GOVERNMENT SUBSIDIES AND CORPORATE FRAUD

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Abstract

This dissertation is comprised of three chapters. The first chapter provides an overview of the problem at hand, using real-world examples to underscore the importance of understanding the relation between firms and the regulators who both enable and oversee them. This chapter primarily serves to set up the remaining two chapters. I provide a review of the relevant literature in this chapter.

The second chapter provides a theoretical model that explains many of the empirical findings in the first chapter. In this theoretical model, I consider three players, comprised of a firm and two types of regulators: a legislator and an enforcer. I show that even though only legislators may award subsidies to firms while only enforcers may punish firms for wrongdoing, the relation between legislative and enforcer incentives means that a link between legislator and firm affects the relation between enforcer and firm as well. I use the model to motivate the empirical paper's three main hypotheses: (1) that firms that receive subsidies engage in fraud more frequently, but that (2) the willingness of firms to engage in fraud is a decreasing function of the amount of the subsidy received, and (3) that all else equal, firms that receive tax breaks engage in fraud more frequently than firms that receive cash grants.

The third, and main, chapter empirically studies the relation between firms' receipt of significant subsidies and their subsequent propensities to engage in – and be caught engaging in – financial fraud. Firms that receive subsidies are likely to have greater influence over the legislators who award these subsidies, relative to nonrecipi-

ent firms, but are also more likely to be subject to external scrutiny. Consistent with regulatory capture, I find that firms receiving tax breaks tend to engage in fraud more frequently relative to nonrecipient firms. However, there is an inverse relation between the value of tax breaks that a firm receives and the likelihood that the firm engages in fraud. Conversely, firms that receive direct cash grants or below-market-rate access to resources do not on average engage in fraud any more or less frequently relative to firms that do not receive such grants.

My findings provide insight into the relation between regulatory capture and financial fraud, and suggest that recent standard-setters' recommendations for additional subsidy disclosures by both governments and firms could provide useful information for both policymakers and investors.

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Chapter 1

Introduction and Background

1.1 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 economic development or growth, and prior research documents whether these subsidies achieve their stated purpose (e.g., Cohen et al. (2011)). Little has been said, however, about what these subsidies mean for firms' relations with regulators – specifically, with respect to regulatory capture – and the resulting effects on firm behavior. I study the relation between receiving government subsidies (tax breaks, no- or low-interest loans, or cash grants) and firms' willingness to engage in financial fraud.[1] My paper addresses the following question: is receiving a government subsidy associated with a higher likelihood of firms engaging in fraud?

[1]For the purposes of this paper, I define fraud broadly, encompassing any material misconduct without requiring an "intent to deceive" as in the legal definition of fraud.

Competing arguments can be made for whether receiving a subsidy increases or decreases the likelihood of a recipient firm engaging in fraud. Recipient firms could be dissuaded from engaging in fraud because these tax breaks and other subsidies are often heavily publicized, with the amount of publicity increasing in the magnitude of the subsidy.[2] Under public scrutiny by government officials, analysts or the media, a firm may find it beneficial to report truthfully as detection of wrongdoing is more likely; Dyck, Morse and Zingales (2010) show that a substantial portion of externally detected fraud is in fact caught by non-regulatory parties such as analysts or the media. A decrease in fraudulent activity is also consistent with the political costs literature suggesting that firms facing public scrutiny face more pressure to be good corporate citizens (e.g., Key (1997); Hanlon and Slemrod (2009)). As such, even if regulatory capture is present, there are reasons that a firm may not engage in fraud.

However, legislators have their own incentives that may affect recipient firms. Legislators responsible for granting subsidies do not want their names tarnished by scandal, as such scandals are likely to adversely affect their chances of reelection. This is especially true in the case of large subsidies, for which positive externalities to regional economies are often heavily publicized.[3] Subsequent negative publicity

[2]For example, searching for Boeing's 10 largest tax breaks of the past ten years on Google News suggests an increasing relation between the number of hits and dollar value of the subsidy.

[3]For example, when Hankook Tire Co. built a new plant in Clarksville, TN, an article in USA Today described 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/

from such deals could result in adverse publicity for the legislator and even corruption charges, and to this end legislators with direct links to firms may attempt to suppress bad news surrounding these firms for their own political gains (Piotroski et al. (2015)). Given evidence that enforcers' career concerns frequently make them susceptible to legislative pressure (Mishkin (2001)), a captured legislator can result in low enforcement, which in turn may lead to higher levels of corporate fraud.

I formalize these competing arguments in a three-player game between a firm, a legislator, and an enforcer. The legislator can transfer (1) subsidies to the firm and (2) political capital to the enforcer. The enforcer is responsible for catching the firm when the firm engages in fraud, but can also obtain political benefits when the firm is not caught. Using the model, I show that the effect of a subsidy on firms' willingness to engage in fraud depends on the legislator's costs of awarding a subsidy. The latter result leads me to predict that tax breaks, which are generally longer-term and bear higher costs to awarding legislators, will be associated with a higher incidence of fraud by recipient firms relative to cash grants, which are shorter-term and bear lower costs to awarding legislators.

I empirically test the model's hypotheses using a dataset on corporate subsidies compiled by the nonprofit watchdog group Good Jobs First. The dataset is an extensive resource on federal, state, and local subsidies awarded to individual firms, containing over 500,000 observations between 1983 and 2015. I am able to match Good Jobs First data with other financial and political data for 2,181 of the largest publicly traded firms.[4]

The simultaneous upward (regulatory capture) and downward (third-party deterrence) pressures on a firm's incentives to engage in fraud suggest that a standard reduced-form approach is not sufficient to address the paper's main question. I first use a standard probit model, where the dependent variable equals one if the firm is caught engaging in fraud (i.e., if the firm has *both* decided to engage in fraud been caught engaging). I define fraudulent activity as either receiving an Accounting and Auditing Enforcement Release (AAER) from the SEC or paying a nontrivial settlement in a shareholder lawsuit.[5] Under this standard probit approach, I find that firms that receive government subsidies are caught engaging in fraud less frequently than firms that do not. However, this finding alone could result from one of two scenarios: either (i) subsidized firms engaging in fraud less frequently or (ii) subsidized firms engaging in fraud the same amount (or more) as non-subsidized firms, but being caught less frequently. To determine which of the two cases my finding represents, I use a partially-observed bivariate probit.

Under the partially-observed bivariate probit model, I simultaneously estimate (1) the effects of receiving a subsidy on firms' willingness to engage in fraudulent activity and (2) the likelihood they will be caught engaging in fraudulent activity,

[4]Good Jobs First prioritizes larger firms when matching individual firm-level subsidy recipients to these firms' publicly traded parents, so very few – if any – of the largest US publicly traded firms are missing from their data.

[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.

regardless of who catches the firm. I find that the average firm that does not receive a tax break engages in fraud less frequently than the average firm that does receive a tax break, but that there is no difference for other types of subsidies. Additionally, these subsidies are not strongly related to firm performance, i.e., there does not appear to be a performance-related selection effect with respect to whether firms obtain subsidies or not. To this end, it is unlikely that my results can be explained by differences in fraud behavior between high-performing and low-performing firms.

My paper contributes to the empirical literature on regulatory capture. Prior literature has relied on regulators' personal backgrounds or focused on the characteristics of regulators who pass through the revolving door between government and the private sector (see Dal Bo (2006) for a review). However, focusing on the revolving door does not account for characteristics of captured politicians who do not revolve, while studies that have used regulators' personal characteristics are only able to do so in a way that predicts favoritism to a class of firms as a whole (e.g., a specific industry) rather than to individual firms. Because I use direct monetary linkages between politicians and individual firms, I am able to construct a stronger proxy for the presence of regulatory capture.

My paper also adds to the literature on the relation between political connections and fraud. Most studies relating money in politics to fraud consider the flow of money from firms to politicians. In contrast, my study uses a unique dataset on subsidies to test the relation between fraud and the flow of money in the reverse direction, from politicians to firms. Money provided by firms to politicians measures the intensity of firms' attempts to influence regulators; by contrast, subsidies represent the outcomes of these attempts, and are thus a more direct proxy for the presence of regulatory capture. Additionally, the flow of (disclosed) subsidy dollars from politicians to firms is nearly ten times larger than the flow of (disclosed) lobbying dollars flowing from firms to politicians, according to the watchdog organization OpenSecrets. Understanding the effects of these subsidies on recipient firms' behavior is therefore crucial to understanding the extent to which these subsidies should be regulated, whether with respect to amount, structure of clawback provisions, or associated disclosure requirements.

My study is timely in light of recent Government Accounting Standards Board (GASB) standards (Statement No. 77) that require, for the first time, state and local governments to provide disclosures on corporate tax abatements awarded. The goal of these standards is to create accountability in the wake of recent regional budgetary issues. Further, the Financial Accounting Standards Board (FASB) is currently considering standards (*Disclosures by Business Entities about Government Assistance*) that would require companies to disclose the level of tax abatements received from state and local governments. My study informs these policy issues by shedding light on potential consequences of these subsidies. My results suggest that both the GASB and FASB standards described above could provide useful information for policymakers, investors, and other corporate stakeholders.

The remainder of this dissertation proceeds as follows. Within Chapter 1, Section 2 briefly describes related literature and develops my empirical hypotheses. Chapter 2 provides a theoretical model that develops the paper's main hypotheses, which I test empirically in Chapter 3. Within Chapter 3, Section 1 describes my econometric approach and Section 2 details my data. Section 3 presents main estimation results, while Section 4 briefly describes additional specifications that I consider as robustness checks. Section 5 concludes.

