

Government Subsidies and Corporate Misconduct

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ABSTRACT

I study whether firms that receive targeted U.S. state-level subsidies are more likely to subsequently engage in corporate misconduct. I find that firms are more likely to engage in misconduct in subsidizing states, but not in other states that they operate in, after receiving state subsidies. Using data on both federal and state enforcement actions, and exploiting the legal principle of dual sovereignty for identification, I show that this finding reflects an increase in the underlying rate of misconduct and that this increase is attributable to lenient state-level misconduct enforcement. Collectively, my findings present evidence of an important consequence of targeted firm-specific subsidies:

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nonfinancial misconduct that potentially could impact the very stakeholders subsidies are ostensibly intended to benefit.

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1. *Introduction*

State governments award billions of dollars in targeted subsidies each year to individual firms in the form of direct cash grants, tax abatements, or tax rebates. These subsidies are unique among sources of firm financing in that they create long-term political and economic connections between subsidizing governments and subsidized firms, which, in turn, may potentially yield negative externalities affecting local stakeholders. In this paper, I examine a key potential subsidy-related externality: subsidized firms (mis)treatment of local stakeholders, as evidenced by cases of corporate misconduct within subsidizing states. Whereas existing research studies the effect of subsidies and awarding governments' behavior on job creation (Slattery and Zidar [2020], De Simone, Lester, and Raghunandan [2024]), little prior research examines subsidy recipients' behavior in subsidizing jurisdictions more generally. The goal of this paper is therefore to study another important potential consequence of subsidies for the taxpayers bearing these subsidies' cost. Understanding whether subsidized firms treat local stakeholders differently from nonsubsidized firms contributes toward a more complete assessment of firm-specific subsidies, especially in light of the growing prevalence of subsidies as a policy tool (Slattery and Zidar [2020]) and increasing concerns about their effectiveness (Jensen and Malesky [2018], Slattery [2021]).

State officials obtain political benefits (costs) when subsidies awarded under their watch succeed (fail) in creating jobs (Jensen and Malesky [2018]). For instance, successful subsidies are frequently cited during political campaigns as part of these officials' track records with respect to job creation (Slattery [2021]).¹ State politicians' desires to observe—and claim credit for—job creation may make them more willing to ignore recipient firms' efforts to reduce costs via noncompliance with stakeholder-focused regulations.² In turn, subsidy recipients may anticipate lower scrutiny toward their broader corporate conduct in the subsidizing state, meaning

¹ As an example, during his 2014 reelection campaign, Florida governor Rick Scott frequently highlighted job growth as a result of subsidies his administration granted to companies such as Hertz, Boeing, and Amazon during his first term (Bousquet [2014]).

² For example, in 2016, Texas provided McKesson Corporation with \$9.75 million in job creation subsidies. Texas subsequently investigated McKesson for its role in facilitating the opioid crisis (Malewitz and Walters [2017]) but, unlike other (nonsubsidizing) states, chose not to file suit against McKesson—the latter being a decision that at least one author has attributed to the presence of the subsidy (Jensen [2019]).

that the state-level enforcement costs of engaging in corporate misconduct are lower for subsidized firms relative to nonsubsidized firms. Lower enforcement costs should, in turn, lead to a greater willingness by subsidized firms to underinvest in compliance, which, in turn, results in misconduct. I therefore predict that firms will engage in misconduct in subsidizing states, representing a negative externality of subsidies, more frequently after receiving a targeted subsidy.

To test the paper's hypothesis, I construct a panel of firm-state-year observations spanning the period 2004–16. I identify subsidies using Subsidy Tracker, an extensive data set published by the nonprofit organization Good Jobs First (GJF). I measure corporate misconduct using data on federal and state enforcement actions from a database called Violation Tracker, also published by GJF. Violation Tracker contains comprehensive information on penalties issued by over 50 federal agencies such as the Environmental Protection Agency (EPA), Occupational Safety & Health Administration (OSHA), and Department of Justice (DOJ). Violation Tracker also contains limited information on enforcement actions by state-level agencies for similar types of violations. The ability to observe federal enforcement actions as a proxy for the true violation rate bypasses concerns that economic and political connections between the firm and the state government may affect enforcement practices by state-level agencies. For example, a firm may seek to cut costs in a factory by underinvesting in maintenance or compliance with safety standards. Although a state-level subsidy may reduce the likelihood that the firm faces sanctions from the state government for this behavior, the subsidy is plausibly exogenous to the likelihood of facing a federal sanction from OSHA.

My research design employs a rich set of fixed effects at the firm-state and state-industry-year level to rule out potential confounds. Firm-by-state fixed effects ensure that my results are not driven by a mechanical correlation between a firm's level of economic activity in a given state and its violation rate (e.g., the case where a firm with more factories may simply have a greater chance of incurring a violation in one of them). These fixed effects also rule out unobservable firm or geographic characteristics as possible explanations for my results. State-industry-year fixed effects ensure that my results are also not driven by temporal and/or state shifts in enforcement or economic activity within industries.

I find that firms receiving targeted subsidies from state governments are more likely to subsequently be penalized by the federal government for corporate misconduct committed in subsidizing states (but not in other states in which the firm operates). The plausible exogeneity of federal enforcement to state-level connections implies that this finding represents an increased underlying level of corporate misconduct committed by these companies rather than any shifts in enforcement. Moreover, the increased level of misconduct occurs while the subsidy-awarding state administration is still in power, consistent with firms perceiving there to be more lenient enforcement by the authorities that granted the subsidy in the first place.

These results account for potential shifts in firms' business presence across states and time and are robust to several alternative specifications.

A possible explanation for my findings is that subsidies and corporate misconduct are both consequences of firms' political contributions to state-level officials. However, I find no relation between firms' or their CEOs' contributions to in-state politicians and subsequent violations. I also find no cross-sectional difference in the link between subsidies and misconduct based on the presence of contributions to state politicians. The latter result suggests that the relation between subsidies and corporate misconduct does not vary as a function of the firm's presubsidy political activity, even if that presubsidy political activity may have helped the firm win the subsidy in the first place (Aobdia, Koester, and Petacchi [2021]). These findings highlight the importance of distinguishing ex ante attempts to develop political connections from ex post political connections arising from subsidies.

I next study why subsidized firms engage in corporate misconduct more frequently. One potential mechanism is lenient state-level enforcement. State-level leniency should lower the marginal costs of misconduct, which, in turn, may make a firm more willing to take on the higher expected costs of federal enforcement arising from a higher underlying misconduct rate. To test this possibility, I use a subsample of states where data on penalties issued by both state and federal enforcement agencies for corporate misconduct are available. I rely on the legal principle of *dual sovereignty* for identification. Dual sovereignty represents the lone exception to American double jeopardy laws, allowing the U.S. federal government and state governments to separately sanction an individual or firm for the same or similar conduct on the grounds that both the country and individual states are sovereign entities. In the misconduct setting, the principle of dual sovereignty allows state and federal enforcement agencies to concurrently penalize the same underlying action. Comparing concurrent instances of federal and state enforcement actions therefore allows me to separate the effect of subsidies on a firm's proclivity to engage in misconduct from the effect of subsidies on state-level enforcement.

I find a positive relation between the likelihood of facing federal enforcement for misconduct and the likelihood of facing state enforcement. This result is consistent with the existence of dual sovereignty in practice and rules out a de facto substitution effect wherein state enforcement agencies are less likely to bring a case against a firm that is also under federal investigation (or vice versa). However, I find that subsidized firms facing federal penalties for corporate misconduct are less likely to also face state penalties, relative to unsubsidized firms. I interpret this result as evidence of state-level leniency for subsidy firms. In sum, my findings suggest that (i) the increase in federal enforcement actions subsequent to state subsidy receipt reflects an increase in the underlying incidence of misconduct; and (ii) the latter increase may be attributable to lenient state-level enforcement.

Although my research design and results thus far mitigate potential endogeneity concerns, I conduct an additional test to ensure that my results are

not reflective of unobservable time-varying firm characteristics driving both firms' self-selection into seeking subsidies as well as their compliance behavior. Specifically, I use merger activity in which an acquirer exogenously "inherits" a political connection in a certain state by purchasing a target firm that previously received a subsidy under the existing state administration. Underlying this approach is the assumption that mergers are primarily motivated by target firm fundamentals and acquirer-target operational synergies, with political ties attributable to subsidies playing a minor or no role. To verify this assumption, I first estimate models of merger prediction and the takeover premium. If mergers were affected by acquiring firms' desire to obtain access to target firms' subsidies, I should observe that target firms with subsidies are more likely to be acquired and/or that acquirers pay a higher price in these mergers. However, I find that subsidy receipt by target firms predicts neither a higher likelihood of being acquired nor any takeover premium, validating the merger-based approach.

I then estimate a difference-in-differences model comparing postmerger activity in states that had awarded a subsidy to the target to a control sample composed of the acquiring firm in states that did not award a subsidy to the target.³ Because my control sample is composed of the same firm in other states during the same time period, any results I find in this analysis cannot be driven by time-variant firm characteristics—including changes to the firm resulting from the merger. Using these firm-state-years in five- and seven-year windows around the merger year (i.e., $t - 2$ to $t + 2$ and $t - 3$ to $t + 3$), I find that acquiring firms are more likely to engage in misconduct in states where they inherit political connections via a subsidy relative to states where they do not inherit a subsidy.

To provide further evidence of the mechanism underlying the paper's main results, I consider the political orientation of the state's top enforcement official, the attorney general. State attorneys general are directly elected in 43 out of 50 states and in many cases go on to run for higher office, including governor. Hence, an attorney general of the opposing party to the governor may politically benefit from enforcement cases against firms connected to the governor (Nolette [2014]). Thus, when the attorney general and governor are politically unaligned, postsubsidy state-level enforcement may not weaken because the in-state political costs of leniency are higher. Consistent with this argument, I find that the relation between subsidy receipt and misconduct attenuates when the attorney general and governor are of different political parties. As an alternative specification, I consider instead politically unaligned elected state commissioners (of labor, agriculture, and natural resources), as these commissioners oversee state-level agencies that enforce labor and environmental laws. I find similar results.

³ As an example, suppose Company A acquires Company B. If Company B had a premerger subsidy in California but not Texas, then Company A inherits the California subsidy and the treatment firm-state is Company A in California while the control is Company A in Texas.

My study makes three main contributions. First, prior work on the link between political connections and corporate misconduct in the U.S. focuses on *financial* misconduct (e.g., Correia [2014], Heese [2019]). I extend this literature by studying how such connections affect nonfinancial misconduct. Moreover, prior studies that document a relation between political connections and enforcement outcomes only observe endogenously determined enforcement actions. Hence, these studies infer weaker enforcement as the mechanism underlying their results but cannot empirically identify whether underlying violation rates also shift. The exogeneity of federal enforcement to state political connections as well as dual sovereignty enables me to empirically identify the mechanism underlying the link between subsidies and misconduct, that is, lower deterrence from state-level enforcers resulting in a higher underlying violation rate.

Second, I contribute to an emerging literature that studies how firms' financial incentives relate to nonfinancial misconduct. Recent work finds that firms' short-term financial incentives and constraints increase the likelihood of misconduct (Cohn and Wardlaw [2016], Heese and Perez-Cavazos [2020], Raghunandan [2021]). I extend this literature by documenting how subsidies—representing both a source of firm financing and a political connection to government—relate to firms' decisions to engage in corporate misconduct.

Third, I contribute to a growing body of research on the effects of targeted subsidies awarded to individual firms. Prior studies examine whether subsidies attain their stated goals of boosting employment and wages, finding mixed results (e.g., Pew Charitable Trusts [2017], Slattery and Zidar [2020]). However, in recent years, subsidy recipients have come under scrutiny from unions, politicians, and the media regarding their broader behavior toward the local communities providing the subsidies.⁴ Given such scrutiny, and the ongoing debate over the merits of targeted subsidies, it is crucial to more comprehensively understand how subsidies affect the taxpayers bearing their costs. My findings suggest that subsidies have potentially adverse effects toward the local citizens that subsidies are ostensibly intended to benefit, in the form of recipients exhibiting a greater willingness to not comply with labor or environmental laws. Although I cannot speak to the extent to which the collective (direct and indirect) costs of such misconduct offset the economic benefits subsidies may accrue, at minimum my study highlights an externality of subsidies that awarding entities ought to consider.

⁴For example, in 2018 Amazon announced plans to construct a second headquarters (HQ2) in New York in exchange for \$3 billion in state and local subsidies. However, shortly after the announcement, labor unions and local politicians highlighted Amazon's track record with respect to labor relations to question the net benefit that these subsidies would provide to the local community beyond direct job creation (Goodman [2019]). This public pressure ultimately led Amazon to walk away from the subsidy and the project.

2. Background and Related Literature

2.1 SUBSIDIES

State governments frequently award subsidies to specific private-sector firms with the intent of stimulating economic growth. Subsidies are awarded at the full discretion of the state government, in the sense that the federal government has no formal say in how and when states award subsidies. The most common type of subsidies are tax breaks, including property tax exemptions or sales tax abatements explicitly designed to reduce a specific firm's tax burden in a specific location. Other methods of subsidizing companies involve direct cash payments or reimbursements for approved activities, as well as discounted access to resources. For example, in 2007 Alcoa struck a deal with New York State that allowed it to pay 25% of the market 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 United States at \$8.7 billion. In Boeing's case, key components of the subsidy package consisted of property tax exemptions, a reduction of the business-and-occupation tax rate, and funds that would reimburse the company for worker training. State-level subsidies are typically awarded through economic development agencies housed within the governor's office and, in many cases, require the explicit sign-off by the governor and/or other high-ranking state-level officials. For example, all subsidies awarded through the Texas Enterprise Fund program require the explicit approval of the Governor, Lieutenant Governor, and Speaker of the (Texas) House.

