10.121***
	(0.52)	(1.671)
Received subsidy	4.208**	-12.515***
	(1.696)	(3.431)
Regulated firm	-0.358	-0.884***
	(0.306)	(0.332)
Qui tam industry	0.176	1.81***
	(0.309)	(0.391)
Log assets	0.255**	1.064***
	(0.103)	(0.257)
Log subsidy amount	-0.374***	0.164
	(0.124)	(0.16)
Leverage	-0.79***	1.171***
	(0.218)	(0.282)
ROA	-1.29***	1.608***
	(0.377)	(0.538)
Annual returns	-0.445***	0.788***
	(0.134)	(0.2)
Number of analyst estimates	0.027***	0.022***
	(0.009)	(0.008)
Log R&D	-0.07**	0.39***
	(0.035)	(0.078)
Subsidy indicator × abnormal R&D	-0.676***	-0.156***
	(0.142)	(0.059)
Log options	0.055***	
	(0.012)	
Log incentive pay	-0.051*	
	(0.027)	
Subsidy indicator × log incentive pay	-0.093	
	(0.338)	
Abnormal leverage		0.044***
		(0.012)
Abnormal ROA		-0.915***
		(0.23)
Abnormal returns		-2.514***
		(0.494)

Panel C: Grants ($N = 5,243$)

	(Cheat Equation)	(Caught Equation)
Constant	-2.638***	-2.822*
	(0.553)	(1.661)
Received subsidy	1.984	-11.019***
	(1.588)	(3.286)
Regulated firm	-0.201	-1.198***
	(0.301)	(0.372)
Qui tam industry	0.369	1.021***
	(0.305)	(0.358)
Log assets	0.446***	-0.202
	(0.121)	(0.268)
Log subsidy amount	-0.449***	0.572***
	(0.133)	(0.177)
Leverage	-0.842***	1.599***
	(0.235)	(0.382)
ROA	-1.182**	1.449**
	(0.460)	(0.574)
Annual returns	-0.415***	1.042***
	(0.138)	(0.244)
Number of analyst estimates	0.013	0.033***
	(0.008)	(0.009)
Log R&D	-0.086*	0.231**
	(0.045)	(0.100)
Subsidy indicator $\times$ abnormal R&D	-0.627***	-0.269***
	(0.138)	(0.075)
Log options	0.025	
	(0.021)	
Log incentive pay	-0.126***	
	(0.04)	
Subsidy indicator $\times$ log incentive pay	-2.48***	
	(0.603)	
Abnormal leverage		0.079***
		(0.027)
Abnormal ROA		0.227
		(0.332)
Abnormal returns		-3.75***
		(0.698)

Table 6: Bivariate Probit Results Including Book-Tax Differences: Tax Breaks

Bivariate probit specifications that include the book-tax difference as a variable in the caught equation. The dependent variable is an indicator for AAERs and investor lawsuits. In the caught equation, the following variables are lagged: log assets, (abnormal) leverage, (abnormal) ROA, (abnormal) returns, log R&D. Standard errors are in parentheses. One asterisk (*) denotes significance at the 0.1 level, two (**) the 0.05 level, and three (***) the 0.01 level.

Tax Breaks ($N = 4,788$)

	(Cheat Equation)	(Caught Equation)
Constant	-2.799***	-4.727***
	(0.566)	(0.878)
Received subsidy	4.686**	-8.201***
	(1.93)	(2.571)
Regulated firm	-0.101	-0.328
	(0.297)	(0.301)
Qui tam industry	0.049	1.261***
	(0.309)	(0.329)
Log assets	0.307***	0.312**
	(0.11)	(0.121)
Log subsidy amount	-0.493***	0.248
	(0.139)	(0.159)
Leverage	-0.808***	0.697**
	(0.235)	(0.271)
ROA	-1.312**	-1.013
	(0.523)	(0.645)
Annual returns	-0.526***	0.63***
	(0.145)	(0.181)
Number of analyst estimates	0.025***	0.015*
	(0.008)	(0.008)
Log R&D	-0.072*	0.245***
	(0.037)	(0.058)
Subsidy indicator $\times$ abnormal R&D	-2.58***	-0.154*
	(0.536)	(0.07)
Log options	0.046**	
	(0.021)	
Log incentive pay	-0.068***	
	(0.019)	
Book-tax difference		1.043***
		(0.276)
Abnormal leverage		0.009
		(0.011)
Abnormal ROA		-1.624***
		(0.27)
Abnormal returns		-2.586***
		(0.407)