1.2 Prior Literature

My study lies in the intersection of two areas of the academic literature, which are (1) the causes and effects of financial fraud and (2) the role of money in politics. I briefly describe below some of the studies most relevant to this paper.

1.2.1 Corporate Fraud

I outline a handful of papers on the detection and determinants of fraud most relevant to my study. Miller (2006) documents the importance of the press as a monitor for accounting fraud, finding in particular that original analyses by the press lead to a nontrivial amount of detection. Dyck et al. (2010) consider nontrivial investor lawsuits as a measure of corporate fraud, and find that both regulators and third parties account for a substantial portion of fraud detection. Dyck et al. (2013), in a subsequent paper, use a partially-observed bivariate probit to estimate the frequency with which firms engage in fraud. They compare their structural estimation results with a natural experiment based on the sudden demise of Arthur Andersen, and find that the bivariate probit approach is reasonably accurate in estimating the frequency with which corporate fraud occurs. One of the more comprehensive recent studies on the determinants of financial fraud is Dechow et al (2011), who compare the predictive ability of numerous performance-based, accrual-based, and nonfinancial variables to predict Accounting and Auditing Enforcement Releases (AAERs). My contribution to this literature is to document another potential determinant of both the incidence and the likelihood of detection of fraud, in the form of subsidies.

1.2.2 Political Connections

A large literature studies the influence that political connections between firms and politicians have on favoritism displayed toward these firms. I outline below a handful of recent studies in this literature. Kim and Zhang (2016) construct three different measures of political connectedness between firms and politicians, both monetary (campaign contributions and lobbying) and non-monetary (employment of directors connected to politicians). They find that more politically-connected firms are more aggressive in their use of tax shelters, likely as a result of decreased scrutiny from regulators. Regulators may sometimes even go beyond simply turning a blind eye; Piotroski et al. (2015) document that Chinese local politicians often take actions to suppress bad news about firms they are connected to. Regulators may also take a more direct approach to suppressing bad news, via making sure there is no bad news to suppress; Heese (2015) finds an inverse relation between the likelihood of SEC enforcement and the proportion of a congressional district's workers a firm employs, conditional on the representative from that district sitting on a committee that oversees the SEC.

There is also a subset of the political connections literature that focuses specifically on the flow of money between politicians and firms. For example, 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 different measure of the interconnectedness between firms and politicians, based on politicians' personal financial portfolios, and uses this measure to show that firms with stronger ties to politicians receive more government contracts. Collectively, these papers suggest a substantial return on investment that firms make in politicians. My study expands this literature in two ways. First, I introduce a much broader setting to study this ROI with respect to politicians, in the form of government subsidies. Second, the purpose of much of the literature described above has been to document the existence of politician-to-firm monetary flow; I take the existence of this flow as given and study the effects it may have on firms' willingness to engage in fraud.

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 government spending, there is a much stronger association between congressional committee representation and the distribution of government spending (also see Aghion et al. (2009)). These findings suggest that a city/county/state's subsidy offer is more related to its politicians' incentives than to other aspects of its business environment.

Perhaps the most similar study to mine is Correia (2014). Correia 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 considers the firm-to-politician monetary flow, in part due to the availability of comprehensive data on corporate lobbying expenditures. In contrast, I study the effect of monetary flows in the opposite direction: from politicians to firms.

Chapter 2

Model and Hypotheses

I provide a theoretical model in this section that complements the main empirical analyses. My goal in this section is not to directly derive parameters for estimation so much as it is to provide some intuition into the political process – and potential linkages between firms and regulators. The paper's three main hypotheses can all be derived from the model results presented below. I begin by providing a table of relevant variables on the next page for the reader, and then explain the model and its main conclusions.

Model Variables

Variable	Description
s	Subsidy amount awarded by regulator (decision variable)
$\tilde{w}$	Strength of manager's incentive to misreport
$\tilde{x} \in \{x_L, x_H\}$	Earnings privately observed by manager
p	Probability that $\tilde{x} = x_H$
r	Report about $\tilde{x}$ that manager makes
c_F	Cost to manager if caught misreporting $\tilde{x}$
θ	Probability enforcer detects misreporting (decision variable)
$z(\theta)$	Cost to enforcer of exerting effort level θ
$G(s)$	Probability third-parties detects misreporting
q	Conditional probability firm misreports upon observing low (x_L) state
π_1	Payoff to enforcer from successful fraud detection
π_0	Payment from legislator to enforcer if nobody detects fraud ("political capital")
$B(s)$	Benefit to legislator from awarding subsidy amount s
c_L	Cost to legislator if firm caught misreporting
$\sigma(\pi_0)$	Cost to legislator of providing political capital π_0

2.1 Gameplay

I consider a game with three players: (1) the firm, (2) the subsidizing regulator (hereafter referred to as the "legislator"), and (3) the enforcing regulator (hereafter referred to as the "enforcer"). Third-party (e.g., media, analyst) monitoring also enters the model, but I do not explicitly model the incentives of these third parties.

The game proceeds in the following stages. Initially, in Stage 1, the firm is offered a take-it-or-leave-it subsidy s from the legislator, which it may accept or decline. The subsidy amount s is publicly known if the firm accepts, although if the firm declines the public cannot distinguish this from a setting in which no subsidy was offered. The firm also has private information $\tilde{w}$, which represents managerial incentives for engaging in fraud. At this stage in the game, neither legislator nor firm knows the realization of $\tilde{w}$. Since subsidies are often awarded for a several-year period it is reasonable to assume that a manager, although he may know the general structure of his pay in a future year, may not know the specific incentive structure $\tilde{w}$ until much later than the date of the subsidy award. As an example, a manager may know that her compensation two years from now will include a bonus based on an earnings threshold, but she may not know the exact earnings threshold or the level of the bonus.

Also in Stage 1, the legislator offers the enforcer a potential incentive π_0 for *not* detecting fraud by a subsidized firm. π_0 need not, and in most developed economies will not, represent a direct cash transfer. Rather, π_0 represents other benefits that the legislator may provide to the enforcer, such as political support or promises of increased power.[1] I assume that π_0 is common knowledge to firm, legislator, and enforcer.

After being offered the subsidy and accepting or rejecting, in Stage 2 the firm privately observes managerial incentives $\tilde{w}$ and earnings $\tilde{x}$. $\tilde{x}$ is either low (x_L) with probability $1-p$ or high (x_H) with probability p. The market rationally prices the firm according to its belief about the firm's expected true value. When the firm observes x_H, it reports truthfully; when the firm observes x_L it may either report truthfully or misreport $r = x_H$. Its reporting decision is based on $\tilde{w}$ as well as anticipation of the likelihood of being caught by the enforcer or a third party. Finally, in Stage 3, the enforcer and third parties attempt to catch the firm engaging in fraud if the firm reports x_H. Below, I outline the three parties' incentives as well as equilibrium predictions on the effect of subsidies.

2.2 Firm's Problem

Let $r \in \{x_L, x_H\}$ denote the firm's disclosure of its earnings. Recall that $\tilde{w}$ denotes the manager's private incentives. The firm's problem can be written as follows:

$$\max_r w\mathbb{E}[\tilde{x}|r] - c_F \cdot \mathbb{P}(\text{caught}) \tag{2.1}$$

[1] As an example, there were 37 gubernatorial elections in 2010; in 10 of these, the state attorney general (an enforcer) ran as a candidate for governor (a legislator). Political support from an incumbent governor could therefore have meaningful value to a current enforcer.

where $\mathbb{P}(\text{caught})$ is a function of the subsidy s and other regulatory characteristics and c_F is a constant, commonly known multiplier that determines the severity of punishment when a firm is caught. The probability of being caught is the sum of two quantities: (1) the probability of being detected by a regulator, and (2) the probability of being detected by a third party (analysts, the media, etc.) when the regulator fails to detect. In other words, the probability the firm is caught, conditional on engaging, is

$$\mathbb{P}(\text{caught}|\text{engage}) = \underbrace{\theta}_{\text{Enforcer}} + \underbrace{(1-\theta)\,G(s)}_{\text{Third parties}} \tag{2.2}$$

where θ denotes the probability the enforcer catches the firm cheating and $G(s)$ is the conditional probability the firm will be caught engaging by third parties (media, analysts, etc.), as a function of the subsidy s. I assume that $G'(s) \geq 0$, i.e., an increase in the level of a subsidy does not make it *less* likely that a third-party detects fraud, conditional on a regulator not doing so.

The market is rational and prices the firm according to its expected value given the firm's earnings report:

$$\mathbb{E}[\tilde{x}|r = x_H] = x_L + (x_H - x_L)\frac{p}{p + (1-p)q} \tag{2.3}$$

where q denotes the conditional probability the firm misreports when it observes low earnings. Given this, upon observing $\{w, x_L\}$, the firm misreports if it is better to do so than to report truthfully, i.e., if

$$x_L + w\frac{p}{p+(1-p)q}(x_H - x_L) - c_F \cdot [\theta + (1-\theta)G(s)] > x_L. \tag{2.4}$$

Normalizing $c \equiv \frac{c_F}{x_H - x_L}$, this means the firm misreports iff

$$\frac{wp}{p+(1-p)q} \geq c\,[\theta + (1-\theta)G(s)] \tag{2.5}$$

or, equivalently, iff

$$w \geq \left(1 + \frac{1-p}{p}q\right)K \tag{2.6}$$

where $K = c\,[\theta + (1-\theta)G(s)]$.