I obtain subsidy data from the Subsidy Tracker database, compiled by the nonprofit organization GJF. Subsidy Tracker contains detailed data on over 600,000 economic development subsidies.⁵ Data are at the subsidiary company level and includes information about subsidies targeted to specific firms but excludes, for example, statutory tax breaks written to benefit an entire industry.

Subsidy packages awarded by state governments frequently include clauses about benefits to consumers, workers, or the environment.⁶ It is important to note that violations of these clauses do not necessarily constitute violations of federal law. In the example of Florida's Quick Action Closing

⁵ Subsidy Tracker obtains the vast majority of its data from state government sources, primarily data posted publicly online and supplemented with Freedom of Information Act (FOIA) requests. This approach alleviates a potential concern related to Subsidy Tracker, that is, that the data could be constructed on the basis of GJF targeting certain firms.

⁶ For example, companies receiving subsidies through Florida's *Quick Action Closing Fund* must pay employees in subsidized locations at least 125% of the area-wide average private-sector annual wage. Many more such examples are available in GJF's report on "job quality" provisions included in several key subsidy programs at <http://www.goodjobsfirst.org/sites/default/files/docs/pdf/moneyforsomething.pdf>.

Fund, a firm that received a subsidy from this program and paid its employees the federal minimum wage would be violating the subsidy-specific clause, but not federal law; only if the firm paid less than the federal minimum wage would it be in violation of both.

State governments control both the design and implementation of these clauses and their enforcement—or the lack thereof.⁷ There are at least two reasons that states may not hold subsidy recipients accountable for poor nonfinancial performance, even in the presence of formal clauses. First, state administrations may have incentives to be perceived as “business-friendly” or to avoid highlighting failures within economic development policies may dominate their incentives to hold firms accountable for non-compliance with subsidy-related promises (Mattera et al. [2012]). Second, politicians may also use subsidies as a tool for their personal political gain (Jensen, Malesky, and Walsh [2015], Jensen and Malesky [2018], Slattery [2021]). Inasmuch as both of these incentives apply beyond the context of formal subsidy-specific clauses, they suggest that state officials may exhibit leniency, more broadly, toward subsidy recipients’ corporate conduct in their states.

An emerging literature studies the causes and consequences of targeted subsidies awarded to individual firms. One stream of this literature studies the determinants of subsidy receipt. For example, Slattery [2020] and Mast [2020] highlight the role of interstate competition by state governments to lure firms to relocate operations (or to prevent firms from relocating out of state). Other studies, outlined in subsection 2.3, highlight politicians’ personal political gain as a potential reason for subsidy granting. A second stream of literature focuses on the financial reporting consequences of subsidy receipt. For example, Huang [2022] finds that subsidy recipients provide more voluntary disclosure in their annual reports about operating activities, perhaps to convince subsidy grantors that they are using the funds for their stated purpose. Conversely, Pappas et al. [2024] document a link between subsidy receipt and income smoothing, arguing that this reflects subsidized firms’ desires to avoid scrutiny over their finances in light of their use of taxpayer funds. Finally, a third stream of the subsidy literature focuses on the efficiency of these subsidies with respect to their stated goals of job creation and wage creation, with mixed results. For example, Slattery and Zidar [2020] find minimal evidence of job creation resulting from large subsidies, whereas De Simone, Lester, and Raghunandan [2024] find that smaller subsidies specifically intended to boost employment or capital investment achieve those goals when paired with governmental disclosures that facilitate accountability.

⁷For example, out of 238 individual subsidy programs studied by Mattera et al. [2012], 144 either had no mechanism whatsoever to assess penalties for noncompliance with subsidy-related promises or explicit discretion over whether to enforce penalties for noncompliance (rather than an enforcement mandate).

2.2 CORPORATE MISCONDUCT

One benefit of studying nonfinancial misconduct is the frequency of enforcement; 23% of firm-years in my sample commit at least one observed violation of federal law, compared to the 1%–3% violation rate typically observed in financial misconduct studies (e.g., Dechow et al. [2011]).⁸ Nonetheless, as with all types of misconduct, a potential issue is that enforcement may not be random. Due to financial constraints, federal agencies may systematically target for investigation firms that are larger, higher-risk, or located in certain areas. However, my fixed effects design, outlined in section 3, mitigates concerns about systematic differences in enforcement rates. More importantly, I am not aware of evidence suggesting that connections to state politicians affect federal enforcement outcomes.

I obtain corporate misconduct data from Violation Tracker, also compiled by GJF. Violation Tracker contains comprehensive information on enforcement actions undertaken by the federal government as well as a handful of state-level agencies from 2000 onward. Violation Tracker primarily contains data on nonfinancial misconduct; most observations reflect violations of environmental, labor, or consumer protection laws. GJF's approach in compiling these data is to go to agencies' Web sites, bulk-download data, and then apply a matching algorithm with substantial hand-verification to match violator firms to their parent companies. Because Violation Tracker obtains its data by starting with comprehensive agency records, rather than on a firm-by-firm basis, I argue that these data are unlikely to reflect biases toward or against certain firms.

I observe the state in which misconduct was committed for most observations; observations without state data are typically firm-wide (e.g., accounting violations). Current parent-subsidy linkages are compiled by GJF, so I manually inspect all observations for which parent-subsidiary linkages are provided in order to match recipient firms to their parent companies at the time of a violation.

Common violation types include wage and hour as well as workplace safety issues, enforced by the federal Department of Labor (DOL) and state labor commissioners' offices; environmental violations, enforced by the federal EPA and state-level environmental protection offices; and consumer protection violations, enforced by the federal DOJ and Federal Trade Commission (FTC) and most commonly at the state level through attorney general lawsuits. Although federal data and state-level attorney general lawsuit

⁸ I am also unable to consider financial misconduct in this paper for two practical reasons. First, two laws enacted prior to my sample period (the 1995 Private Securities Litigation Reform Act and 1998 Securities Litigation Uniform Standards Act) prevent individual states from sanctioning firms for most types of large securities violations, instead reserving exclusive jurisdiction for the federal enforcement and court system. Second, federal securities violations are not tied to a particular state; because I conduct analyses at the firm-state-year level I am unable to map such violations to specific observations in my sample.

data are comprehensive, other state-level data are only available for a limited number of states. I outline these states in subsection 4.3.

A notable feature of the misconduct data is the low magnitude of financial penalties for most offenses, relative to larger penalties seen in financial fraud studies. For example, the average ratio of penalty dollars to total assets for firm-state-years with violations in my sample is 0.02%. Low penalty values arise for two reasons. First, in many cases, the amount that a federal agency can charge for a violation is capped by law, which restricts discretion in assessing fines that match a violation's economic impact. Second, most agencies in my sample adopt a high-investigation frequency, low-fine approach. In contrast, the SEC combines lower investigation frequency with higher penalties. Nonetheless, although direct penalties are often low, prior work highlights several indirect costs of corporate misconduct. For instance, Li and Raghunandan [2022] find that labor violations result in a higher likelihood of subsequent labor lawsuits (i.e., nonfinancial litigation risk), with the average settlement over \$10 million. Violations may also result in reputational damage. For example, Johnson [2020] shows that press releases associated with OSHA violations act as an effective deterrence tool whereas Heese, Peter, and Perez-Cavazos [2022] show that local newspaper closures lead to increases in corporate misconduct.

Low direct federal penalties may also explain why politicians may not take proactive steps to prevent misconduct. Although the indirect costs noted above may have a meaningful effect on firm behavior, it is not clear that they would affect politicians' behavior in the same way. Small-dollar violations are less likely to receive press coverage and, even when they do, may not invite the same amount of scrutiny from the press that would lead reporters to link the misconduct to the subsidy. Moreover, although the potential legal and reputational costs borne by firms caught engaging in misconduct are nontrivial, these may take several years to materialize. For instance, the average labor lawsuit studied in Li and Raghunandan [2022] takes more than three years to settle. If a politician's incentives are heavily shaped by the desire to win the next election, such indirect costs may not affect them in the same way.

Recent work uses Violation Tracker for other purposes. For example, Raghunandan [2021] documents a relation between firms' financial reporting incentives and wage theft, whereas Heese and Perez-Cavazos [2020] find that the introduction of direct flights between a firm's headquarters city and branch cities reduces the likelihood of misconduct in those cities. More generally, although an extant literature studies how political connections affect *financial* misconduct (e.g., Correia [2014], Heese [2019]), the literature on how political connections relate to *nonfinancial* misconduct is sparse. The closest study to mine in this regard is Fisman and Wang [2015], who find that Chinese firms who hire high-level government officials subsequently cut investments in workplace safety. My study differs from theirs via (i) considering the U.S. setting, which has a strong legal culture and in which the types of connections studied in Fisman and Wang [2015] are

unlikely to occur; and (ii) considering a setting (economic development subsidies) where politicians obtain political, rather than personal financial, benefits from lenient enforcement.

2.2.1. Dual Sovereignty. A key element of my empirical approach to identifying the mechanism linking subsidies to misconduct is the doctrine of dual sovereignty. Dual sovereignty is a legal principle that enables an individual or firm to be simultaneously prosecuted or penalized by the federal government as well as by any of the 50 U.S. states for the same or similar conduct, without running afoul of double jeopardy laws. Dual sovereignty was first established as legal precedent by the U.S. Supreme Court in a series of three cases between 1847 and 1852: *Fox v. Ohio* in 1847, *United States v. Marigold* in 1850, and *Moore v. Illinois* in 1852. The June 2019 Supreme Court case *Gamble v. United States* upheld a recent challenge to the constitutionality of dual sovereignty. The Court's rationale for the dual sovereignty doctrine, that antidouble jeopardy laws only prohibit multiple prosecutions for the same underlying conduct *by the same sovereign entity*, is illustrated by the following quote from the majority opinion in *Gamble*⁹:

We have long held that a crime under one sovereign's laws is not "the same offense" as a crime under the laws of another sovereign. Under this "dual-sovereignty" doctrine, a State may prosecute a defendant under state law even if the Federal Government has prosecuted him for the same conduct under a federal statute. Or the reverse may happen, as it did here.

Because Violation Tracker contains both state and federal enforcement data, I am able to exploit dual sovereignty to identify whether state-level subsidies' effect on in-state misconduct, if any, reflects a change in the underlying misconduct rate or the rate of enforcement (or both). As an example, dual sovereignty in the misconduct setting could represent a firm that engaged in wage and hour violations in California having to pay both a Wage and Hour Division fine and a fine to the California Labor Commissioner's Office. If state-level subsidies yield more lenient state-level enforcement, I should observe a decrease in the likelihood of state-level enforcement actions after a firm receives a subsidy while observing no such decrease in the likelihood of federal enforcement. A simultaneous increase in federal sanctions would further suggest weaker state-level enforcement as the channel through which in-state misconduct increases subsequent to subsidy receipt.

2.3 WHY WOULD SUBSIDIES RELATE TO MISCONDUCT?

The paper's main hypothesis is motivated by recent work examining how subsidies relate to other types of firms' political connections. Aobdia, Koester, and Petacchi [2021] show that firms that spend more on state-level political campaign contributions obtain more frequent state-level sub-

⁹ See <https://casetext.com/case/gamble-v-united-states-16>

sidies although Dong, Raghunandan, and Rajgopal [2023] highlight that politically motivated subsidies may be less effective. At a federal level, Cohen, Coval, and Malloy [2011] show that federal legislators with more Congressional power funnel more federal funds (“pork barrel” spending) to firms in their home districts. Subsidies themselves facilitate political and economic ties between awarding politicians and recipient firms. Successful subsidies are frequently used to boost politicians’ job-creation credentials, whereas opponents often attack subsidies that appear inefficient (Jensen and Malesky [2018]).¹⁰ Politicians’ incentives for subsidies to succeed in creating jobs, may, in turn, result in their supporting weaker enforcement with respect to certain costs of creating these jobs. For example, it is likely cheaper to create manufacturing jobs when workplace safety standards are not adhered to or when environmental regulations are ignored.¹¹ Given the separate state and federal costs of misconduct that result from dual sovereignty, weaker state-level enforcement should result in a lower overall marginal cost of misconduct, which may in turn induce firms to engage in higher levels of misconduct.

The argument above relies on the assumption that state legislators are able to influence within-state enforcement practices. This assumption is supported by prior law literature on deference by state-level courts and enforcement agencies to the gubernatorial administration. Most enforcement of state-level labor and environmental laws is carried out by administrative agencies housed within the governor’s office and led by labor and environmental commissioners; these commissioners are appointed by the governor, rather than directly elected, in all but a handful of states.¹² Gray [2019] finds empirical evidence that appointed state-level legal officials engage in a pattern of “strategic deference” to the governor’s preferences in order to maximize their chances of maintaining their positions. In addition, state governors wield significant influence over the composition of state-level courts. Johnson [2014] shows that this influence leads to fewer legal challenges to actions taken (or inaction) by state-level agencies, in turn allowing these agencies greater discretion to engage in enforcement actions consistent with the preferences of the administration.

¹⁰ For example, Texas Governor Greg Abbott’s Web site highlights the apparent success of several Texas state-level subsidy programs (source: <https://gov.texas.gov/organization/financial-services/grants>). In contrast, in response to a subsidy awarded to Foxconn frequently criticized for its excessive subsidy dollar value per job created, 2017 Wisconsin gubernatorial candidate Tony Evers ran a series of negative advertisements linking the deal to incumbent governor Scott Walker. Evers ultimately won the election (Johnson [2017]).