This yields a threshold solution in which the firm misreports when the manager's incentives w are higher than some threshold w^* and does not otherwise; in such a case, q represents $\mathbb{P}(w > w^*)$. Recall that because of the timing of the game, the distribution of w^* is invariant to the subsidy s, and suppose w is drawn from distribution F. Then

$$w^* = \left(1 + \frac{1-p}{p}\left(1 - F(w^*)\right)\right)K \tag{2.7}$$

which can be rewritten as

$$\frac{w^*}{1 + \frac{1-p}{p}\left(1 - F(w^*)\right)} = K. \tag{2.8}$$

It is evident that an increase in K leads to an increase in the incentive threshold w^*.

Because $\frac{\partial K}{\partial \theta} > 0$, this means that w^* is increasing in θ, and hence that $\frac{\partial q}{\partial \theta} < 0$, i.e., the probability of engaging decreases when θ increases. Because θ is a function of the subsidy s, it is not evident without further information on the distributions G and F whether $K(\theta(s), s)$ is increasing or decreasing in s.

2.3 Enforcer's Problem

The enforcer's problem represents two types of career concerns. Specifically, the enforcer can add to his reputational value in two ways: (1) by catching the firm in the act of cheating, or (2) by currying enough political favor with the legislator for political promotion. The latter represents the case in which the enforcer is nominated by the legislator as well as the case in which the enforcer may ultimately want the legislator's job. The enforcer thus chooses a conditional probability of detection to maximize his payoff function.

The enforcer's effort is costly, such that successfully catching fraud – conditional on it existing – with probability θ bears cost $z(\theta)$. The enforcer obtains positive payoff if he catches the fraud or if the fraud is not caught by any party (currying favor), while he obtains no payoff if the fraud is caught by a third party. Recalling $q \equiv q(s, w^*)$ denotes the probabiltiy of the firm cheating, the enforcer's payoff function conditional on observing high earnings x_H is

$$\max_{\theta} \underbrace{q\theta\pi_1}_{\text{Catch firm}} + \left[\underbrace{q(1-\theta-(1-\theta)G(s))}_{\text{Firm cheats but not caught}} + \underbrace{(1-q)}_{\text{Firm doesn't cheat}}\right]\pi_0 - z(\theta) \tag{2.9}$$

$$= \max_{\theta} q \cdot [\pi_1\theta + \pi_0(1-\theta)(1-G(s))] + \pi_0(1-q) - z(\theta) \tag{2.10}$$

where π_1 and π_0 are the payoffs he obtains from detecting fraud himself and from nobody detecting fraud, respectively. I assume that π_1 is exogenously given and known to all parties. π_0 represents political capital, i.e., non-monetary benefits that the legislator can provide to the enforcer, since the enforcer cannot be directly bribed with money. Rather, the legislator "pays" the enforcer by providing benefits such as increased power or endorsement during electoral races.

Given s, π_1, and π_0, the enforcer chooses a success probability θ to solve his first-order condition. Because of the fact that q has already been determined at this point in the game based on what the firm thinks the enforcer will do, the enforcer's first-order condition is given by

$$q[\pi_1 - \pi_0\,((1-G(s))] - z'(\theta) = 0 \tag{2.11}$$

As long as $\pi_1 > \pi_0(1-G(s))$, there is a solution in θ. One relatively weak sufficient condition for an equilibrium to exist is therefore $\pi_1 \geq \pi_0$, i.e., π_0 is bounded above by π_1.

2.4 Legislator's Problem

The legislator can influence both the firm and the enforcer through his behavior. When the legislator awards subsidy s, he obtains some benefit $B(s)$ from doing so. This benefit could represent improved reputation via job creation or potential future political support from the firm. Since the legislator makes the first move, his decision fully informs the firm's and enforcer's strategies.

The legislator also bears a potential cost when awarding a subsidy. If the firm is caught engaging in fraud, there is a chance that the legislator's name is tarnished as well. I assume this cost is proportional to the probability of detection. The legislator can also attempt to control his subsidy costs via incentives for the enforcer to not catch the firm by choosing political capital π_0. The cost of awarding π_0 is given by some function $\sigma(\pi_0)$, and is only incurred if π_0 is collected by the enforcer. The legislator's payoff function is therefore

$$\max_{s,\pi_0} B(s) - \delta \cdot c_L - \sigma(\pi_0) \tag{2.12}$$

where $\delta = (1-p) \cdot q \cdot (\theta + (1-\theta)G(s))$, i.e., the probability that the firm observes x_L, reports x_H, and is caught. The legislator faces a tradeoff: to reduce the likelihood of paying c_L as a result of the firm being caught, he must pay $\sigma(\pi_0)$ to lessen the enforcer's incentive to detect fraud. The cost c_L need not be a direct monetary cost; it could be, for example, the increased likelihood a legislator will be voted out of office the next time (s)he runs for office.

2.5 Equilibrium

The Enforcer's first-order conditions provide insight into the effects of changes in political capital π_0 and in the subsidy s. I prove below that when subsidies increase relative to political capital the firm engages in manipulation less frequently. Put another way, consider two sets of parameters that generate equilibria $\{s_1^*, \pi_0^*\}$ and $\{s_2^*, \pi_0^*\}$ where $s_1^* > s_2^*$. Firms are less likely to engage in manipulation under the first equilibrium compared to the second; even though the increase in $G(\cdot)$ motivates the Enforcer to exert more effort, this is dominated by the third-party monitoring effect. This result does not rely on any distributional assumptions about G or σ, and generates the following hypothesis, which I test empirically:

Hypothesis 1. *Subsidy size is negatively associated with firms' fraudulent activity.*

2.5.1 Proof of Equilibrium Result

I compare two equilibria with the same level of political capital and show that in the one where subsidies are higher, manipulation is lower.

Consider the Enforcer's first-order condition (2.11). Assuming that $z'(\cdot)$ is invertible, we can rearrange this to write

$$\theta^* = z'^{-1}\left(q[\pi_1 - \pi_0(1 - G(s))]\right) \tag{2.13}$$

Since $z'(\cdot) > 0$, we also have $z'^{-1} > 0$; and since z is assumed to be convex, the derivative of $(z')^{-1})$ is positive as well. This can be used to study the effects of changes in the legislator's decisions s and π_0 on the firm's behavior. Recall that q is determined by the firm's problem, via

$$\frac{p}{c}\frac{w^*}{p+(1-p)q} = \theta(1-G(s)) + G(s)$$

Plugging in for θ we can rewrite this as

$$\frac{p}{c}\frac{w^*}{p+(1-p)q} = z'^{-1}\left(q[\pi_1 - \pi_0(1-G(s))]\right)(1-G(s)) + G(s) \tag{2.14}$$

We can study the effect of a change in s, holding π_0 the same, on q by considering the RHS of this equation. Suppose q is held fixed, and s is increased by some small amount to s_2. We have

$$z'^{-1}\left(q[\pi_1 - \pi_0(1-G(s_2))]\right) > z'^{-1}\left(q[\pi_1 - \pi_0(1-G(s))]\right) \tag{2.15}$$

Note also that $(z')^{-1}(\cdot) \leq 1$. Consider the following lemma:

Lemma 1. *Suppose* $1 \geq x_2 > x_1 \geq 0$ *and* $1 \geq \alpha_2 \geq \alpha_1 \geq 0$. *Then* $\alpha_1 + (1-\alpha_1)x_1 < \alpha_2 + (1-\alpha_2)x_2$.

Proof:

$$\begin{aligned} & [\alpha_2 + (1-\alpha_2)x_2] - [\alpha_1 + (1-\alpha_1)x_1] \\ = \; & (\alpha_2 - \alpha_1) + (1-\alpha_2)x_2 - (1-\alpha_1)x_1 \\ > \; & (\alpha_2 - \alpha_1) + (1-\alpha_2)x_1 - (1-\alpha_1)x_1 \\ = \; & (\alpha_2 - \alpha_1)(1 - x_1) \\ \geq \; & 0 \end{aligned}$$

Using Lemma 1, with $x_1 = z'^{-1}(q[\pi_1 - \pi_0(1-G(s))])$, $x_2 = q[\pi_1 - \pi_0(1-G(s_2))]$, $\alpha_1 = G(s)$, and $\alpha_2 = G(s_2)$, we see that holding q constant, an increase in s increases the right-hand side. Hence, for the equality to hold, q must be lower (which means w^* is higher). In words, for two equilibria that admit a given level of political capital, the equilibrium in which subsidies are higher will see a lower probability the firm manipulates.

2.5.2 Can Subsidies Result in Higher Levels of Fraud?

The result above may suggest that the likelihood of misreporting decreases in the subsidy size s. However, it is not obvious what happens when the equilibrium subsidy level s^* is compared to the no-subsidy case. When subsidies are not awarded, the legislator has no incentive to spend political capital on the enforcer, i.e., he sets $\pi_0 = 0$; the legislator's overall payoff is zero and he is weakly worse off than the case in which he is allowed to subsidize firms. Without subsidies, the enforcer now only obtains payoff if successfully detecting fraud, i.e., he chooses a new effort level $\hat{\theta} > \theta^*$. I therefore consider whether it is possible for the $s = 0$ case to lead to lower fraud levels than the regulator's offer $s = s^*$.

It suffices to compare the probability of detection under the no-subsidy case, $\hat{\theta} + (1 - \hat{\theta})G(0)$, with the probability of detection under the game described above, $\theta^* + (1 - \theta^*)G(s^*)$. The difference between the two quantities can be written as

$$\underbrace{\left(\hat{\theta} - \theta^*\right)(1 - G(s^*))}_{>0} + \underbrace{\left(1 - \hat{\theta}\right)[G(0) - G(s^*)]}_{<0} \tag{2.16}$$

A positive value of (2.16) means that manipulation is higher when $s = 0$, whereas a negative value of (2.16) means that manipulation is lower when $s = 0$. When written in this form, it is evident that as long as $G(\cdot)$ can take on the full range of values between $[0, 1]$, it is always possible for there to be an s^* such that more manipulation occurs for $s = s^*$ than for $s = 0$. To see this, suppose that ζ parameterizes the function σ such that a higher ζ denotes higher costs of political capital for any level of π_0. By making ζ smaller we can increase π_0, until the point where θ goes to zero and the legislator's optimization problem is effectively

$$\max_s B(s) - G(s) \cdot c_L \tag{2.17}$$

The s^* chosen to solve (2.17) solves $B'(s^*) = c_L G'(s^*)$, and the conditional probability of detection is given by $K = G(s^*)$. If instead π_0 were set to zero, a large enough

π_1 can be chosen such that the probability of detection $\hat{\theta}$ by the enforcer is higher than $G(s^*)$; thus, even if $G(0) = 0$, the no-subsidy case results in a higher chance of being conditionally caught and therefore a lower likelihood of the firm engaging.

The results above suggest that the overall association between subsidies and fraud depends on several factors and hence cannot be conclusively signed. I therefore test the following hypothesis, stated in the null, about the subsidy effect:

Hypothesis 2. *Receipt of a subsidy is not associated with the propensity to engage in fraud.*

Hypothesis 2 motivates the need for empirical tests of the subsidy effect. However, note that not all subsidies are the same from the legislator's perspective; some subsidies may come at higher cost than others. These costs are important because the equilibrium quantities s and π_0 are substitutes in the following sense. Suppose that α parameterizes the function G, such that $\frac{\partial G}{\partial \alpha} > 0$; that is, a higher α indicates a higher likelihood of third-party fraud detection given any subsidy level s. Recalling that ζ parameterizes the function σ in the same way, it follows directly from Shephard's Lemma that s and π_0 are substitutes with respect to costs. That is, when political capital becomes more expensive (ζ increases), π_0 goes down and s goes up; and when subsidies become costlier to award via an increase in α, s goes down while π_0 goes up. In practice, this substitutability means that costlier subsidies are associated with higher levels of political capital, in turn leading to lower enforcement and potentially higher levels of fraud.

I test this notion empirically by considering two distinct classes of subsidies: tax breaks and 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 economic actions.[2] Additionally, tax breaks are never competitively awarded and are often longer-term, creating stronger linkages between politician and firm; they are therefore typically higher-cost than cash grants, which are often shorter-term and may sometimes be competitively awarded. I therefore test the following empirical prediction:

Hypothesis 3. *Tax breaks will be associated with a higher incidence of fraud relative to cash grants.*

As an empirical matter, there is also a potential distinction between regional and federal subsidies. Given that most enforcement agencies are at the federal level, one might expect a difference between the effects of the two subsidy sources. However, there are significant political links between regional politicians and federal regulatory agencies.[3] External monitoring of subsidies also has a substantial local and state

[2]For an example of academic research, see Alesina and Ardagna (2010), who compare macroeconomic growth rates as a result of both tax cuts and spending increases. For an example of popular press, see an op-ed by Christina Romer at `http://www.nytimes.com/2011/07/03/business/economy/03view.html?_r=0`.

[3]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. 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. For example, Kedia and Rajgopal (2011) provide evidence that the location of SEC regional offices plays a role in the enforcement process.

component.[4] Given these linkages, I therefore do not distinguish between state-funded and federally-funded subsidies in testing H2 or H3.

[4]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. Galvin (2008) describes the significant role of state-level securities regulators in initiating enforcement action, while Coffee and Sale (2009) find that both (1) state attorneys general both play a substantial role in prosecuting fraud and in aiding the SEC; and (2) state-level securities regulators are highly susceptible to capture.

Chapter 3

Empirical Analyses

3.1 Empirical Strategy

3.1.1 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. To this end, while I begin my empirical analyses by estimating a standard probit model relating the likelihood of being caught engaging in fraud to two subsidy-related variables, my goal is to understand the channel through which a subsidy effect, if any, occurs. To disentangle the effect of subsidies on firms' incentives to cheat from the effect of subsidies on firms' likelihood of being caught, I therefore use a partially observed bivariate probit framework (Poirier (1980)). This framework is used in several corporate fraud papers including Dyck et al. (2013), 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 simultaneously 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 engage in fraud and (2) whether the firm is caught cheating conditional on deciding to engage in fraud.

Estimation of a partially-observed bivariate probit requires a set of variables that satisfy an exclusion restriction in thes 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:

$$\mathbb{P}(\text{cheat}) = X\beta_1 + Z_1\gamma_1 + \varepsilon \tag{3.1}$$

$$\mathbb{P}(\text{caught}|\text{cheat}) = X\beta_2 + Z_2\gamma_2 + u \tag{3.2}$$

where Z_1 and Z_2 must be mutually exclusive and nonempty and X denotes variables that influence both equations. The error terms ε and u must be normally distributed, but do not need to be independent of one another. Note that when initially estimating the standard probit, I use all possible variables (X, Z_1, Z_2).

3.1.2 Variables

Subsidy Variables

The analytical model in Section 3 predicts that subsidies influence both the firm's willingness to engage in fraud as well as the likelihood of detection, and to this end I include subsidy-related variables in both equations of the bivariate probit. I include both an indicator variable for whether a company has received a subsidy as well as a variable that indicates the dollar amount. I construct these two variables using the company's most recent three years (years t, $t-1$, $t-2$) in order to take a longer-term view of the effect of government spending and political favors. Use of a three-year sum is also consistent with prior literature (Snyder (1992); Kroszner and Stratmann (2005)).

Other Variables

Prior literature (Burns and Kedia (2006); Efendi et al. (2007); Armstrong et al. (2010)) suggests using executive compensation metrics as a proxy for managerial incentives. Following Dyck et al. (2010, 2013) I use managerial incentive pay as well as unexercised exercisable options. I assume that these two variables affect the "cheat" equation, but not the caught equation (i.e., they make up Z_1); this assumption is based on the temporal dynamic of required managerial compensation disclosures. While firms are required to disclose the compensation of top executives, this disclosure occurs after executives have already earned 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 behavior is not a figure known publicly at the time of decision-making.[1]

For Z_2 – that is, variables that appear only in the "caught" equation – I use quantities observed by investors, regulators, and other parties outside the firm. All of these parties have their own incentives to detect (and report) fraud when it occurs. Investors who fail to proactively detect fraud may incur a larger financial loss if the fraud is then announced by another party, due to an inability to sell shares before a stock price crash. While there may be some desire by investors to keep fraud quiet to maintain a high stock price, a rational investor would be unlikely to quietly hold stock that she knows is overvalued. Analysts and regulators who miss instances of fraud will suffer adverse reputational consequences. Based on previous literature (Dyck et al. (2010), Wang (2013)) I use abnormal ROA, abnormal returns, and abnormal leverage as Z_2. Wang (2013) provides caveats for the use of these variables;[2] however, the temporal aspect of these three abnormal variables makes them reasonably

[1]Some caveats apply; for example, ex ante disclosure of the structure of a longer-term contract would provide additional public information on managerial incentives. However, I assume that this type of disclosure is not public information.

[2]Wang specifically mentions two caveats: (1) that the variables are "not completely exogenous," and (2) that management has better information about the firm's future performance. However, with respect to (1), Wang (2013) argues that the direction of causality is unambiguous, because in the short term poor performance can reveal fraud, but not vice versa. With respect to (2), she argues that managers commit fraud because they believe that current bad performance is an aberration, not believing that the firm may have taken a lasting downturn.

suitable for use as Z_2. Specifically, when a manager is deciding whether to engage in fraud, she does not know what the realizations of abnormal variables will be. In general, while all of the variables that I use as Z_1 and Z_2 ultimately become public information, I rely on differences in information sets at the time of decision-making in order to satisfy Poirier's exclusion restriction of nonempty $Z_1 \neq Z_2$.

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

$$x_{ijt} = \gamma_0 + \gamma_1 x_{it-1} + \gamma_2 \overline{x_{jt}} + \varepsilon_{ijt} \quad (3.3)$$

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 (3.3) above serve as my abnormal ROA, abnormal returns, and abnormal leverage. Jones and Weingram (1996) and Dyck et al. (2010, 2013) find that abnormal ROA and abnormal returns are negatively associated with the likelihood that firms are caught engaging in fraud, and suggest that this is because firms with high abnormal ROA or abnormal leverage are perceived as less likely to need to misreport. These three studies also find that higher abnormal leverage is associated with firms being caught more often conditional on engaging, because of the perception of higher risk.

Variables appearing in both the cheat and caught equations include firm size, measured by log assets, and log R&D. I do not have a prediction for the effects of firm size in either equation. While a larger firm may have more political influence, it is also likely to be subject to more third-party monitoring and hence may not find it optimal to engage in fraud. 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 (2013) 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 on fraud incidence for subsidy recipients specifically is less clear. In my sample, high-R&D firms tend to receive subsidies more frequently and the subsidies they receive tend to be higher. As a result, recipient firms' heightened dependence on subsidies for R&D purposes may serve as a deterrent from fraud – even though the higher level of opacity of the fundamentals of high-R&D firms may make fraud more difficult to detect.

Additional control variables in both equations include firm leverage, an indicator for whether a firm is in an industry in which qui tam lawsuits[3] are possible, and the number of analysts following a firm. The first two are based on previous literature (Yu (2008), Jones and Weingram (1996)) and represent cases where there may be more regulatory monitoring, while the analyst variable represents the amount of attention that the firm is being paid by analysts; more analyst attention may increase the likelihood of an analyst detecting fraud.