¹¹ As an example, part of gubernatorial candidate Tony Evers’s criticism of the Foxconn subsidy noted above centered around Wisconsin’s leniency toward Foxconn’s corporate conduct, with Evers specifically claiming that Wisconsin had relaxed its state-level environmental standards for Foxconn (Zettel-Vandenhouten [2017]).

¹² In subsection 5.2, I explicitly test for differences in states where these commissioners and other state-level legal officials are directly elected by the public.

State politicians' incentives for subsidized firms to create jobs, combined with the influence they wield over the state enforcement system, leads to the paper's main hypothesis:

HYPOTHESIS 1. Firms that receive subsidies are more likely to engage in corporate misconduct in subsidizing jurisdictions.

3. *Research Design*

3.1 SAMPLE SELECTION

Violation Tracker and Subsidy Tracker do not provide parent-subsidiary linkages for all publicly traded parents. Although parent-subsidiary linkage is relatively complete for most of the Russell 3000 as well as some large private and foreign-listed firms, these linkages are sporadic for other firms. To ensure that I do not erroneously label firms missing parent-subsidiary linkages as having zero violations and subsidies, I limit my analyses to firms with at least one observation in either Violation Tracker or Subsidy Tracker. Under this approach, 83.5% of sample firms have at least one subsidy from any state in any year, whereas 86.5% of sample firms receive at least one enforcement action for corporate misconduct from any federal agency in any year.

To study firms' behavior in individual states, I first need to identify which firms operate in which states. I do this using ReferenceUSA, a widely used database of establishments in the United States, that contains the parent company (if any) for a given establishment.¹³

I omit 12 states from my sample because of poor coverage in Subsidy Tracker.¹⁴ Each of these states has less than three years of subsidy data corresponding to my sample period, which, according to GJF, reflects a refusal by those states to publicly disclose subsidy recipients' names rather than a lack of subsidies awarded by those states. I also end my sample period in 2016 because Subsidy Tracker's data coverage is far less complete for the years 2017 onward (De Simone, Lester, and Raghunandan [2024]). I omit firms in the financial sector (two-digit SIC codes 60–69) because these firms often maintain indirect ownership of several industrials. I omit utility firms (two-digit SIC code 49) because these are often quasi-governmental entities

¹³ Prior work (Haltiwanger, Jarmin, and Miranda [2013], Makridis and Ohlgrogge [2017]) identifies several problems with the accuracy of establishment-level employment and sales figures in ReferenceUSA as well as a commonly used database built on ReferenceUSA, the National Economic Time Series ("NETS"), due to the heavy use of imputation and rounding. I therefore do not attempt to quantify firm-state employment figures in order to more precisely identify firm-state level operations.

¹⁴ These states are Alaska, Delaware, Hawaii, Idaho, Montana, New Hampshire, North Dakota, Pennsylvania, Rhode Island, South Dakota, Vermont, and Wyoming.

TABLE 1
Sample Selection

Start: All firms with at least one subsidy or violation between 2004 and 2016, times 50 potential states	1,821,300
Less: 12 states with poor subsidy coverage	(437,112)
Less: firm-state-years with no establishments	(1,046,065)
Less: Financial and utility firms	(60,239)
Less: Missing Compustat variables	(11,459)
Less: Singletons dropped due to fixed effects structure	(8,536)
Final sample size	257,889

for which the classification of cash transfers from government to firm is inconsistent.¹⁵ Table 1 provides details of how I arrive at my main sample.

3.2 ECONOMETRIC SPECIFICATION

Although my unit of analysis is the firm-state-year level, I only observe most firm-specific control variables at the firm-year level. I therefore estimate a linear probability model in order to incorporate a rich fixed effects structure to overcome this limitation. The basic specification that I estimate is:

$$Misconduct_{ijt} = \beta_0 + \beta_1 ConnectedSub_{ijt} + \beta_2 Controls_{ijt} + \gamma_{ij} + \theta_{jkt} + \varepsilon_{ijt}, \quad (1)$$

where the subscripts i , j , k , and t denote firm, state, industry, and time, respectively. The dependent variable $Misconduct_{ijt}$ is an indicator that equals 1 if firm i pays a penalty to the federal government for misconducted committed in state j during year t . Because federal enforcement is plausibly exogenous to state-level political connections, this variable captures the underlying incidence of misconduct but *not* any effects of concurrent changes in state-level enforcement that may arise from subsidy receipt.

The primary independent variable of interest, $ConnectedSub_{ijt}$, is an indicator variable that equals 1 if firm i has received a subsidy from state j in year t or prior and while the current governor of state j was in office. For example, if the governor of State s is in office from 2007 to 2010 and Firm x receives a subsidy in 2008, then $ConnectedSub_{xst}$ equals 0 for $t = 2007$, 1 for $t \in \{2008, 2009, 2010\}$, and 0 for $t = 2011$.

I use an indicator variable to capture subsidized firms rather than the dollar value of the underlying subsidies for several reasons. First, the subsidy value is missing from the underlying data more than one-sixth of the

¹⁵ For example, if a state government imposes a price ceiling on electricity for low-income families, it may make compensating payments to an electricity company to cover the difference between the price ceiling and the prevailing market rate. State governments may classify these payments as corporate subsidies in some cases but subsidies to individual persons (which would not be reflected in Subsidy Tracker) in others.

time (i.e., the existence of a subsidy, but not its value, is observed in many cases). Second, it is difficult to accurately identify annual dollar amounts. Although Subsidy Tracker provides estimates of the dollar value of a subsidy, the value provided is an expected total value over the length of the subsidy, which typically lasts for multiple years. However, because the length of each subsidy is not given in the data, there is no way to easily split a subsidy into its yearly components. Additionally, subsidy value estimates are not computed in a consistent fashion; estimates come from a variety of sources ranging from the awarding agencies to independent newspapers. Different sources may provide different estimates of subsidy value; for example, one may provide the expected value of a subsidy whereas the other may provide the maximum possible value.¹⁶

I control for a limited number of factors that are observable at the firm-state-year level. Most notable among these is the number of establishments (i.e., business locations such as stores, manufacturing plants, or offices) that firm i has in state j in year t . I obtain these data from yearly ReferenceUSA snapshots. In this as well as in all other tests I double-cluster standard errors by firm and industry-year. Industry-year clustering accounts for any systematic differences in federal agencies' enforcement focuses over time.

My research design relies on a rich set of fixed effects at the firm-state (γ_{ij}) and state-industry-year (θ_{jkt}) level. I measure industry as a firm's two-digit SIC code. The inclusion of firm-state fixed effects is key to my design as it is otherwise plausible that a correlation between misconduct committed in a state and political connections in that state simply reflects a firm's overall presence in the state.¹⁷ *Ceteris paribus*, a firm that has a large presence in a state is more likely to engage in misconduct in that state relative to a firm with a small (or no) presence in that state simply because there are more chances to do so. Firm-state fixed effects also account for the possibility that some firms are both (i) systematically more likely to seek and obtain subsidies and (ii) engage in systematically different forms of misconduct to non-subsidy-seeking firms, some of which may be of more interest to federal regulators than others. I include state-industry-year fixed effects, following Aobdia, Koester, and Petacchi [2021], to capture any aggregate trends in misconduct or its enforcement that may be reflected in the firm's behavior. This fixed effects structure means that the coefficient β_1 measures

¹⁶ Despite these issues, I verify in section 4 that my inferences do not change if I use subsidy dollar values rather than subsidy indicators to construct *ConnectedSubijt*.

¹⁷ Controlling for the number of establishments firm i has in state j in year t , in conjunction with my fixed effects structure, accounts for any potential systematic differences in how firms may choose to set up their establishments. For example, the modal establishment for a manufacturing firm may be a plant whereas the modal establishment for a hospitality business may be a restaurant; my fixed effects strategy accounts for this. More generally, the use of firm-state fixed effects in conjunction with controlling for firm-state-year establishment counts mitigates problems associated with being unable to observe accurate firm-state-year employment or sales figures as long as the number of establishments is correlated with the number of employees in a given firm-state.

the change in the likelihood that a firm engages in misconduct in a state after receiving a state-level subsidy, relative to both the same firm-state in the presubsidy period and contemporaneous levels of misconduct committed by unsubsidized industry peers in the same state and year.

A firm's decision to engage in misconduct is financially motivated (e.g., by the desire to cut costs). I therefore select firm-level control variables based on existing research on financial determinants of nonfinancial misconduct (Cohn and Wardlaw [2016], Caskey and Ozel [2017]). These include firm size (measured as log assets), market to book ratio, return on assets (ROA), sales growth rate, PP&E, and leverage. All ratio-based variables (i.e., those that are not the natural logarithm of an underlying construct) are winsorized at 1% in each tail. I present a full list of variables used in my analyses and their definitions in the appendix. After removing firms with missing data, there are 257,889 firm-state-years reflecting 1,568 distinct firms in the final sample, spanning the years 2004–16. Each firm-year has operations in 16.7 states on average out of the 38 I retain in my sample.

4. Results

4.1 DESCRIPTIVE STATISTICS

Table 2 presents summary statistics for subsidies awarded to firm-state-years in my sample. In panel A, I provide information on the value of subsidies at the firm-state-year level conditional on obtaining at least one subsidy in the given state-year. This reflects 9,758 firm-state-years in which at least one subsidy was awarded in the sample, which corresponds to 17,992 firm-state-year observations for which *ConnectedSub_{ijt}* equals one.

The average value of subsidies awarded within a firm-state-year is \$5.35 million. The conditional distribution of subsidy dollar amounts is right-skewed as the median subsidy awarded, within subsidy firm-state-years with nonmissing data on dollar values, is \$193,692 whereas the 90th percentile is \$3.76 million. At the firm-year level, the average firm receives subsidies in 1.7 states on average in years in which it receives at least one subsidy from any state; when considering all firm-years this figure is 0.6 states on average. In years in which a firm receives at least one subsidy, it receives subsidies valued at an average of \$8.95 million. Given that the average firm-year in my sample operates in 16.7 states, this result supports using firm-state-years rather than firm-years as the unit of observation. Finally, when considering the aggregated *ConnectedSub_{ijt}* measure, firms are connected in an average of 2.3 states in years in which they are connected, when considering all firm-years firms are connected in on average 1.2 states.

Panels B, C, and D of table 2 present statistics on the distribution of subsidies (number of subsidies, count of subsidies with nonzero dollar value, and aggregate estimated dollar value of subsidies awarded) by year, industry, and state. It is evident from panel B that subsidies have been increasing in frequency and value over time, consistent with popular press coverage

TABLE 2
Descriptive Statistics on Subsidies

Panel A: Descriptive statistics for subsidy firm-state-years						
	<i>N</i>	Mean	Median	SD	10th %ile	90th %ile
All subsidy firm-state-years	9,758	4,716.22	132.96	100,433.80	0	3,047.81
Firm-state-years with subs. \$ available	8,608	5,346.29	193.69	106,917.30	11.77	3,755.48

Panel B: Subsidies by year					
Year	<i>N</i>	# Subs. Firm-State-Years	# Subs.	# Subs. with \$ Value	Total Subsidy \$
2004	17,342	301	456	320	915.35
2005	18,723	351	565	379	1,015.15
2006	19,409	447	720	454	722.74
2007	19,659	583	1,044	721	968.12
2008	20,174	788	1,437	1,135	1,325.71
2009	20,189	953	1,725	1,459	2,833.47
2010	20,029	968	1,866	1,627	2,859.76
2011	20,185	933	1,665	1,525	4,926.98
2012	20,201	918	1,683	1,523	4,176.15
2013	20,693	1,008	2,210	2,086	12,509.21
2014	20,737	961	2,063	1,938	7,325.91
2015	20,866	825	1,650	1,562	3,465.1
2016	19,682	722	1,545	1,460	2,977.25
Total	257,889	9,758	18,629	16,189	46,020.90

Panel C: Subsidies by industry					
Industry	<i>N</i>	# Subs. Firm-State-Years	# Subs.	# Subs. with \$ Value	Total Subs. \$
Agriculture	529	28	30	24	2.77
Construction	6,273	54	60	55	32.34
Manufacturing	11,8221	6,314	11,469	9,841	35,248.62
Mining	5,399	209	601	575	420.11
Other	1,552	214	480	415	883.45
Retail Trade	47,067	1,077	2,615	2,406	1,366.74
Services	41,199	861	1,287	1,102	3,133.41
Transp./Comms.	19,936	610	1,504	1,313	1,415.74
Wholesale Trade	17,713	391	583	458	3,517.7
Total	257,889	9,758	18,629	16,189	46,020.90

Panel D: Subsidies by state					
State	<i>N</i>	# Subs. Firm-State-Years	# Subs.	# Subs. with \$ Value	Total Subsidy \$
AL	6,186	58	234	234	3.95
AR	5,244	159	284	284	188.69
AZ	7,063	198	392	84	40.17
CA	11,574	518	631	486	1,230.14
CO	7,794	267	1,274	1,274	117.65
CT	5,770	59	110	103	1,239.84

(Continued)

TABLE 2—(Continued)