3.1.3 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 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; see Table 3.1 for details. To disentangle the effect of R&D or firm size from the effect of a subsidy, I instrument for the received-subsidy indicator as well as the log of subsidy dollars received.

[3]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.

Previous literature (Cohen et al. (2011)) has used politicians' ascent to various House of Representatives and Senate committees as an exogenous shock to the subsidy supply function for firms headquartered in those politicians' jurisdictions. Because 73.5% of my subsidy data is at the state and local level, however, using the Cohen et al. (2011) instrument would require ignoring the vast majority of my data; I am therefore unable to use the Cohen et al. (2011) instrument.[4] I therefore use state gubernatorial elections as a shock to a state's economic environment. 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. Gubernatorial elections have previously been studied in the accounting literature, with results suggesting that election-year behavior is systematically different (Kido et al. (2012)). Practitioners themselves have highlighted the importance of elections; in a 2004 presentation for corporate government-affairs professionals, the director of Ernst & Young's Business Incentives Practice – which frequently consults with these professionals on obtaining subsidies – highlights the need to be mindful of election years when asking for subsidies (LeRoy (2005), p. 88).

I use as my instruments indicator variables indicating election years, whether the incumbent governor reruns for election,[5] whether the incumbent governor loses, and

[4]Data on state-level House and Senate committees is less available, and it is more difficult to determine how much say state politicians have in a given bill compared to how much say U.S. Congress members have in a given bill.

[5]Several states have term limits for governors. I do not differentiate those governors facing term

whether the margin of victory in the gubernatorial race was less than four percent; the four percent cutoff comes from political analysis website Politico. This latter variable captures tight races and allows me to study whether an incumbent's behavior changes in the year before running for reelection against a tough opponent. Using media definitions of swing states allows me to capture public perception surrounding around an election.

I expect that during election years, subsidies will become more generous when the race is close. In such races, a legislator may be willing to employ a high-risk strategy of awarding generous subsidies to garner short-term support from constituents. By contrast, in non-tight races, a legislator may prefer to employ a more conservative strategy of reducing subsidy awards given that the risk of fraud being exposed outweighs the marginal extra support a subsidy would garner from recipient firms. I thus expect that the coefficient on the election-year indicator will be negative; that the coefficient on the tight race indicator will be positive; and that the sum of these two coefficients will be positive.

As a falsification test, I also estimate my first-stage (instrumental) equation defining the subsidy variable to include only federal subsidies. I find no significant election-year effects when the subsidy is at the federal level, supporting the notion that regional elections do primarily serve as a supply shifter for subsidies issued within the relevant state.

limits from those governors who voluntarily choose not to run for reelection due to, for example, other political pursuits (e.g., becoming a U.S. Senator) or private sector pursuits.

I also construct instruments based on competitors' subsidies received.[6] Specifically, I use the log of the sum of all other subsidies received by other firms within the same industry-time groups as well as the log of the sum of all other subsidies received by other firms headquartered within the same state during the same time period. This latter variable accounts for the fact that the majority of subsidy dollars in my dataset come from state and local governments. Constructing a state-level aggregate subsidy variable therefore reflects characteristics of a firm's environment that are determined outside of the firm, providing exogenous variation in the environments faced by competitor firms based in different states. 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, some specifications involve interaction terms between the subsidy indicator and at least one other variable (incentive pay, or a financial crisis indicator). I instrument for these interaction terms as follows. Letting *subs* denote a subsidy variable (either indicator or dollar value) and x the variable to be interacted with

[6] For a discussion of these types of instruments, see Larcker and Rusticus (2010) as well as Nevo (2000)

subs, if the 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 a first-stage equation for the interaction term.

3.2 Data

My data falls into four categories: (1) subsidy data, (2) fraud data, (3) financial data, and (4) political data.

3.2.1 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 500,593 total subsidies between 1983 and 2015 (with more per year in more recent years). Each observation in the dataset provides the recipient name, name of the awarding regulatory body, name of the specific subsidy program, year of award, dollar value of the subsidy, and type of subsidy (e.g., grant, low-interest loan, tax credit, enterprise zone), as well as several other attributes (location, funding agency, etc.). Appendix A provides additional details on subsidy classifications.[7] In addition, Appendices B and C provide examples of large subsidies as well as a breakdown of subsidies by state over the sample period, respectively. While the exact amount of a subsidy may vary based on firm performance, especially in the case of tax abatements, the actual term lengths are relatively stable with respect to proposed term lengths; historically, very few subsidies have included strong clawback provisions to be implemented in the case that a firm does not meet stated criteria.

GJF does not provide any firm identifier beyond company name (and parent company name), so I hand-match GJF data to Compustat data. I obtain a total of 78,855 individual subsidy observations for 2,181 publicly traded firms between 2003 and 2011.[8] I aggregate this to the firm-year level, using the sum of all subsidies received. I create the subsidy variable using total subsidy value rather than per-year subsidy value, as these are the numbers used by politicians and the media when discussing subsidies.[9] My intent is to study the impact of *receiving* a subsidy rather than the direct monetary impact of the subsidy itself.

After imposing restrictions on firm size and removing firms with missing data,

[7]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; to this end, it is likely that any estimated effect I find understates the true magnitude of a subsidy effect.

[8]Conversations with Good Jobs First staff suggest that the subsidy data is much more complete from 2003 onward, and so I consider this the initial year of usable subsidy data.

[9]In the case of Alcoa, for example, major news outlets reported the $5.6 billion figure but not yearly figures (either broken down by year, or a simple yearly average). See `http://www.forbes.com/pictures/emeh45mfmk/no-2-alcoa-new-york-state-5-6-billion-3/` for sample news coverage.

there are 1,300 distinct firms in the final sample. 752 of these firms received a subsidy at least once between 2003 and 2011, while the remaining 548 firms did not. Table 3.2 presents subsidy summary statistics at the firm-year level.

Table 3.2 presents summary statistics on my subsidy dataset. Panel A provides information on the value of subsidies aggregated to the firm-year level obtained conditional on receiving a subsidy in a given year. We can also see from Panel A that there is no obvious trend (increase or decrease) in the level of the average subsidy awarded each year. Unsurprisingly, subsidies peak in both volume and dollar value around the 2007-09 financial crisis. Panel B of Table 3.2 presents aggregate statistics on subsidies by year level, aggregated across firms with assets greater than $750 million. Note that the average tax break is more valuable than the average cash grant; although the number of grants exceeds the number of tax breaks awarded in most years, the total dollar value of tax breaks awarded is generally larger than the total dollar value of grants awarded. Panel A of Figure 3.1 provides additional detail on the distribution of dollar values of these subsidies, which appear to be roughly lognormal. Figure 3.1 also provides detail on the frequency of subsidy awards. Panel B suggests that while the plurality of firms receive subsidies in only one state-year between 2003 and 2011, many firms receive multiple subsidies across locations and time. Panel C considers within-year behavior, and shows that the vast majority of firms receiving subsidies in a given year do so from only one state; this is somewhat surprising given that my sample consists of large firms which, by nature of their size, frequently have operations in many states.

3.2.2 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). Further details on the fraud variables can be found in Table 3.3. 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 fraud.[10]

Lawsuits

Lawsuit data comes from the Stanford Securities Class Action Clearinghouse (SSCAC) database. SSCAC contains information on securities lawsuits filed from 1995 onward, including date, status (settled/dismissed/ongoing), and settlement amount. On average, class action lawsuits take 577.1 days to resolve from the date they are filed; many SSCAC lawsuits from 2012 onward are still ongoing, so I limit the sample period to years prior to 2012. Because larger firms are more likely to be sued than smaller ones, I also limit the sample to firms with assets over $750 million (Dyck et al. (2013)). The lawsuit-based fraud indicator is equal to one when a lawsuit is

[10] 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.

settled with a settlement amount equal to at least $1.5 million (Choi (2007); Choi et al. (2009)).

AAER

My second proxy for fraud is SEC Accounting and Auditing Enforcement Release (AAER) data (Dechow et al. (2011)). Because AAERs are subject to the SEC's budget constraints, they are a high-quality proxy for fraud in that they represent the most serious of accounting violations; it is extremely unlikely that an AAER recipient did *not* engage in fraud. However, due to the limited scope of AAERs, I use AAERs in conjunction with the SSCAC data rather than as a standalone proxy for fraud.

3.2.3 Financial Data

Managerial incentive variables are incentive pay and total value of exercisable unexercised options. I construct both of these variables as an average taken over all executives for whom data is available in Execucomp or Equilar.[11] I define incentive pay as total compensation minus base salary.[12] I primarily use the log of total incentive pay rather than incentive pay as a fraction of total compensation.

[11] When data on an individual is available from both Execucomp and Equilar, I use the Execucomp data values; otherwise, I use Equilar.

[12] Dyck et al. (2013) define incentive pay as the ratio of restricted stock grants to total compensation, but using this definition of incentive pay cuts my final regression sample almost in half due to data unavailability. My total compensation variable is TDC1 in Compustat, which 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 control 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)

The remainder of the variables in my "cheat" and "caught" equations are financial. Returns and CAPM residuals are computed using CRSP monthly returns data; ROA, assets, sales, R&D, and leverage are computed using Compustat data. My proxy for analyst attention is the number of distinct analysts making forecasts in each firm-year, from I/B/E/S.

3.2.4 Descriptive Statistics

Because I obtain data from several different sources and drop firms with less than $750 million in assets, the intersection of the set of available data from all of these sources limits my analysis to 7,048 firm-year observations. Table 3.4 demonstrates how I arrive at this final sample size. The majority of dropped observations result from either being banks or utilities, or from missing executive compensation data from Equilar or Execucomp. Table 3.3 provides information on the relative frequency of the fraud variable. While lawsuits are relatively stable over time, the number of AAERs filed against large firms declines over time.

Tables 3.5 and 3.1 presents summary statistics and correlations for the final regression sample. In Table 3.5, 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. By contrast, I use the logs of these variables in Table 3.1 to reflect the final regression variables. Subsidies have a substantial correlation with assets and R&D, but have a much lower correlation with all of the other control variables. In particular, the low correlation between subsidies and the firm performance variables (ROA and returns) suggests that adverse selection with respect to subsidy recipients – i.e., the idea that firms that chase subsidies are systematically under- or over-performers – is not a significant issue.

3.2.5 Political Data

I construct my election-based instruments using CQ Voting and Elections Collection data. The majority of these gubernatorial elections occur on the same four-year cycle, as the years 2006 and 2010 alone account for 71 of the 114 elections held between 2003 and 2012. Of these 114 elections, 5 involved an incumbent governor losing a reelection bid while 59 involved the incumbent not running for reelection, for a total of 64 cases of gubernatorial change and 50 cases of incumbents keeping power. I observe a tight race (a race in which the margin of victory was narrower than four percent) in 17 elections. Three tight races involved the incumbent losing; eleven involved the incumbent not running for reelection; and the remaining three involved the incumbent winning by a narrow margin.

3.3 Results

3.3.1 First Stage Instrumental Variables Regressions

The three columns in Table 3.6 correspond to regressions run defining the subsidy variable in three ways: (1) all subsidies received, (2) the subset of tax breaks, and (3) the subset of grants. Assets and R&D (not shown in Table 3.6) are significant predictors of both the receipt of a subsidy as well as the magnitude of a subsidy if received, across all three subsidy sets. The fact that larger firms are more likely to receive a 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.

The estimated coefficient on the election-year indicator variable is negative and significant for tax breaks in both the indicator and dollar value regressions, while it is insignificant in all other specifications. Put another way, in election years the likelihood of receiving a tax break and the expected magnitude of these tax breaks both decrease; however, there is no such effect for cash grants or when considering subsidies as a whole. There are political risks to providing tax breaks that a political opponent can take advantage of as part of a campaign, and so politicians may be less likely to provide such abatements leading up to an election unless there is a compelling reason to do so. One such reason may be a tight race, in which it may be politically expedient to provide benefits to firms in exchange for support in the short-term regardless of long-term risks; this is supported by the strong positive and significant indicator on the tight race indicator variable across all subsets of subsidies. The combined effect of the coefficients on the election-year and tight-race dummies is statistically significantly positive in all specifications.

I also find that the frequency with which within-industry competitors' receive subsidies is a significant positive predictor of receiving a subsidy, while state-level competitors' subsidies are a significant predictor of receiving tax breaks but not grants. With respect to subsidy dollar amounts, aggregate industry and state subsidy dollar amounts are positive predictors of the value of a subsidy received when considering the set of all subsidies or the set of tax breaks only, but negative predictors of the value of a cash grant received. This could be due to the nature of the two types of subsidies. Tax breaks represent future lost revenues, which may be subject to a discount factor in the eyes of the regulating body and/or unaccounted for in future budgets. By contrast, cash grants come out of a present-period budget. When more subsidy dollars have already been spent on competitor firms there are fewer incremental dollars remaining to award.

3.3.2 Baseline Probit Estimations

The main argument in favor of the partially-observed bivariate is that it allows me to disentangle firms' willingness to engage in fraud from the conditional likelihood of detection. I demonstrate that this is necessary by first carrying out a standard probit estimation using all of my $\{X, Z_1, Z_2\}$ variables (i.e., all of the variables that appear in *either* my cheat or caught equations). Table 3.7 presents the results. We can see from Table 3.7 that the subsidy indicator is negative and significant, for both tax breaks and for subsidies as a whole, while the subsidy dollar value variable is statistically insignificant. This result suggests that firms that receive subsidies are caught engaging in fraudulent activity less frequently; while consistent with prior literature on the effects of regulatory capture, on its own the result in Table 3.7 is not sufficient to determine whether firms are engaging in fraudulent activity more frequently versus simply being conditionally detected by regulators and third parties with lower frequency. To disentangle these two portions, I therefore turn to the partially-observed bivariate probit.

3.3.3 Bivariate Probit Estimations

Table 3.8 presents results for the main partially-observed bivariate probit estimations. For tax breaks and the whole sample, the subsidy indicator is a significantly positive predictor of the probability that firms choose to engage in fraud (i.e., firms that receive subsidies are more willing to engage). The subsidy indicator, however, is insignificant in both the cheat and caught equations when considering only grants. This supports the idea that tax breaks and grants are distinct classes of subsidies that lead to different behavior by firms. Computing marginal effects at the means of variables, I find that a 1 percentage point increase in the likelihood of a firm receiving a subsidy leads to the average firm engaging in fraud with probability 0.31% (0.24%) higher when considering all subsidies (tax breaks) and being conditionally caught engaging with probability 0.92% lower when considering all subsidies (no difference for tax breaks). These findings suggest that regulatory capture may exist across different types of subsidies, but that the effect of capture may be more powerful for tax breaks. In additional tests (untabulated), I consider a specification that includes both tax break-specific and grant-specific indicators and dollar values. My findings are unchanged: the tax break indicator is significant and positive in the cheat equation while the grant indicator is insignificant in both cheat and caught equations.

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 is consistent with the idea that above a certain level, third-party watchdogs have a nontrivial effect. The result 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 for these specifications are essentially the same whether I use 10, 15, or 25 percent as my cutoff. In all three cases, all coefficients maintain their signs and significance at the 5% level. 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,[13] 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.

[13]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.

In all specifications, 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 take advantage of the opportunity to do so.

Considering the variables hypothesized to influence only the decision to cheat (Z_1), recall that unexercised exercisable options capture short-term gain while the incentive pay variable captures a mix of short-term incentives (for example, 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. This suggests that executives may 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, and so managers with larger potential longer-term incentives may be more concerned with the risk of detection in the interim period.

Overall, the results of the bivariate probit estimations provide additional information over the baseline probit presented in Table 3.7 with respect to the mechanism by which subsidized firms attain different fraud outcomes. Taken together these three tables suggest that firms with monetary linkages to government officials tend to engage in fraud more frequently but get conditionally caught less frequently, and that the net effect is that these firms are caught less overall. Additionally, while the amount of the subsidy does not seem to affect the unconditional likelihood of being caught, the subsidy amount does influence each individual component (cheating and conditional detection).

3.4 Managerial Compensation

I consider alternate specifications of my empirical tests. Since compensation influences managers' incentives, I test whether there is an interaction effect between subsidies received and my compensation variables in the cheat equation. Positive coefficient values on such terms could indicate that a subsidized firm tends to take advantage of its political pull more when the firm's executives have more at stake financially, while negative coefficient values might suggest that executives tread carefully when they have more of a personal stake in the firm. Because the managerial compensation variable affects only the cheat equation, the detection ("caught") equation remains unchanged.

Results for specifications that include an interaction term between the subsidy indicator and log option compensation are presented in Table 3.9. In all cases, the coefficient on the interaction term is negative and significant in all cases. When instead using an interaction term between the subsidy indicator and incentive pay, I obtain a significant negative coefficient on the interaction only in the case of tax breaks; the coefficient on the interaction term in the all-subsidies and grants cases is insignificant. This result suggests that subsidized firms may engage in fraud less when long-term incentives are at stake, relative to non-subsidized firms. Otherwise, coefficients in all three specifications for each of the two interaction terms have a similar sign and significance level to the base specification, and so I tabulate only the main coefficients of interest.

3.5 Conclusion

I estimate the impact of firm-level government subsidies on firms' decisions to engage in fraud as well as the conditional likelihood that cheating firms will be caught. I find that on the whole, the receipt of a 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 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. Firms that receive tax breaks engage in fraudulent activity more frequently on average, while firms that receive large grants or discounts do not on average engage any differently from un-subsidized firms. 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 contributions to the academic literature are to provide a new proxy for regulatory capture, to document the relation between the politician-to-firm monetary flow and corporate fraud. Although it is unlikely that corporate subsidies will significantly lessen, a greater understanding of subsidies' potential indirect consequences could lead to more well-designed tax abatements and grants. In particular, subsidy awards could be tied to stronger provisions for negation if wrongdoing is detected or promises are broken. Such provisions could have greatly aided Washington state, which in 2013 granted Boeing the single largest tax break ever given in the United States, valued at $8.7 billion. By 2016, Boeing had reduced nearly 5% of jobs in the state,[14] despite vague – and, more importantly, unenforceable – promises to "maintain and grow" employment in Washington. Boeing is also, as of this writing, under investigation for accounting irregularities with respect to the 787 airplane; the state of Washington has heavily subsidized the 787 program since 2003, with billions of dollars in tax exemptions specifically mentioning the 787. Uncoincidentally, early 2016 also saw Washington state pass a law in compliance with GASB Statement No. 77.[15] Doing so even three years earlier could have potentially saved billions of dollars in public funds.

[14] http://www.reuters.com/article/us-boeing-taxbreaks-idUSKCN0VE2LS

[15] http://www.theolympian.com/opinion/editorials/article53782210.html

Appendix A

Subsidy Classification

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 consider two classes of subsidies: reductions to governmental revenue (tax breaks) and increases in governmental spending (non-tax-breaks, hereafter referred to as "grants").

Below I provide a table of GJF's subsidy categories and how I classify each one.

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 B

Large Subsidies

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

B.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[1] 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)[2].