Panel D: Subsidies by state					
State	<i>N</i>	# Subs. Firm-State-Years	# Subs.	# Subs. with \$ Value	Total Subsidy \$
FL	8,966	203	327	316	726.46
GA	8,659	51	56	56	480.33
IA	4,815	158	220	218	620.54
IL	9,583	424	669	477	530.78
IN	7,075	286	461	461	523.08
KS	5,651	96	106	89	30.37
KY	6,020	319	610	605	941.14
LA	6,142	528	1,969	1,957	7,221.44
MA	7,942	119	143	87	373.75
MD	6,356	365	415	378	114.57
ME	3,156	606	755	724	88.91
MI	7,527	80	184	183	3,201.68
MN	6,814	19	20	4	1.74
MO	6,797	109	132	132	2,790.69
MS	4,848	137	182	167	71.78
NC	8,264	1,003	1,679	1,679	1,799.26
NE	4,233	58	61	3	1.73
NJ	7,789	82	105	101	1,122.87
NM	4,498	48	117	117	55.42
NV	5,228	30	71	63	1,434.35
NY	8,691	713	1,531	1,531	2043.3
OH	8,501	495	787	710	1,032.77
OK	5,836	453	1,367	1,361	1,195.95
OR	5,604	166	423	410	4,678.81
SC	6,013	98	112	56	1,122.79
TN	7,186	95	115	96	162.57
TX	11,292	325	599	82	164.17
UT	5,757	125	143	141	370.69
VA	7,541	216	310	310	431.71
WA	7,345	868	1,696	1021	9,420.87
WI	6,470	98	124	120	436.34
WV	3,659	126	215	69	9.65
Total	257,889	9,758	18,629	16,189	46,020.90

This table presents descriptive statistics on subsidies for sample firm-state-years. Panel A presents descriptive statistics for the pooled sample for the 9,758 firm-state-years that award at least one subsidy, as well as for the 8,608 subsidy firm-state-years that have subsidies with estimated dollar values available. Panels B, C, and D present subsidy descriptive statistics on aggregate frequency and dollar value by year, industry, and state. In panel A, figures are in thousands of dollars; in panels B, C, and D figures are in millions of dollars. Figures in panel A are in thousands of dollars. Panel B presents the total number of subsidies awarded, total number of subsidies with dollar value available, and total dollar amount (in millions) by year for each year of my sample period (2004 through 2016). Note that the numbers 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. Panel C presents the total number of subsidies awarded, total number of subsidies with dollar value available, and total dollar amount (in millions) by SIC major industry classification cumulatively between 2004 and 2016. Note that the numbers 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. Panel D presents the total number of subsidies awarded, total number of subsidies with dollar value available, and total dollar amount (in millions) by state cumulatively between 2004 and 2016. 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.

suggesting such a trend.¹⁸ Panel C indicates that firms in the manufacturing, retail trade, and wholesale trade industries are the most prominent subsidy recipients in the sample. Panel D illustrates state-level heterogeneity across subsidies. Collectively, panels B, C, and D highlight the importance of state-industry-year fixed effects.

Table 3 presents summary statistics for corporate misconduct committed by firm-state-years and assessed by federal agencies in my sample. In panel A, I provide information on penalties assessed by federal agencies at the firm-state-year level for the 5,761 firm-state-years with at least one violation in the state.¹⁹ As with subsidies, the conditional distribution of penalty amounts is right-skewed. The mean (median) amount of compliance penalties paid by a firm for misconduct in a given state-year, conditional on misconduct occurring, is \$526, 744 (\$14, 400). This skewness likely results from legally imposed caps on the penalties that most federal agencies can charge, regardless of a violation's severity.

Panels B, C, and D of table 3 present statistics on the distribution of violation frequency by year, industry, and state. Violation rates are generally consistent within the sample over time, ranging from 1.8% of firm-state-years in 2005 to 2.7% of firm-state-years in 2016. There is greater variation in violations across states. For example, the firm-state-year violation rate ranges from lower than 1% in Arizona, Maine, South Carolina, and Utah to over 5% in California and Texas. Because these statistics capture federal enforcement patterns, they are not reflective of heterogeneity in enforcement practices across states (i.e., the possibility that some states may be systematically stricter or more lenient toward certain corporate actions). These statistics may instead reflect differences in industry composition and other statewide economic factors, underscoring the importance of using state-industry-year fixed effects.

Panel E of table 3 presents the distribution of federal violations by assessing federal agency. For brevity, I tabulate figures for the six agencies that are associated with violations by at least 100 firm-state-years in the sample. From this panel, it is apparent that labor violations are the most common in my sample. Of the set of firm-state-years with at least one violation of any type, the majority (58.0%) contain a workplace safety violation, assessed by OSHA; when considering safety more broadly, incorporating MSHA violations, this figure rises to 62.8%. Another 12.8% contain a wage and hour violation, assessed by WHD. Conversely, only 12.3% of violation firm-state-years contain at least one violation assessed by the EPA (figures do not add up to 100% because a single firm-state-year can have multiple violation types). This disparity likely reflects both (i) a greater underlying frequency

¹⁸ See, for example, <https://www.nytimes.com/2012/12/02/us/how-local-taxpayers-bankroll-corporations.html>

¹⁹ Although the 5,761 figure represents 2.2% of firm-state-years, the proportion of firm-years that have a violation in *any* state is much higher, at 23% (untabulated), highlighting that firms' corporate conduct varies over time.

TABLE 3
Descriptive Statistics on Corporate Misconduct

Panel A: Descriptive statistics for violation firm-years						
	<i>N</i>	Mean	Median	SD	10th %ile	90th %ile
Violation firm-state-years	5,769	540.28	14.27	8,614.97	5.60	165.00

Panel B: Violations by year		
State	<i>N</i>	# Federal Penalty Firm-State-Years
2004	17,342	353
2005	18,723	344
2006	19,409	362
2007	19,659	417
2008	20,174	414
2009	20,189	406
2010	20,029	518
2011	20,185	521
2012	20,201	486
2013	20,693	476
2014	20,737	454
2015	20,866	507
2016	19,682	511
Total	257,889	5,769

Panel C: Violations by industry		
Industry	<i>N</i>	# Federal Penalty Firm-State-Years
Agriculture	529	9
Construction	6,273	186
Manufacturing	118,221	2,952
Mining	5,399	393
Other	1,552	81
Retail Trade	47,067	1,001
Services	41,199	496
Transp./Comms.	19,936	455
Wholesale Trade	17,713	196
Total	257,889	5,769

Panel D: Violations by state		
State	<i>N</i>	# Federal Penalty Firm-State-Years
AL	6,186	126
AR	5,244	78
AZ	7,063	54
CA	11,574	684
CO	7,794	184
CT	5,770	115
FL	8,966	207
GA	8,659	219
IA	4,815	63
IL	9,583	330

(Continued)

TABLE 3—(Continued)

Panel D: Violations by state		
State	<i>N</i>	# Federal Penalty Firm-State-Years
IN	7,075	106
KS	5,651	94
KY	6,020	156
LA	6,142	95
MA	7,942	160
MD	6,356	67
ME	3,156	29
MI	7,527	89
MN	6,814	92
MO	6,797	129
MS	4,848	72
NC	8,264	193
NE	4,233	64
NJ	7,789	193
NM	4,498	56
NV	5,228	92
NY	8,691	246
OH	8,501	346
OK	5,836	89
OR	5,604	33
SC	6,013	43
TN	7,186	139
TX	11,292	558
UT	5,757	44
VA	7,541	157
WA	7,345	135
WI	6,470	152
WV	3,659	80
Total	257,889	5,769

Panel E: Violations by Federal Agency	
Agency	# Federal Penalty Firm-State-Years
Occupational Safety and Health Administration	3,344
Wage and Hour Division	737
Environmental Protection Agency	707
National Labor Relations Board	553
Mine Safety and Health Administration	281
Equal Employment Opportunity Commission	142
Other	269
Total	5,769

This table presents descriptive statistics on corporate misconduct (i.e., violations) for sample firm-state-years. Panel A presents descriptive statistics for the pooled sample for the 5,769 firm-state-years with at least one violation of one federal agency's laws that resulted in monetary penalties. Panels B, C, and D present descriptive statistics on aggregate misconduct frequency by year, industry, and state. Panel A presents descriptive statistics on the monetary value of penalties assessed for corporate misconduct, within misconduct firm-state-years. Figures in this panel are in thousands of dollars. Panel B presents the number of corporate

(Continued)

TABLE 3—(Continued)

misconduct firm-state-years by year for each year of my sample period (2004 through 2016). Note that the numbers represent penalties assessed to firms that enter my sample; the actual number of penalties in Violation Tracker for these states and years is significantly larger. Panel C presents the number of corporate misconduct firm-state-years by industry cumulatively between 2004 and 2016. Note that the numbers represent penalties assessed to firms that enter my sample; the actual number of penalties in Violation Tracker for these states and years is significantly larger. Panel D presents the number of corporate misconduct firm-state-years by state cumulatively between 2004 and 2016. Note that the numbers represent penalties assessed to firms that enter my sample; the actual number of penalties in Violation Tracker for these states and years is significantly larger. Panel E presents the number of corporate misconduct firm-state-years by the federal agency assessing the violation. I provide information for the six agencies for which at least 100 firm-state-years have a violation; the remainder are aggregated under “other.” Note that the total number of violation firm-state-years by agency need not necessarily sum to the total number of violation firm-state-years because one firm can have multiple violations at the same time in the same state.

TABLE 4
Sample Descriptive Statistics

	<i>N</i>	Mean	Median	SD	10th %ile	90th %ile
<i>Misconduct</i> _{<i>ij</i>}	257,889	0.022	0.000	0.148	0.000	0.000
Log misconduct penalty \$	257,889	0.225	0.000	1.503	0.000	0.000
<i>ConnectedSub</i> _{<i>ij</i>}	257,889	0.070	0.000	0.255	0.000	0.000
Log connected subsidy \$	255,604	0.785	0.000	3.122	0.000	0.000
<i>CEOStatePAC</i> _{<i>ij</i>}	257,889	0.002	0.000	0.048	0.000	0.000
<i>CEOFederalPAC</i> _{<i>it</i>}	257,889	0.221	0.000	0.415	0.000	1.000
<i>CorporateFederalPAC</i> _{<i>it</i>}	257,889	0.336	0.000	0.472	0.000	1.000
<i>FederalLobbying</i> _{<i>it</i>}	257,889	0.387	0.000	0.487	0.000	1.000
<i>CorporateStatePAC</i> _{<i>ij</i>}	257,889	0.015	0.000	0.123	0.000	0.000
<i>BoardConnectedState</i> _{<i>it</i>}	235,780	0.049	0.000	0.216	0.000	0.000
<i>BoardConnectedFederal</i> _{<i>ij</i>}	235,780	0.526	1.000	0.499	0.000	1.000
<i>AGGovOpp</i> _{<i>ij</i>}	257,889	0.306	0.000	0.461	0.000	1.000
<i>CommissionerOpp</i> _{<i>ij</i>}	257,889	0.114	0.000	0.317	0.000	1.000
<i>ElectionYear</i> _{<i>ij</i>}	257,889	0.245	0.000	0.430	0.000	1.000
<i>CompetitiveElection</i> _{<i>ij</i>}	257,889	0.061	0.000	0.239	0.000	0.000
<i>StateFederalOpp</i> _{<i>ij</i>}	257,889	0.349	0.000	0.477	0.000	1.000
<i>UnifiedGovt</i> _{<i>it</i>}	257,889	0.371	0.000	0.483	0.000	1.000
<i>ConnectedFederalSub</i> _{<i>ij</i>}	257,889	0.210	0.000	0.407	0.000	1.000
Log establishments	257,889	1.579	1.386	1.494	0.000	3.761
Log assets	257,889	8.136	8.020	1.561	6.204	10.231
ROA	257,889	0.113	0.104	0.089	0.026	0.218
Sales growth rate	257,889	1.072	1.056	0.191	0.903	1.243
Market to book	257,889	3.076	2.380	4.180	0.945	6.119
Leverage	257,889	0.228	0.203	0.187	0.000	0.472
PP&E	257,889	0.269	0.215	0.203	0.057	0.577

This table provides descriptive statistics for variables used in the paper's main regression analyses. Sample size reflects the sample used in tables 5, 6, and 9. For variable definitions please refer to appendix A.

of labor infractions across a broader set of industries and (ii) the fact that several nonlabor violations (e.g., antitrust issues) are assessed to the firm as a whole, with no specific violation location given, making it impossible to map the violation to a specific firm-state.

Table 4 provides descriptive statistics for variables used in my empirical analyses. Sample firms are on average large (measured by log assets) and

profitable (measured by ROA). The median firm-state-year has four establishments, although this reflects the large number of firm-state-years I pick up; the 90th percentile firm-state-year has 43 establishments. In untabulated analyses, these figures are higher (median of 6, 90th percentile of 67) when considering only firms' headquarters states.

4.2 SUBSIDIES AND CORPORATE MISCONDUCT

I present results from estimating equation (1) in table 5. The positive significant coefficient on $ConnectedSub_{ijt}$ supports the paper's primary hypothesis: After controlling for overall and time-variant measures of firm-state economic activity and a battery of firm characteristics, I find in columns 1 and 2 that after receiving state subsidies, firms are more willing to engage in corporate misconduct in awarding states. In column 1, I use as the dependent variable an indicator variable for the existence of misconduct committed by firm i in state j in year t (incidence). Compared to the base firm-state-year average violation rate of 2.2%, the estimates in column 1 imply a 34% increase in violation likelihood for subsidized firms. Note, however, that my fixed effects structure means that this 34% figure is with respect to the same firm in the same state in other years and within-industry peer firms in the same state in the same years; it is not with respect to the unconditional violation likelihood. Column 2 instead uses the natural logarithm of one plus the dollar value of penalties assessed for this misconduct (severity). Although penalty dollar values are typically quite small (see subsection 2.2), this specification nonetheless enables me to assess the *relative* severity of penalties on the basis of their dollar values; I find that subsidized firms pay more in penalties, suggesting more severe levels of underlying misconduct relative to nonsubsidized firms. I validate these findings in columns 3 and 4 with a placebo test, replacing the dependent variable to reflect an indicator for and the log dollar value of out-of-state misconduct, respectively. I find no relation between subsidies received in state j and misconduct committed in states other than j .

In terms of control variables, I find a positive coefficient on the number of establishments firm i has in state j in year t . This result is unsurprising, as firms with a greater presence in a state may be more likely to engage in misconduct simply by having more chances to do so. My fixed effects structure means that this coefficient reflects deviations from the firm's average level of establishments in the state, and so this variable also accounts for firm-level economic growth or decline *within* a state over time. I also find a positive coefficient on firm size, which may reflect regulators' enforcement preferences for larger targets. I do not find statistically significant coefficients on other control variables, perhaps because my use of firm-by-state fixed effects absorbs most variation in these variables—especially for the set of firms retained in my final sample.²⁰

²⁰ In untabulated analyses, I reestimate my main specification without the firm-state fixed effect. The coefficient on $ConnectedSub_{ijt}$ remains positive and significant, as does the

TABLE 5
State Subsidies and Corporate Misconduct

Violation Variable: Violation Location: Dependent Variable Type:	Federal Violations						
	In-State			Out-of-State			In-State Indicator
	Indicator (1)	Log Penalty \$ (2)	Indicator (3)	Indicator (3)	Log Penalty \$ (4)	Indicator (6)	
<i>ConnectedSub_{jt}</i>	0.008*** [3.06]	0.072*** [2.84]	0.007 [1.23]	0.067 [0.97]	0.007*** [2.85]	0.007*** [2.89]	0.008*** [2.89]
<i>CorporateStatePAC_{jt}</i>	0.002 [0.50]	0.026 [0.61]	0.011 [0.82]	0.043 [0.27]	0.001 [0.27]	0.002 [0.50]	0.004 [0.95]
<i>ConnectedSub_{jt} × CorporateStatePAC_{jt}</i>					0.005 [0.56]		
<i>CEOSStatePAC_{jt}</i>	-0.005 [-0.37]	-0.065 [-0.47]	-0.040* [-1.84]	-0.620** [-2.51]	-0.005 [-0.38]	-0.016 [-1.05]	-0.003 [-0.24]
<i>ConnectedSub_{jt} × CEOSStatePAC_{jt}</i>						0.044 [1.30]	
<i>BoardConnectedState_{it}</i>							0.008* [1.73]
<i>ConnectedSub_{jt} × BoardConnectedState_{it}</i>							-0.001 [-0.17]
<i>CEOFederalPAC_{it}</i>	0.002** [2.18]	0.024** [2.12]	0.017 [1.34]	0.158 [0.98]	0.002** [2.18]	0.002** [2.18]	0.002* [1.86]
<i>CorporateFederalPAC_{it}</i>	-0.001 [-0.69]	-0.011 [-0.63]	-0.017 [-0.71]	-0.229 [-0.78]	-0.001 [-0.69]	-0.001 [-0.69]	-0.002 [-0.97]
<i>FederalLobbying_{it}</i>	0.002 [1.38]	0.028 [1.62]	0.031* [1.80]	0.503** [2.37]	0.002 [1.38]	0.002 [1.38]	0.003* [1.67]
<i>BoardConnectedFederal_{it}</i>							0.000 [0.38]

(Continued)

TABLE 5—(Continued)

Violation Variable:	Federal Violations						
	In-State			Out-of-State			In-State
	Indicator	Log Penalty \$	Indicator	Log Penalty \$	Indicator	Log Penalty \$	Indicator
Dependent Variable Type:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log establishments	0.003 ^{***} [3.11]	0.025 ^{***} [3.13]	0.002 [0.46]	0.048 [0.72]	0.003 ^{***} [3.11]	0.002 ^{***} [3.10]	0.003 ^{***} [3.30]
Log assets	0.007 ^{***} [4.24]	0.069 ^{***} [4.22]	0.065 ^{***} [4.26]	0.916 ^{***} [4.71]	0.007 ^{***} [4.24]	0.007 ^{***} [4.23]	0.007 ^{***} [3.91]
ROA	0.010 [*] [1.67]	0.108 [*] [1.77]	-0.030 [-0.36]	-0.658 [-0.64]	0.010 [*] [1.67]	0.010 [*] [1.67]	0.010 [1.43]
Sales growth rate	-0.003 [-1.28]	-0.030 [-1.19]	-0.011 [-0.45]	-0.165 [-0.54]	-0.003 [-1.28]	-0.003 [-1.28]	-0.003 [-0.99]
Market to book	-0.000 [-0.50]	-0.000 [-0.47]	-0.000 [-0.29]	0.000 [0.00]	-0.000 [-0.50]	-0.000 [-0.51]	-0.000 [-0.58]
Leverage	-0.006 [-1.31]	-0.045 [-1.07]	0.008 [0.17]	0.697 [1.19]	-0.006 [-1.31]	-0.006 [-1.31]	-0.006 [-1.35]
PP&E	0.005 [0.56]	0.058 [0.61]	-0.009 [-0.10]	-0.515 [-0.48]	0.005 [0.56]	0.005 [0.55]	0.005 [0.45]
Firm-State FE	Y	Y	Y	Y	Y	Y	Y
State-Industry-Year FE	Y	Y	Y	Y	Y	Y	Y
Observations	257,889	257,889	257,889	257,889	257,889	257,889	234,391
Adjusted R ²	0.146	0.157	0.394	0.418	0.146	0.146	0.143

This table presents results from estimating equation (1). Observations are at the firm-state-year level, and the sample period ranges from 2004 to 2016. In columns 1, 5, 6, and 7, the dependent variable is an indicator for whether firm i was sanctioned for corporate misconduct in state j in year t ; in column 2, the dependent variable is the natural logarithm of the dollar value of penalties assessed for corporate misconduct in state j in year t ; column 3 is an indicator for whether firm i was sanctioned for corporate misconduct in any state other than j in year t ; and in column 4, the dependent variable is the natural logarithm of one plus the dollar value of penalties assessed for corporate misconduct in states other than j in year t . See appendix A for variable definitions. The primary independent variable of interest is *ConnectedSub_{ijt}*, an indicator that equals 1 if firm i received a subsidy prior to or during year t while the governor of state j was in power. Columns 5–7 include an additional variable of interest, reflecting an interaction between *ConnectedSub_{ijt}* and one of three variables indicative of political connections to state officials, based on firm-level PAC contributions, CEO PAC contributions, and board members' connections, respectively. All specifications include firm-state and state-industry-year fixed effects. Standard errors are double-clustered by firm and industry-year. Estimated t -statistics are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Prior literature (e.g., Bertrand et al. [2020a]) highlights the importance of accounting for as many forms political connections as possible in assessing the incremental impact of a specific type of connection (in this case, subsidies). I therefore control for several forms of political connections in all specifications, to ensure that my results reflect the impact of subsidies rather than other, potentially related, connections between firms and state governments. These include indicators for whether the firm or its CEO has made contributions to federal politicians in the current administration (*CorporateFederalPAC_{it}* and *CEOFederalPAC_{it}*, respectively) and whether the firm has engaged in federal lobbying efforts (*FederalLobbying_{ijt}*).²¹ Two variables are particularly important in this regard, inasmuch as they also represent a potential connection to state-level politicians: whether (i) firms or (ii) their CEOs contribute to candidates for state office. These variables, *CorporateStatePAC_{ijt}* and *CEOStatePAC_{ijt}*, are constructed as indicators for whether firm *i* or its CEO made political campaign contributions to state officials (governor, state-level House and Senate, etc.) during the current governor's term. Prior literature documents a link between this measure and subsidy receipt (Aobdia, Koester, and Petacchi [2021]). As an additional form of political connection, I consider board members who are former politicians.²² Prior literature (Goldman, Rocholl, and So [2009], Houston et al. [2014]) shows that board members' connections influence their firms' capital market outcomes; it is plausible that politically connected boards may be able to influence enforcement outcomes as well. Following Houston et al. [2014], I create indicators for whether the firm's board members are connected to state governments (*BoardConnectedState_{it}*) or the federal government (*BoardConnectedFederal_{it}*). I caveat that although *BoardConnectedState_{it}* picks up connections to state-level politicians in general, unlike the other two measures (*CorporateStatePAC_{ijt}* and *CEOStatePAC_{ijt}*), it does not pick up which state(s') governments board members have connections to.

Using these measures, in columns 5–7 of table 5, I interact *ConnectedSub_{ijt}* with *CorporateStatePAC_{ijt}*, *CEOStatePAC_{ijt}*, and

coefficient on the number of establishments. In this specification, I also find positive coefficients on firm size, PP&E and sales growth. The former two results may reflect variation in firm-level characteristics that result in enforcement actions; for example, a firm with more fixed assets (PP&E) may also be one with more opportunities to create an unsafe environment for employees operating those fixed assets. One interpretation of the positive coefficient on sales growth is that growing firms may be more willing to cut corners to continue on their trajectory. I also find negative coefficients on ROA and leverage, with the former suggesting that better performing firms likely see less need to take actions that result in violations.

²¹ Although state-level lobbying would also be a natural quantity to control for, such data are unavailable for most U.S. states as a result of relatively weak state-level disclosure laws.

²² I do not include this variable in my main specification because I am unable to construct these data for several firms; rather than induce nontrivial sample attrition throughout the paper's empirical analyses, I instead consider board members' connections in a separate column of table 5.

$BoardConnectedState_{it}$, respectively. In all three cases, I do not find that other forms of state-level political connections appear to affect the link between subsidies and corporate misconduct.²³ This result underscores the importance of distinguishing ex ante attempts at forming connections (PAC contributions) from ex post realized connections (subsidies) and suggests that the effect of subsidies on firms' misconduct outcomes does not depend on whether the subsidy may have arose through a pre-existing political connection resulting from campaign contributions or personal ties.

4.2.1. Robustness to Alternative Measures. To ensure that the results in table 5 are not driven by specification choices, before moving on to the channels potentially driving these results, I conduct sensitivity tests with respect to model specification. First, my main independent variable of interest $ConnectedSub_{ijt}$ is an indicator and as such measures the *incidence* of a political connection. For the reasons given in subsection 3.2, this is my preferred measure of connectedness. Nonetheless, it may be possible that subsidy intensity plays a role. As a result, despite imperfections in measurement, I replace $ConnectedSub_{ijt}$ with the natural logarithm of one plus the cumulative dollar value of connected subsidies awarded to firm i in state j while the governor is in power.²⁴ Results from estimating this alternative specification are presented in columns 1 and 2 of table 6. I continue to find a relation between subsidy receipt and subsequent corporate misconduct.

Second, although I control for a variety of firm-level variables that may affect the likelihood of misconduct, my results could potentially be driven by unobservable time-varying firm-specific characteristics. To rule out this possibility, I replace firm-level control variables with firm-year fixed effects (i.e., I include three sets of fixed effects: (i) firm-state, (ii) state-industry-year, and (iii) firm-year). Results from this alternative fixed effects specification are presented in columns 3 and 4 of table 6. My findings are virtually unchanged relative to table 5 and so, because control variables are informative in understanding my results, I conduct further tests based on the specification given in equation (1).

Third, in column 5 of table 6, I consider the timing of misconduct relative to subsidies. I construct indicators for one, two, three, and four years prior and subsequent to a subsidy (as well as an indicator for the subsidy year itself and for future years). I conduct this test to ensure that the onset of misconduct does not precede subsidies within a state, which could suggest alternative explanations to the paper's main results. I do not find, in

²³ As an additional robustness test, I rerun the analyses in columns 1 and 2 of table 5 on the subset of observations with no political connections to state officials. My results continue to hold on this sample.

²⁴ For example, if the governor of state j is in power from 2007 to 2011 and firm i receives nothing in 2007, 2009, and 2011, \$10 in subsidies in 2008, and \$15 in 2010, this variable equals 0 in 2007, $\log(11)$ in 2008 and 2009, and $\log(26)$ in 2010 and 2011.