[1] http://bbjtoday.com/blog/boeings-tax-break-how-8-7-billion-adds-up/26977

[2] http://budgetandpolicy.org/schmudget/proposed-boeing-tax-breaks-should-include-accountability-me

B.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 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.*

B.3 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:

> *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 to the following terms:[3]

- 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.

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

- 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.[4]

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

Appendix C

Subsidies by State, 2004-2012

The table below presents the total number of subsidies awarded by state cumulatively between 2004 and 2012. Note that the numbers below represent subsidies to firms that enter my sample; the actual number of subsidies in the GJF database for these states and years is significantly larger.

State	# Subsidies		State	# Subsidies
Alaska	59		Mississippi	354
Alabama	767		Montana	16
Arkansas	393		North Carolina	1719
Arizona	1089		North Dakota	27
California	1215		Nebraska	234
Colorado	145		New Hampshire	50
Connecticut	143		New Jersey	317
District of Columbia	8		New Mexico	195
Delaware	214		Nevada	150
Florida	785		New York	4485
Georgia	80		Ohio	1442
Hawaii	24		Oklahoma	1757
Iowa	611		Oregon	953
Idaho	44		Pennsylvania	554
Illinois	1236		Rhode Island	120
Indiana	1159		South Carolina	286
Kansas	221		South Dakota	20
Kentucky	1205		Tennessee	1578
Louisiana	2591		Texas	2133
Massachusetts	267		Utah	415
Maryland	723		Virginia	378
Maine	1027		Vermont	54
Michigan	2728		Washington	2234
Minnesota	109		Wisconsin	194
Missouri	448		West Virginia	219
Federal Subsidies	30126			

Appendix D

Empirical Variables

Below I provide a table of variables used in the empirical estimations.

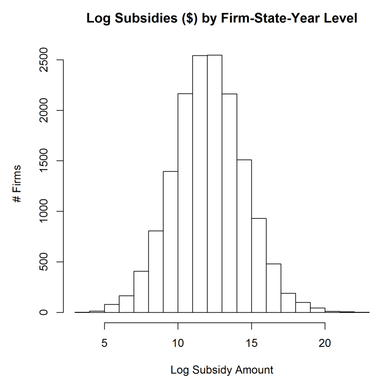
Variable	Description
Received subsidy	Equals 1 if firm received subsidy in past three years
Subsidy dollar amount	Sum total of dollar value of subsidies received in past three years
Assets	Log of firm assets at year-end
R&D	Log of firm's annual research and development
Qui tam	Equals 1 if whether firm is in an industry in which whistleblower lawsuits are possible
Leverage	Measured as $\frac{\text{Debt+Liabilities}}{\text{Assets}}$
Return on assets	Ratio of firm annual returns to firm assets
Abnormal returns	See Equation (3.3) for computation. Returns data is taken from CRSP.
Analyst estimates	Number of distinct analysts who provide estimates for the firm in a given year
Incentive pay	Total compensation minus base salary; total compensation measured as TDC1 from Compustat. Variable is averaged across all firm executives for whom data is available in Equilar or Execucomp
Unexercised exercisable options	Value of all outstanding unexercised but exercisable options, averaged across all firm executives for whom data is available in Equilar or Execucomp
Election year	Indicator for election year
Tight race	Equals 1 if gubernatorial race was decided by less than 4%
Incumbent did not run	Equals 1 if incumbent governor did not re-run for election

Tables and Figures

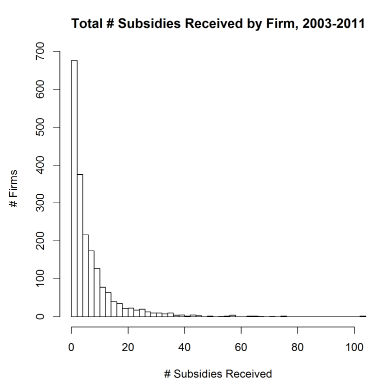
Figure 3.1: Distribution of Number of Subsidies – All Firms

This figure presents three histograms pertaining to subsidy receipt; I use the set of all firms who receive a subsidy at least once. Panel A provides the distribution of the log-dollar value of subsidies an individual state provides to a firm, conditional on providing at least one subsidy to that firm. Panel B provides the distribution of the overall number states from which individual firms received subsidies between 2003 and 2011, while Panel C provides the distribution of the number of states from which individual firms received subsidies at the firm-year level for each year between 2003 and 2011. For example, if Company X received three subsidies from Texas and two from Tennessee in 2005, one from North Carolina in 2006, and one from Texas in 2007, and no other subsidies, the histogram in Panel B would reflect Company X receiving 4 subsidies, while the histogram in Panel C would reflect three data points for Company X, with values equal to 2, 1, and 1 (corresponding to 2005, 2006, and 2007). Bin width is 1 in Panels A and C, and 2 in Panel B.

(a) Within-State



(b) Overall Frequency



(c) By Firm-Year

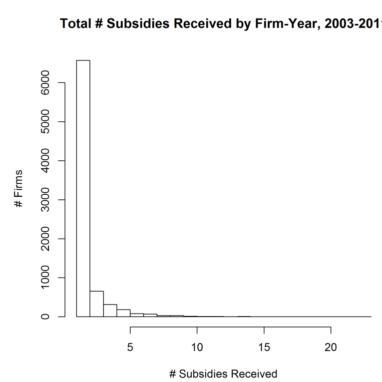


Table 3.1: Correlations Between Regressors

This table presents correlations between all independent variables used in my regression sample. The subsidy variable presented in this table is the actual version of the variable rather than the instrumented-for version that I use in my bivariate probit estimations, and reflects subsidy awards over a three-year (present and two-year lag) timeframe. Pearson correlations are above the diagonal, while Spearman correlations are below.

	Log assets	Log R&D	Subsidy	Qui tam	Lev.	ROA	Returns	Analyst	Options	Inc. pay
Log assets		0.315	0.394	-0.007	0.143	0.103	-0.041	0.486	0.015	0.011
Log R&D	0.218		0.245	0.150	-0.140	0.011	-0.014	0.293	-0.014	-0.003
Subsidy	0.391	0.236		-0.055	0.010	0.086	-0.015	0.185	-0.006	-0.053
Qui tam	-0.010	0.139	-0.055		0.026	-0.034	-0.018	0.053	-0.005	0.007
Leverage	0.224	-0.142	0.040	0.010		-0.215	-0.012	-0.168	0.014	0.104
ROA	0.078	0.040	0.094	-0.013	-0.338		0.077	0.193	0.014	-0.049
Returns	-0.019	-0.005	0.007	-0.030	-0.083	0.139		-0.047	0.037	0.049
Analyst	0.491	0.260	0.197	0.061	-0.148	0.259	-0.023		-0.003	-0.069
Options	0.138	0.086	-0.013	0.072	-0.037	0.250	0.243	0.178		0.257
Incentive pay	0.432	0.161	0.114	0.059	0.125	0.052	0.055	0.282	0.571	

Table 3.2: Subsidy Data Summary Statistics

Panel A: Subsidy Size

This panel presents summary statistics on subsidies received for all firms with assets greater than $750 million during the sample period 2005-2011. Subsidies are aggregated to the firm-year level. Separate statistics are provided for the set of all subsidies, as well as for tax breaks and for cash grants.

Type of Subsidy (N)	25th %ile	Median	75th %ile	Mean	St. Dev.
All Subsidies (4759)	182	1049	5000	21012	184745
Tax Breaks (3445)	165	810	3410	17506	133005
Grants (3241)	116	553	2789	12250	173052

Panel B: Total Subsidy Awards by Year

This panel presents a year-by-year breakdown of the total number of subsidies awarded to firms with assets $750 million or higher between 2003 and 2011. It also presents the aggregate dollar value of subsidies awarded to firms with assets $750 million between 2003 and 2011; this represents between 15 and 20 percent of subsidies by dollar value, depending on the year, awarded to all firms (i.e., public or private and of any size) over this time period. The first two columns present the tax breaks and grants, while the right two columns present the total dollar value (in millions) of each type of subsidy awarded by year.

Year	Tax Breaks (#)	Grants (#)	Tax Breaks ($M)	Grants ($M)
2003	667	1,095	4,630	823
2004	972	1,246	3,380	1,081
2005	1,239	1,183	1,769	1,369
2006	1,547	1,240	1,435	1,032
2007	2,283	2,408	2,783	6,952
2008	2,731	6,087	3,275	1,801
2009	2,647	7,224	5,704	4,220
2010	3,157	3,747	5,658	7,683
2011	3,173	3,194	5,475	2,575

Table 3.3: Breakdown of Fraud Variables by Year

This table presents the total number of AAERs and investor lawsuits that appear in my database for each year of the sample period 2005-2011. The number of AAERs and lawsuits that appear in the final regression sample is somewhat smaller due to other data cuts, but yearly frequencies remain relatively consistent.

Year	AAERs	Lawsuits	Overlap
2005	59	67	14
2006	35	85	7
2007	29	82	4
2008	12	46	3
2009	5	42	1
2010	3	53	0
2011	1	28	0

Table 3.4: Sample Selection

Start: All COMPUSTAT firms with assets > $\$750M$, 2005-2011	30,533
Less: Banks and utilities	(12,033)
Less: Missing executive compensation data	(10,007)
Less: Missing analyst forecast data	(758)
Less: Missing election data	(532)
Less: Missing returns data	(58)
Less: Missing leverage	(45)
Less: Missing industry identifier	(52)
Final Sample Size	7,048

Table 3.5: Summary Statistics for Non-Subsidy Variables - Final Estimation Dataset

This table presents summary statistics for control variables used in my bivariate probit estimations. Summary statistics are provided for my final regression dataset ($N = 7,048$; see Table 3.4 for details on how I arrive at this number) rather than for the universe of all available data in COMPUSTAT. In particular, note that all firms in my sample must have assets of at least $750 million. Assets and R & D are in millions, while managers' unexercised exercisable options and incentive pay are measured in thousands.