TABLE 6
Robustness to Alternative Specifications

Violation Variable: Violation Location: Dependent Variable Type:	Federal Violations				
	In-State				
	Indicator (1)	Log Penalty \$ (2)	Indicator (3)	Log Penalty (4)	Indicator (5)
Log connected subsidy _{ijt}	0.001** [2.44]	0.005** [2.21]			
<i>ConnectedSub_{ijt}</i>			0.008*** [2.97]	0.074*** [2.80]	
<i>CEOStatePAC_{ijt}</i>	-0.005 [-0.37]	-0.061 [-0.44]	-0.001 [-0.08]	-0.027 [-0.18]	-0.005 [-0.37]
<i>CEOFederalPAC_{it}</i>	0.002** [2.02]	0.022* [1.95]	0.000 [0.00]	0.000 [0.00]	0.002** [2.19]
<i>CorporateFederalPAC_{it}</i>	-0.001 [-0.76]	-0.012 [-0.73]	0.000 [0.00]	0.000 [0.00]	-0.001 [-0.71]
<i>FederalLobbying_{it}</i>	0.002 [1.33]	0.026 [1.58]	0.000 [0.00]	0.000 [0.00]	0.002 [1.40]
<i>CorporateStatePAC_{ijt}</i>	0.001 [0.29]	0.019 [0.47]	0.000 [0.04]	0.005 [0.12]	0.002 [0.45]
<i>PreSubsidy4</i>					-0.006 [-1.21]
<i>PreSubsidy3</i>					0.001 [0.20]
<i>PreSubsidy2</i>					0.007* [1.67]
<i>PreSubsidy1</i>					0.006 [1.37]
<i>PostSubsidy0</i>					0.009*** [2.96]
<i>PostSubsidy1</i>					0.008* [1.78]
<i>PostSubsidy2</i>					0.015** [2.01]
<i>PostSubsidy3</i>					0.016* [1.77]
<i>PostSubsidy4</i>					0.022* [1.73]
<i>PostSubsidy5+</i>					0.031* [1.88]
Log establishments	0.002*** [2.97]	0.024*** [2.96]	0.002** [2.07]	0.022** [1.99]	0.002*** [3.09]
Log assets	0.007*** [4.24]	0.070*** [4.21]			0.007*** [4.20]
ROA	0.011* [1.79]	0.113* [1.87]			0.010 [1.64]
Sales growth rate	-0.003 [-1.24]	-0.029 [-1.15]			-0.003 [-1.26]
Market to book	-0.000 [-0.55]	-0.000 [-0.52]			-0.000 [-0.50]

(Continued)

TABLE 6—(Continued)

Violation Variable:	Federal Violations				
	In-State				
Violation Location:					
Dependent Variable Type:	Indicator	Log Penalty \$	Indicator	Log Penalty	Indicator
	(1)	(2)	(3)	(4)	(5)
Leverage	−0.005 [−1.25]	−0.045 [−1.03]			−0.005 [−1.30]
PP&E	0.006 [0.65]	0.068 [0.72]			0.005 [0.54]
Firm-State FE	Y	Y	Y	Y	Y
State-Industry-Year FE	Y	Y	Y	Y	Y
Firm-Year FE	N	N	Y	Y	N
Observations	255,541	255,541	259,054	259,054	257,889
Adjusted R ²	0.143	0.153	0.146	0.159	0.146

This table presents results from estimating alternative specifications of equation (1). Observations are at the firm-state-year level, and the sample period ranges from 2004 to 2016. In columns 1 and 3, the dependent variable is an indicator for whether firm *i* was sanctioned for corporate misconduct in state *j* in year *t*; in columns 2 and 4, the dependent variable is the natural logarithm of the dollar value of penalties assessed for corporate misconduct in state *j* in year *t*. See appendix A for variable definitions. The primary independent variable of interest in columns 1 and 2 is an indicator labeled *ConnectedSub_{ijt}*, which equals 1 if firm *i* received a subsidy prior to or during year *t* while the governor of state *j* was in power; in columns 3 and 4, it is the natural logarithm of the running total of subsidies awarded to firm *i* in state *j* while the governor of state *j* was in power. All specifications include firm-state and state-industry-year fixed effects; columns 3 and 4 additionally include firm-year fixed effects. Standard errors are double-clustered by firm and industry-year. Estimated *t*-statistics are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

column 5, evidence of such a “run-up”; misconduct largely commences during and after the subsidy year. I plot coefficients corresponding to column 5 in figure 1 for the three years prior to the three years after a subsidy. This result also suggests that, although state governments may be aware of subsidy recipients’ postsubsidy shifts in the likelihood of misconduct, governments are not providing subsidies to firms that already exhibit a proclivity for misconduct.

4.3 STATE-LEVEL ENFORCEMENT

The plausible exogeneity of federal enforcement practices to state-level political connections suggests that the results in table 5 reflect a higher underlying corporate misconduct rate for subsidy recipients. In this section, I consider the most likely mechanism underlying this result: lenient state-level enforcement. Because non-securities-related misconduct is punishable at both state and federal levels, the total cost of engaging in misconduct is a function of both state- and federal enforcement-related costs. Because of dual sovereignty, these two costs are de jure additive. As long as these two costs are not de facto substitutes—that is, as long as state enforcement agencies do not divert investigative resources or sanctions away from firms already under federal investigation—then a reduction in state-level enforcement may make firms willing to engage in higher levels of underlying misconduct. This result would be consistent with tables 5 and 6.

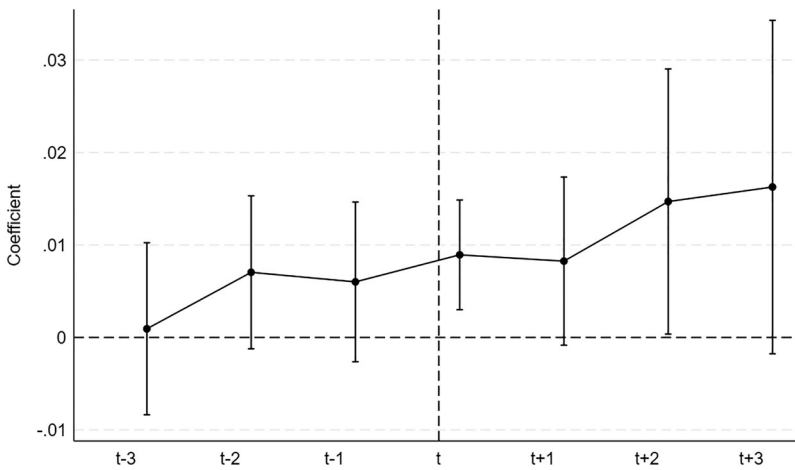


FIG 1.—Coefficient plot: subsidies and misconduct, relative to connection formation. This figure plots coefficients corresponding to column 5 of table 6, with time periods on the x-axis reflecting the year relative to the time a subsidy-related connection was awarded.

Although Violation Tracker does not yet contain comprehensive state-level enforcement data, limited data are available for a handful of states. Using a subsample comprising states with relatively more complete state agency data (California, Illinois, Kentucky, Massachusetts, New York, and Washington), I attempt to verify the assertion given above by testing whether subsidy receipt is associated with state-level leniency.²⁵

Results from estimating the model described above are in table 7. In all columns, the dependent variable is $StateViolation_{ijt}$, an indicator for whether firm i received sanctions for at least one instance of corporate misconduct by *state* agencies in state j in year t . I first verify, in column 1, that there does not appear to be a substitution effect. The primary independent variable in this column is $FederalViolation_{ijt}$, an indicator for whether firm i was sanctioned by federal agencies for misconduct committed in state j in years $t - 1$ or t . I construct this variable using both years to explicitly account for the possibility that state agencies exhibit leniency in response to a firm already receiving federal sanctions, rather than in response to political connections.²⁶ The positive and significant coefficient on $FederalViolation_{ijt}$ does not support this type of substitution effect.

I next turn to the relation between state-level subsidies and state enforcement actions. In column 2, I estimate a modified version of equation (1)

²⁵ I assess “completeness” based on whether data exist during my sample period for all three of labor-, environmental-, and consumer protection-related offenses.

²⁶ All results in table 7 are unchanged, both qualitatively and in terms of statistical significance, if I instead construct $FederalViolation_{ijt}$ to reflect only violations committed in year t .

TABLE 7
State-Level Enforcement

	State-Level Enforcement Indicator				
Violation Variable:	(1)	(2)	(3)	(4)	(5)
Federal violation	0.030*** [5.87]		0.036*** [6.17]	0.025*** [4.13]	0.027*** [3.70]
<i>ConnectedSub_{jt}</i>		-0.002 [-0.61]	0.001 [0.28]	-0.002 [-0.35]	
Federal violation × <i>ConnectedSub_{jt}</i>			-0.025** [-2.46]	-0.023* [-1.85]	
<i>ConnectedOutO fStateSub_{jt}</i>					0.001 [0.70]
Federal violation × <i>ConnectedOutO fStateSub_{jt}</i>					0.005 [0.52]
<i>CorporateStatePAC_{jt}</i>	0.001 [0.14]	0.001 [0.14]	0.001 [0.16]	0.017 [1.38]	0.001 [0.14]
<i>CEStatePAC_{jt}</i>	-0.005 [-0.39]	-0.004 [-0.33]	-0.004 [-0.35]	0.002 [0.08]	-0.005 [-0.39]
<i>CorporateFederalPAC_{jt}</i>	0.002 [1.10]	0.002 [0.96]	0.002 [1.09]	-0.003 [-0.66]	0.002 [1.11]
<i>CEFederalPAC_{jt}</i>	-0.003** [-1.96]	-0.003* [-1.88]	-0.003* [-1.94]	-0.002 [-0.57]	-0.003** [-1.97]
<i>FederalLobbying_{jt}</i>	0.003* [1.93]	0.003** [2.07]	0.003* [1.89]	0.008* [1.79]	0.003* [1.93]
Log establishments	0.001 [1.05]	0.001 [1.27]	0.001 [1.06]	0.003 [1.19]	0.001 [1.04]
Log assets	-0.001 [-0.79]	-0.001 [-0.46]	-0.001 [-0.75]	-0.005 [-1.24]	-0.001 [-0.85]

(Continued)

TABLE 7—(Continued)

Violation Variable:	State-Level Enforcement Indicator				
	(1)	(2)	(3)	(4)	(5)
ROA	-0.004 [-0.55]	-0.003 [-0.36]	-0.004 [-0.56]	-0.014 [-0.68]	-0.004 [-0.56]
Sales growth rate	-0.000 [-0.20]	-0.001 [-0.28]	-0.000 [-0.21]	-0.000 [-0.04]	-0.000 [-0.21]
Market to book	0.000* [1.86]	0.000* [1.78]	0.000* [1.88]	0.000 [1.40]	0.000* [1.87]
Leverage	0.005 [1.40]	0.005 [1.26]	0.005 [1.37]	0.002 [0.14]	0.006 [1.40]
PP&E	-0.008 [-0.78]	-0.008 [-0.75]	-0.008 [-0.81]	0.000 [0.00]	-0.008 [-0.80]
Firm-State FE	Y	Y	Y	Y	Y
State-Industry-Year FE	Y	Y	Y	Y	Y
Observations	51,132	51,155	51,132	18,701	51,132
Adjusted R^2	0.084	0.079	0.084	0.065	0.084

This table presents results from estimating equation (2). Observations are at the firm-state-year level, and the sample period ranges from 2004 to 2016. In all columns the dependent variable is an indicator for whether firm i was sanctioned for corporate misconduct in state j in year t by state-level agencies. See appendix A for variable definitions. The primary independent variables of interest are an indicator labeled *Federal violation*, which equals 1 if firm i received at least one federal sanction for misconduct committed in state j in either year $t-1$ or t ; an indicator labeled *ConnectedSub_{ijt}*, which equals 1 if firm i received a subsidy prior to or during year t while the governor of state j was in power; in column 2; and the two aforementioned variables plus an interaction term *Prior federal violation* \times *ConnectedSub_{ijt}* in column 3. Column 4 reestimates the specification in column 3 for the subset of firm-years with violations in states other than j . Finally, column 5 estimates an alternative specification to column 3 that replaces *ConnectedSub_{ijt}* with *ConnectedOut_{OjStateSub_{ijt}}*, an indicator that equals one if the firm has subsidy-driven political connections to states other than j in year t . All specifications include firm-state and state-industry-year fixed effects. Standard errors are double-clustered by firm and industry-year. Estimated t -statistics are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

but replace the dependent variable with $StateViolation_{ijt}$. I find no link between $Connected_{ijt}$ and $StateViolation_{ijt}$, though this is not surprising; a simultaneous increase in firms' willingness to engage in misconduct and decrease in state (but not federal) enforcement intensity should yield exactly this result. Hence, to test for evidence of state-level leniency, I interact $FederalViolation_{ijt}$ with $Connected_{ijt}$. That is, I estimate

$$\begin{aligned}
 StateViolation_{ijt} = & \alpha_0 + \alpha_1 Connected_{ijt} + \alpha_2 FederalViolation_{ijt} \\
 & + \alpha_3 (Connected_{ijt} \times FederalViolation_{ijt}) + \alpha_4 Controls_{ijt} + \gamma_{ij} + \theta_{jkt} + \varepsilon_{ijkt}.
 \end{aligned}
 \tag{2}$$

Equation (2) accounts for shifts in the underlying violation rate; a negative α_3 indicates leniency. I find results consistent with this argument in column 3 of table 7. For robustness, in column 4, I reestimate equation (2) on the subset of firm-state-years with at least one violation in a state other than j in years t or $t - 1$. The firms in this sample have all engaged in misconduct; assuming the proclivity for misconduct is at least partially a firm-level trait, these firms are the most likely to have unpenalized misconduct in state j in year t . Hence, if the results in column 3 hold in this subsample, that would provide additional evidence of state-level leniency. Column 4 of table 7 supports this argument.

Another explanation for the findings in column 3 of table 7 is that subsidy-seeking firms may engage in different forms of misconduct to non-subsidy-seeking firms (e.g., subsidized firms may have worse financial performance and thus engage in forms of misconduct that yield faster boosts to income). If subsidized firms were more likely to engage in misconduct of particular interest to, for example, federal regulators, this would yield a different interpretation: Subsidized firms engage in misconduct that has higher risks of federal but not state, enforcement. I rule out this explanation in two ways. First, if the possible difference above is cross-sectional, it will be absorbed by firm-state fixed effects. However, there may be a temporal component inasmuch as firms may seek subsidies at some times but not others. If time-varying subsidy-seeking behavior drives the results in column 3 of table 7, then I should observe a similar result using a broader proxy for a firm's subsidy-seeking behavior in lieu of $ConnectedSub_{ijt}$. In column 5 of table 7, I therefore replace $ConnectedSub_{ijt}$ with $ConnectedOutOfStateSub_{ijt}$, an indicator that is constructed analogously to $ConnectedSub_{ijt}$ but using subsidies from states other than j . The interaction term in this specification is not significant, suggesting that any inherent differences between misconduct engaged in by subsidy-seekers and non-subsidy-seekers are unlikely to explain my results. In sum, the results in this section suggest that a key mechanism underlying the paper's main findings is leniency in state-level enforcement.

5. *Additional Tests*

5.1 INHERITED SUBSIDIES

Although unlikely due to my fixed effects structure, it may still be possible that the decision to seek subsidies and the decision to engage in corporate misconduct are linked by a correlated omitted factor varying within firm, state, and year. To overcome this possibility, in this section, I focus on a type of connection resulting from subsidies that firms did not directly seek: those arising from mergers and acquisitions. Because subsidies typically last for several years, a subsidy awarded to a firm will still be ongoing if that firm is acquired within a few years of receiving the subsidy. When this occurs, the acquiring firm inherits the subsidy—and, hence, the political and economic connection associated with the subsidy. To validate the paper's main findings, I test whether these plausibly exogenously obtained subsidies are associated with subsequent misconduct committed by the acquiring firm.

I first verify whether mergers appear to occur because of target-firm subsidies; if this is the case, then the argument surrounding plausible exogeneity of the inherited subsidies would not hold. Prior literature documents a link between target-firm political connections and merger outcomes. For example, Mehta, Srinivasan, and Zhao [2020] find that political connections can affect merger outcomes via more favorable antitrust reviews whereas Croci et al. [2017] find that politically connected target firms are less likely to be acquired. However, both of these studies rely on connections to federal politicians, with the connections' primary use being help with the federal antitrust process. It is thus unclear whether these arguments would generalize to the state setting. Other recent work suggests that firms may develop political connections that mimic those of investors who take large stakes (Bertrand et al. [2020b]), and an acquiring firm may represent an extreme example of this. However, the findings in Bertrand et al. [2020b] have implications for how a target firm behaves *post*-investment; it is unclear whether these findings would apply to a firm's preinvestment or acquisition behavior.

To assess the validity of my merger-based identification approach, I first test whether subsidies play a role in determining merger outcomes. I do so by estimating (i) a merger prediction model, where I test whether subsidy receipt is associated with a greater likelihood of being acquired; and (ii) a merger premium model, where I test whether subsidy receipt is associated with a merger premium. I measure merger premium as the ratio of the acquisition price to the target's stock price 30 trading days preacquisition. Control variables are selected based on prior merger literature (e.g., Jenter and Lewellen [2015], Marquardt and Zur [2015]).

I identify mergers using similar screens to Jenter and Lewellen [2015] and Mehta, Srinivasan, and Zhao [2020]. Specifically, I (i) omit divestments and nonmerger transactions such as recapitalizations, (ii) limit the sample to publicly traded targets so that I observe financial information, and (iii)

consider only cases where the acquirer obtained 100% ownership of the target. I also impose a fourth screen: I limit the sample to firms with at least one observation in *either* Violation Tracker or Subsidy Tracker. Although this removes many merger observations from my sample, this screen ensures that any results are not driven by measurement error in the independent variable of interest.

Results from the merger prediction and premium models are provided in panel A of table 8. In columns 1 and 2, I estimate a logistic regression model, controlling for industry and year fixed effects, where the dependent variable is an indicator for whether firm i was taken over during year $t + 1$. I use as the primary independent variable of interest either an indicator for whether the firm received state subsidies from any state in the two years prior to acquisition (column 1) or the natural logarithm of the dollar amount of subsidies received in these two years, collectively across all states (column 2). In both cases, I find that subsidy receipt is negatively associated with the likelihood of being taken over, suggesting that subsidies do not turn firms into more attractive acquisition targets. This result is consistent with Croci et al. [2017] and may arise for a similar reason to what they argue: Politicians may wish to “protect” their connected firms from being taken over.

In columns 3 and 4 of table 8, panel A, I estimate a merger premium model. The sample in these two columns is limited to the 279 merger firm-year observations that pass screens (i)–(iv) above. Crucially, I find no relation between the merger premium and subsidy receipt. Collectively, I interpret the results in panel A of table 8 as validation of the merger-based “inherited subsidy” approach.

To test whether firms are more likely to engage in corporate misconduct after inheriting a subsidy, I estimate the following regression model:

$$Misconduct_{ijkt} = \beta_0 + \beta_1 InheritedSub_{ijt} + \beta_2 Controls_{ijt} + \gamma_{ij} + \theta_{jkt} + \varepsilon_{ijkt}. \quad (3)$$

In equation (3), $InheritedSub_{ijt}$ is an indicator variable that takes the value of one if firm i inherited a subsidy through an acquisition in year t or prior while the current governor was still in power. In testing the model, I limit the sample to firm-state-year observations in a five- or seven-year window around the merger. That is, if firm i makes an acquisition in year θ , I include all firm-state-year observations corresponding to firm i in years $\theta - 2$ through $\theta + 2$ (or $\theta - 3$ through $\theta + 3$) but otherwise exclude firm-state-year observations corresponding to firm i as well as any firm-state-year observations corresponding to firms that did not inherit subsidies during my sample period. This approach means that equation (3) represents a difference-in-differences specification where the “control” group is the acquiring firm in states where the target did not have a premerger subsidy.

Results from estimating equation (3) are presented in panel B of table 8. Columns 1 and 2 use a window of $t - 2$ to $t + 2$ whereas columns 3 and 4 use a window of $t - 3$ to $t + 3$. The positive and significant coefficient on $InheritedSub_{ijt}$ in all columns is consistent with the paper’s main results;

TABLE 8
Inherited Subsidies

Panel A: Subsidies and merger outcomes				
Dependent Variable: Specification:	Firm Acquired Indicator		Merger Premium	
	Logit		Linear	
	(1)	(2)	(3)	(4)
Had subsidy last two years	-0.266** [-1.98]		0.060 [1.18]	
Log last two years' subsidy \$		-0.023** [-2.20]		0.005 [1.19]
Log market value	-0.084* [-1.86]	-0.079* [-1.71]	-0.095*** [-2.88]	-0.095*** [-2.90]
Market to book	-0.004 [-0.66]	-0.004 [-0.67]	0.001 [0.12]	0.001 [0.12]
ROA	0.341 [1.31]	0.333 [1.29]	-0.420 [-1.50]	-0.417 [-1.49]
Leverage	0.261 [0.97]	0.262 [0.97]	0.185 [0.89]	0.187 [0.90]
Sales growth rate	-0.446*** [-2.69]	-0.448*** [-2.69]	0.162 [1.28]	0.160 [1.26]
Annual returns	0.057 [0.96]	0.057 [0.95]	-0.062* [-1.69]	-0.062* [-1.67]
Log R&D	0.060* [1.65]	0.061* [1.68]	0.047** [2.24]	0.047** [2.25]
Industry FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	18,765	18,765	279	279
Adjusted/Pseudo R ²	0.035	0.035	0.060	0.060

Panel B: Inherited subsidies				
Violation variable: Violation location: Window about merger year:	Federal Violations			
	In-State			
	[t - 2, t + 2]		[t - 3, t + 3]	
Dependent variable type:	Indicator	Log Penalty \$	Indicator	Log Penalty
	(1)	(2)	(3)	(4)
<i>InheritedSub_{ijt}</i>	0.044** [2.30]	0.452** [2.35]	0.046*** [2.80]	0.475*** [2.80]
<i>CorporateStatePAC_{ijt}</i>	0.011 [0.19]	0.064 [0.11]	-0.001 [-0.04]	-0.034 [-0.09]
<i>CEOStatePAC_{ijt}</i>	-0.040 [-0.55]	-0.777 [-0.84]	0.139 [1.47]	1.125 [1.18]
<i>CorporateFederalPAC_{ijt}</i>	0.024 [1.03]	0.260 [1.07]	-0.001 [-0.10]	-0.027 [-0.21]
<i>CEOFederalPAC_{ijt}</i>	-0.005 [-0.87]	-0.074 [-1.45]	0.006 [0.90]	0.045 [0.69]

(Continued)

TABLE 8—(Continued)

Panel B: Inherited subsidies				
Violation variable:	Federal Violations			
Violation location:	In-State			
Window about merger year:	[$t - 2, t + 2$]		[$t - 3, t + 3$]	
Dependent variable type:	Indicator	Log Penalty \$	Indicator	Log Penalty
	(1)	(2)	(3)	(4)
<i>FederalLobbying</i>	0.010 [0.34]	0.139 [0.47]	-0.006 [-0.29]	-0.045 [-0.23]
Log establishments	0.002 [0.27]	0.011 [0.15]	0.006 [0.93]	0.063 [0.99]
Log assets	0.009 [0.73]	0.090 [0.74]	0.011 [1.16]	0.131 [1.31]
ROA	0.017 [0.17]	0.056 [0.05]	-0.037 [-0.47]	-0.653 [-0.84]
Sales growth rate	-0.013 [-0.92]	-0.067 [-0.46]	-0.022* [-1.93]	-0.156 [-1.28]
Market to book	0.001 [1.54]	0.009* [1.88]	0.001** [2.11]	0.014** [2.45]
Leverage	-0.025 [-0.58]	-0.271 [-0.65]	0.019 [0.38]	0.242 [0.49]
PP&E	-0.145 [-0.99]	-0.819 [-0.56]	-0.129 [-1.11]	-1.087 [-0.92]
Firm-State FE	Y	Y	Y	Y
State-Industry-Year FE	Y	Y	Y	Y
Observations	9,940	9,940	13,634	13,634
Adjusted R^2	0.109	0.111	0.117	0.119

This table presents results from estimating the relation between subsidies inherited through mergers and corporate misconduct. Panel A estimates merger prediction and premium models to assess whether firms' receipt of subsidies affects the likelihood of acquisition or the premium paid by the acquirer, whereas panel B estimates equation (3). In panel A, observations are at the firm-year level, whereas in panel B, observations are at the firm-state-year level. In both cases, the sample period ranges from 2004 to 2016. Estimated t -statistics are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. In columns 1 and 2 the dependent variable is an indicator for whether firm i was acquired in year t . Columns 1 and 2 estimate a logistic regression model of acquisition likelihood. In columns 3 and 4, the dependent variable is the merger premium, defined as the difference between the acquisition price and the 30-day preannouncement closing price, scaled by the latter. Columns 3 and 4 estimate a linear regression model. All specifications include industry and year fixed effects. Standard errors are clustered by firm. In columns 1 and 3 the dependent variable is an indicator for whether firm i was sanctioned for corporate misconduct in state j in year t ; in columns 2 and 4 the dependent variable is the natural logarithm of the dollar value of penalties assessed for corporate misconduct in state j in year t . See appendix A for variable definitions. The primary independent variable of interest is an indicator labeled *InheritedSub_{ijt}*, which equals 1 if firm i inherited a subsidy through an acquisition while the current governor was in office. The sample is restricted to symmetric windows around firm-years that inherited at least one subsidy in any state. In columns 1 and 2, the sample reflects the merger having occurred in [$t - 2, t + 2$] whereas in columns 3 and 4 the sample reflects the merger having occurred in [$t - 3, t + 3$], three years prior to the merger through around received a subsidy prior to or during year t while the governor of state j was in power; in columns 3 and 4, the dependent variable is the natural logarithm of the running total of subsidies awarded to firm i in state j while the governor of state j was in power. All specifications include firm-state and state-industry-year fixed effects. Standard errors are double-clustered by firm and industry-year.

firms appear to behave similarly with respect to corporate misconduct after inheriting subsidies (and the associated political connections) as when they directly obtain the subsidies.

5.2 POLITICALLY UNALIGNED LEGISLATORS AND ENFORCERS

To further examine the mechanism underlying the paper's main results, I consider the role of state-level enforcement officials. Although most officials with enforcement power (e.g., the labor commissioner or environmental commissioner) are typically appointed by the governor's office, an exception is the state attorney general. The attorney general, the state's top enforcement official, is directly elected in 43 out of 50 states (Nolette [2014]). Hence, in many states, the attorney general and governor are of different political parties. State attorneys general frequently also use their position as a springboard to run for higher office (Nolette [2015]).²⁷ When the attorney general is not politically aligned with the governor, postsubsidy state-level enforcement may not weaken since the attorney general has little incentive to "protect" firms connected to the gubernatorial administration. As a result, the gubernatorial administration may face higher political costs of leniency.

I test this possibility in column 1 of table 9, where I interact $ConnectedSub_{ijt}$ with a new variable $AGGovOpp_{jt}$ that equals one for state-years where the attorney general and governor are of opposing political affiliations. $AGGovOpp_{jt}$ takes the value of one in 31% of firm-state-years. The main effect of $ConnectedSub_{ijt}$ continues to be positive and significant. However, I find a negative and significant coefficient on $AGGovOpp_{jt}$, consistent with the argument in the preceding paragraph: When state-level law enforcement is less aligned with the governor, the effect of subsidies on corporate misconduct attenuates.

To test the robustness of the result above, I next consider the political orientation of other state-level officials with enforcement power. Although consumer protection cases are typically handled by the attorney general (although the attorney general's role is not limited, de jure or de facto, to consumer protection cases), violations of state labor and environmental laws are enforced by state-level labor commissions and environmental or resource commissions.²⁸ I construct a variable $CommissionerOpp_{jt}$, which equals one if any of the labor, agriculture, or natural resource commissioners in a state are of the opposing political party to the governor (which occurs for 11% of firm-state-year observations). In column 2 of table 9, I then interact $ConnectedSub_{ijt}$ with $CommissionerOpp_{jt}$. I find results that are similar, although statistically weaker, to those in column 1, perhaps because such commissioners are appointed by the governor

²⁷ For example, in the 2010 election cycle, 10 states' incumbent attorneys general ran for governor.

²⁸ All 50 states have a labor commissioner, whereas environmental violations are handled through a mix of environmental, agricultural, and natural resource offices.