Variable	Mean	25th %ile	Median	75th %ile	Std. Dev.
Assets	6,209	944	1,880	4,669	17,731
R&D	149	0	0	59	665
Leverage	0.839	0.533	0.784	1.046	0.515
Qui tam industry	0.113	-	-	-	-
Regulated firm	0.209	-	-	-	-
ROA	0.061	0.016	0.066	0.121	0.327
Annual returns	0.131	-0.188	0.059	0.323	0.714
# analyst estimates	10.039	5	8	14	7.013
Unexercised Exercisable Options	1,128	0.7	6	30	8,040
Incentive Pay	1,102	5	11	28	3,869

Table 3.6: 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 all regressions use 10,801 observations. 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.

Panel A: Subsidy Indicator

	All Subsidies	Tax Breaks	Grants
Log assets	0.132***	0.124***	0.100***
	(0.003)	(0.003)	(0.003)
Log R&D	0.026***	0.016***	0.028***
	(0.002)	(0.002)	(0.002)
Election year	−0.017	−0.038***	0.009
	(0.013)	(0.013)	(0.012)
Log other industry subsidies (#)	0.048***	0.054***	0.024***
	(0.004)	(0.004)	(0.004)
Log other state subsidies (#)	0.015***	0.008*	0.004
	(0.005)	(0.004)	(0.004)
Incumbent didn't rerun	0.001	0.016	−0.019
	(0.017)	(0.015)	(0.015)
Tight race	0.096***	0.128***	0.086***
	(0.023)	(0.021)	(0.020)
Adjusted R^2	0.185	0.182	0.155

Panel B: Subsidy Dollar Value

	All Subsidies	Tax Breaks	Grants
Log assets	2.002***	1.688***	1.363***
	(0.045)	(0.041)	(0.039)
Log R&D	0.426***	0.234***	0.434***
	(0.025)	(0.022)	(0.021)
Leverage	−0.470***	−0.247**	−0.249***
	(0.109)	(0.099)	(0.094)
Log other industry subsidies ($)	0.124***	0.081***	−0.047*
	(0.022)	(0.013)	(0.012)
Log other state subsidies ($)	0.052**	0.098***	−0.027**
	(0.022)	(0.013)	(0.012)
Election year	−0.207	−0.532***	0.206
	(0.174)	(0.157)	(0.151)
Incumbent didn't rerun	0.139	0.492**	−0.310*
	(0.215)	(0.194)	(0.186)
Tight race	1.272***	1.646***	0.767***
	(0.296)	(0.267)	(0.256)
Adjusted R^2	0.226	0.192	0.175

Table 3.7: Baseline Probit Results

This table presents results from a standard probit model that estimates the relation between firms' engaging in financial fraud and firms' receipt of subsidies, controlling for a variety of other factors outlined in the body of the paper. The dependent variable in all cases is an indicator for AAERs and investor lawsuits. 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.

Probit Estimation Results

	All Subsidies	Tax Breaks	Grants
Constant	−1.854	−3.083***	−4.572***
	(1.17)	(0.698)	(1.309)
Received subsidy	−2.953**	−2.141**	−3.75**
	(1.074)	(0.811)	(1.188)
Log subsidy amount	0.205	0.06	−0.02
	(0.141)	(0.09)	(0.115)
Qui tam industry	0.219*	0.202*	0.211*
	(0.096)	(0.096)	(0.096)
Log assets	−0.043	0.147	0.386.
	(0.197)	(0.11)	(0.213)
Leverage	−0.045	−0.114	−0.13
	(0.132)	(0.121)	(0.122)
ROA	−0.187	−0.164	−0.185
	(0.317)	(0.315)	(0.314)
Annual returns	−0.092	−0.083	−0.083
	(0.091)	(0.09)	(0.089)
Number of analyst estimates	0.017**	0.016**	0.016**
	(0.006)	(0.006)	(0.006)
Log R&D	0.004	0.035	0.124.
	(0.047)	(0.022)	(0.064)
Abnormal leverage	−0.001	−0.001	0
	(0.007)	(0.007)	(0.007)
Abnormal ROA	0.034	0.046	0.044
	(0.156)	(0.155)	(0.155)
Abnormal returns	−0.209	−0.227	−0.205
	(0.238)	(0.238)	(0.237)
Log options	0.034*	0.032*	0.033*
	(0.015)	(0.015)	(0.015)
Log incentive pay	−0.021	−0.018	−0.021
	(0.022)	(0.022)	(0.022)

Table 3.8: 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 ($N = 7,048$)

	(Cheat Equation)	(Caught Equation)
Constant	-5.686***	0.095
	(0.871)	(0.984)
Received subsidy	4.704***	-3.009**
	(1.706)	(1.401)
Qui tam industry	0.243**	0.386***
	(0.109)	(0.114)
Log assets	0.532***	-0.426**
	(0.139)	(0.175)
Log subsidy amount	-0.411***	0.32**
	(0.137)	(0.14)
Leverage	-0.72***	0.4**
	(0.226)	(0.188)
ROA	-1.035**	1.049**
	(0.427)	(0.454)
Annual returns	-0.273***	0.215**
	(0.105)	(0.106)
Number of analyst estimates	0.025***	0.007
	(0.008)	(0.008)
Log R&D	-0.08**	0.031
	(0.039)	(0.042)
Log options	0.066***	
	(0.015)	
Log incentive pay	-0.007	
	(0.017)	
Abnormal leverage		0.014***
		(0.005)
Abnormal ROA		-0.156
		(0.17)
Abnormal returns		-0.831**
		(0.339)
Log-likelihood	-448.90	

Panel B: Tax Breaks ($N = 7,048$)

	(Cheat Equation)	(Caught Equation)
Constant	-3.764***	-3.118***
	(0.384)	(0.655)
Received subsidy	3.857***	-1.457
	(1.295)	(1.151)
Qui tam industry	0.217*	0.429***
	(0.118)	(0.124)
Log assets	0.223***	0.098
	(0.066)	(0.104)
Log subsidy amount	-0.247***	-0.052
	(0.09)	(0.101)
Leverage	-0.565***	0.233*
	(0.197)	(0.134)
ROA	-0.858***	1.024**
	(0.312)	(0.512)
Annual returns	-0.272**	0.263**
	(0.106)	(0.114)
Number of analyst estimates	0.028***	0.006
	(0.009)	(0.007)
Log R&D	-0.133***	0.122***
	(0.039)	(0.031)
Log options	0.07***	
	(0.014)	
Log incentive pay	-0.014	
	(0.013)	
Abnormal leverage		0.017**
		(0.008)
Abnormal ROA		-0.114
		(0.159)
Abnormal returns		-0.875***
		(0.211)
Log-likelihood	-448.38	

Panel C: Grants ($N = 7,048$)

	(Cheat Equation)	(Caught Equation)
Constant	-3.742***	-8.403***
	(0.752)	(1.278)
Received subsidy	-0.473	-2.573
	(1.459)	(1.8)
Qui tam industry	0.275**	0.6***
	(0.11)	(0.149)
Log assets	0.188	1.065***
	(0.119)	(0.219)
Log subsidy amount	0.01	-0.781***
	(0.114)	(0.181)
Leverage	-0.257*	-0.063
	(0.143)	(0.157)
ROA	-0.501	1.658***
	(0.385)	(0.503)
Annual returns	0.054	-0.171
	(0.108)	(0.109)
Number of analyst estimates	0.025***	-0.005
	(0.007)	(0.008)
Log R&D	-0.099**	0.621***
	(0.048)	(0.112)
Log options	0.017	
	(0.011)	
Log incentive pay	0.026	
	(0.02)	
Abnormal leverage		-0.073***
		(0.018)
Abnormal ROA		-0.572***
		(0.141)
Abnormal returns		-0.302
		(0.209)
Log-likelihood	-446.33	

Table 3.9: Bivariate Probit Results with Unexercised Option Interactions

This table presents a selection of results from the partially observed bivariate probit specifications that include an interaction term between managerial compensation and the subsidy variables in the cheat equation. The dependent variable in all cases is an indicator for AAERs and investor lawsuits. Panel A uses incentive pay as the maangerial compensation variable while Panel B uses unexercised but exercisable options. Because most coefficients are qualitatively similar to the main specification, I include only the main three coefficients of interest **from the caught equation** for each type of subsidy below: the coefficients on the subsidy indicator, subsidy amount, and interaction term indicator. One asterisk (*) denotes significance at the 0.1 level, two (**) the 0.05 level, and three (***) the 0.01 level.

Panel A: Incentive Pay

	All Subsidies	Tax Breaks	Grants
Subsidy indicator	4.464**	5.007***	1.24
	(1.774)	(1.480)	(8.87)
Log subsidy amount	−0.386***	−0.368***	0.117
	(0.131)	(0.142)	(0.895)
Subsidy indicator · Log incentive pay	−0.057	−0.186***	-0.269
	(0.046)	(0.050)	(0.432)

Panel B: Options

	All Subsidies	Tax Breaks	Grants
Subsidy indicator	3.559**	5.683***	-3.569
	(1.743)	(1.655)	(2.437)
Log subsidy amount	−0.201	−0.377**	1.462***
	(0.142)	(0.168)	(0.497)
Subsidy indicator · Log options	−0.294***	−0.378***	−1.191***
	(0.067)	(0.073)	(0.344)

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