TABLE 9
Cross-Sectional Variation in Enforcement

Violation Variable:	Federal Violations	
	In-State	
Violation Location:	(1)	(2)
<i>ConnectedSub_{ijt}</i>	0.010*** [3.55]	0.009*** [3.30]
<i>ConnectedSub_{ijt} × AGGovOpp_{jt}</i>	-0.010** [-2.03]	
<i>ConnectedSub_{ijt} × CommissionerOpp_{jt}</i>		-0.009* [-1.68]
<i>CorporateStatePAC_{ijt}</i>	-0.001 [-0.12]	-0.001 [-0.12]
<i>CEOSTatePAC_{ijt}</i>	-0.005 [-0.35]	-0.005 [-0.35]
<i>CorporateFederalPAC_{ijt}</i>	-0.001 [-0.67]	-0.001 [-0.69]
<i>CEOFederalPAC_{ijt}</i>	0.002** [2.17]	0.002** [2.18]
<i>FederalLobbying</i>	0.002 [1.39]	0.002 [1.39]
Log establishments	0.003*** [3.11]	0.003*** [3.11]
Log assets	0.007*** [4.24]	0.007*** [4.23]
ROA	0.010* [1.66]	0.010* [1.66]
Sales growth rate	-0.003 [-1.27]	-0.003 [-1.28]
Market to book	-0.000 [-0.49]	-0.000 [-0.49]
Leverage	-0.006 [-1.31]	-0.006 [-1.30]
PP&E	0.005 [0.55]	0.005 [0.55]
Firm-State FE	Y	Y
State-Industry-Year FE	Y	Y
Observations	257,889	257,889
Adjusted R ²	0.146	0.146

This table presents results from tests of cross-sectional variation in enforcement. Observations are at the firm-state-year level, and the sample period ranges from 2004 to 2016. In both columns, the dependent variable is an indicator for whether firm *i* was sanctioned for corporate misconduct in state *j* in year *t*. See appendix A for variable definitions. Column 1 interacts *ConnectedSub_{ijt}* with *AGGovOpp_{jt}*, which equals 1 if the governor and attorney general are of different political parties in state *j* in year *t*. Column 2 interacts *ConnectedSub_{ijt}* with *CommissionerOpp_{jt}*, which equals 1 if any of the labor, natural resources, or agriculture commissioners in state *j* in year *t* is of a different party than the governor. Standard errors are double-clustered by firm and industry-year. Estimated *t*-statistics are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

in most states.²⁹ Collectively, my results support the primary mechanism underlying the paper's main findings: that the link between subsidy receipt and subsequent misconduct by recipient firms may be driven by state-level leniency in enforcement.

5.3 FEDERAL AND STATE GOVERNMENT

I next examine potential variation in federal enforcement. I first consider whether the federal government is politically “misaligned,” from a subsidy recipient's perspective, with the state awarding the subsidy. If the state and federal governments are misaligned, an increase in postsubsidy federal misconduct enforcement could reflect the federal government treating firms connected to the opposition party more harshly rather than state-level leniency. I define misalignment as cases where (i) the federal administration is of the opposite party to the state government in state j and (ii) the firm has not made federal PAC contributions to the party in power. I include condition (ii) because prior work (e.g., Correia [2014]) finds that contributions to federal politicians can favorably affect federal enforcement outcomes, which may offset the effect of federal-state differences. I define a new variable *StateFederalOpp_{ijt}* that equals one for misaligned firm-state-years, which I interact with *ConnectedSub_{ijt}* in column 1 of table 10. The insignificant coefficient on the interaction term suggests that my results are not driven by changes in federal enforcement.³⁰

As a second test of variation in federal enforcement, I consider whether the federal government is unified (i.e., the House, Senate, and presidency are all controlled by the same party) or divided. A unified government has more power over nominees to run the agencies overseeing corporate misconduct (e.g., the EPA or OSHA) and so may be more easily able to shift enforcement patterns. In column 2 of table 10, I interact *ConnectedSub_{ijt}* with *UnifiedGovt_t*, an indicator for periods of unified government. I find no evidence that subsidized firms face different enforcement patterns under unified governments. Finally, in column 3, I examine the possibility that misalignment may still matter under unified governments. I find no evidence that this is the case.

5.4 FEDERAL SUBSIDIES

Like state governments, the federal government can also award subsidies. I next examine whether federal subsidies relate to corporate misconduct.

²⁹ Labor commissioners are elected in 4 states, agriculture commissioners in 11, natural resource commissioners in 5, and environmental commissioners in 0. Moreover, the enforcement power of these offices varies by state (whereas attorneys general have similar, expansive enforcement powers across all 50 states).

³⁰ In an additional untabulated analysis, I extend my sample to the period 2017–19 for the 10 most common states in my sample. Doing so allows me to examine whether this effect differs during the Trump period, when the federal agencies that detect and penalize firms for misconduct may have been accused of politicizing their decisions. However, I continue to find that state-federal alignment does not affect my results even during the 2017–19 period.

TABLE 10
State-Federal Alignment

Violation Variable:	Federal Violations		
	In-State		
Violation Location:	(1)	(2)	(3)
<i>ConnectedSub_{ijt}</i>	0.008*** [2.68]	0.009*** [3.34]	0.009*** [2.75]
<i>ConnectedSub_{ijt} × StateFederalOpp_{ijt}</i>	0.000 [0.07]		0.001 [0.23]
<i>ConnectedSub_{ijt} × UnifiedGovt_t</i>		-0.005 [-1.14]	-0.004 [-0.73]
<i>ConnectedSub_{ijt} × UnifiedGovt_t × StateFederalOpp_{ijt}</i>			-0.006 [-0.69]
<i>StateFederalOpp_{ijt}</i>	0.000 [0.10]		-0.000 [-0.26]
<i>UnifiedGovt × StateFederalOpp_{ijt}</i>			0.002 [0.97]
<i>CorporateStatePAC_{ijt}</i>	-0.001 [-0.11]	-0.001 [-0.11]	-0.001 [-0.11]
<i>CEOStatePAC_{ijt}</i>	-0.005 [-0.36]	-0.005 [-0.35]	-0.005 [-0.35]
<i>CorporateFederalPAC_{ijt}</i>	-0.001 [-0.60]	-0.001 [-0.67]	-0.001 [-0.62]
<i>CEOFederalPAC_{ijt}</i>	0.002** [2.14]	0.002** [2.18]	0.002** [2.15]
<i>FederalLobbying</i>	0.002 [1.39]	0.002 [1.39]	0.002 [1.39]
Log establishments	0.003*** [3.12]	0.003*** [3.10]	0.003*** [3.11]
Log assets	0.007*** [4.24]	0.007*** [4.24]	0.007*** [4.26]
ROA	0.010* [1.66]	0.010* [1.67]	0.010* [1.68]
Sales growth rate	-0.003 [-1.28]	-0.003 [-1.27]	-0.003 [-1.27]
Market to book	-0.000 [-0.49]	-0.000 [-0.51]	-0.000 [-0.52]
Leverage	-0.006 [-1.31]	-0.006 [-1.31]	-0.006 [-1.33]
PP&E	0.005 [0.56]	0.005 [0.56]	0.005 [0.57]
Firm-State FE	Y	Y	Y
State-Industry-Year FE	Y	Y	Y
Observations	257,889	257,889	257,889
Adjusted R ²	0.146	0.146	0.146

This table presents results from tests of cross-sectional variation state-federal alignment. Observations are at the firm-state-year level, and the sample period ranges from 2004 to 2016. In all columns, the dependent variable is an indicator for whether firm *i* was sanctioned for corporate misconduct in state *j* in year *t*. See appendix A for variable definitions. Column 1 interacts *ConnectedSub_{ijt}* with *StateFederalOpp_{ijt}*, which equals 1 if the firm does not have any political connections to a federal government of the opposing party to the state administration. Column 2 interacts *ConnectedSub_{ijt}* with *UnifiedGovt_t*, which equals 1 if the federal government is unified in year *t*. Column 3 interacts *ConnectedSub_{ijt}* with both *StateFederalOpp_{ijt}* and *UnifiedGovt_t* (as well as lower-order interactions). Standard errors are double-clustered by firm and industry-year. Estimated *t*-statistics are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

There are several structural differences between state and federal subsidies, and hence it is not clear whether my findings would generalize to federal subsidies. For instance, the federal government can only award cash grants and not longer-term targeted tax breaks. Moreover, the federal government may not have the same incentives for leniency as state governments do. In awarding subsidies, state governments seek for recipient firms to create jobs and do business in their states rather than in others. The relative ease with which firms can shift operations across state lines may therefore induce leniency by state governments. Conversely, it is much more difficult to shift business operations out of the country (operationally, not just in a tax or legal sense) and, hence, the federal government may not have the same incentives to display leniency to economically connected firms.

In columns 1 and 2 of table 11, I reestimate equation (1) but replace $ConnectedSub_{ijt}$ with $ConnectedFederalSub_{ijt}$, an indicator for whether the firm has received a federal subsidy from the current administration. I do not find any evidence that federal subsidies relate to misconduct. In columns 3 and 4, I consider both state and federal subsidies. I continue to find both (i) a relation between state subsidies and misconduct and (ii) a lack of any relation between federal subsidies and misconduct. Collectively, my findings suggest that state—but not federal—subsidies relate to corporate misconduct.

5.5 FIRMS' WILLINGNESS TO ENGAGE IN MISCONDUCT

My final set of tests examines variation in firms' potential willingness to engage in nonfinancial misconduct subsequent to receiving a subsidy. A firm's behavior in a state subsequent to receiving a subsidy from that state may be driven in part by broader firm-level factors, including compliance culture. More specifically, firms with a demonstrated willingness to cut corners or take actions that ultimately result in violations of federal laws may be more likely to then take actions that increase violation rates after a state subsidy, relative to those with a more robust compliance culture. In this section, I directly test this assertion.

To measure compliance culture for firm i in state j in year t , I use the natural logarithm of one plus the dollar value of all penalties received by the firm in all states other than j in the preceding two years. I then interact this variable with $ConnectedSub_{ijt}$ in a modified version of equation (1). I find that my results are stronger for firms with a weaker compliance culture (as evidenced by more firm-wide fines in the past two years). For brevity, I do not tabulate these results.

6. Conclusion

I study the relation between firms' receipt of subsidies and the likelihood that they subsequently engage in misconduct, as reflected in violations of

TABLE 11
Federal Subsidies

Violation Variable: Violation Location: Dependent Variable Type:	Federal Violations			
	In-State			
	Indicator (1)	Log Penalty \$ (2)	Indicator (3)	Log Penalty (4)
<i>ConnectedFederalSub_{ijt}</i>	0.003 [1.35]	0.024 [1.28]	0.002 [1.34]	0.023 [1.27]
<i>ConnectedSub_{ijt}</i>			0.008*** [3.06]	0.073*** [2.84]
<i>CorporateStatePAC_{ijt}</i>	-0.000 [-0.08]	-0.009 [-0.13]	-0.001 [-0.11]	-0.011 [-0.16]
<i>CEOStatePAC_{ijt}</i>	-0.005 [-0.35]	-0.062 [-0.45]	-0.005 [-0.36]	-0.063 [-0.45]
<i>CorporateFederalPAC_{ijt}</i>	-0.001 [-0.58]	-0.009 [-0.52]	-0.001 [-0.61]	-0.009 [-0.55]
<i>CEOFederalPAC_{ijt}</i>	0.002** [2.19]	0.024** [2.13]	0.002** [2.19]	0.024** [2.13]
<i>FederalLobbying</i>	0.002 [1.29]	0.026 [1.53]	0.002 [1.29]	0.026 [1.53]
Log establishments	0.003*** [3.15]	0.026*** [3.17]	0.002*** [3.11]	0.025*** [3.13]
Log assets	0.007*** [4.24]	0.070*** [4.23]	0.007*** [4.17]	0.069*** [4.16]
ROA	0.010* [1.72]	0.112* [1.82]	0.010* [1.72]	0.112* [1.82]
Sales growth rate	-0.003 [-1.29]	-0.031 [-1.20]	-0.003 [-1.27]	-0.030 [-1.19]
Market to book	-0.000 [-0.49]	-0.000 [-0.46]	-0.000 [-0.50]	-0.000 [-0.47]
Leverage	-0.047 [-1.35]	-0.006 [-1.11]	-0.046 [-1.33]	-0.046 [-1.09]
PP&E	0.006 [0.59]	0.061 [0.64]	0.005 [0.57]	0.059 [0.63]
Firm-State FE	Y	Y	Y	Y
State-Industry-Year FE	Y	Y	Y	Y
Observations	257,889	257,889	257,889	257,889
Adjusted R ²	0.146	0.157	0.146	0.157

This table considers the potential link between federal subsidies and corporate misconduct. Observations are at the firm-state-year level, and the sample period ranges from 2004 to 2016. In columns 1 and 3, the dependent variable is an indicator for whether firm *i* was sanctioned for corporate misconduct in state *j* in year *t*; in columns 2 and 4, the dependent variable is the natural logarithm of the dollar value of penalties assessed for corporate misconduct in state *j* in year *t*. See appendix A for variable definitions. The primary independent variable of interest is *ConnectedFederalSub_{ijt}*, an indicator that equals 1 if firm *i* received a subsidy prior to or during year *t* under the current federal administration. All specifications include firm-state and state-industry-year fixed effects. Standard errors are double-clustered by firm and industry-year. Estimated *t*-statistics are in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

laws protecting nonfinancial stakeholders (e.g., consumers, employees, or the environment). Understanding how subsidies may facilitate corporate misconduct is important due to both increasing interest in how taxpayer funds are spent and recent interest in stakeholder capitalism by asset managers and firms (e.g., BlackRock CEO Larry Fink's recent annual letters to firms emphasizing the importance of environmental and social practices).

Using a comprehensive firm-state-year panel, I find that firms receiving state-level subsidies are more likely to engage in corporate misconduct, measured using plausibly exogenous federal enforcement records, while the awarding state gubernatorial administration remains in power. Dual sovereignty—legally permissible simultaneous state and federal misconduct enforcement that does not violate double jeopardy laws—enables me to identify the mechanism underlying my findings as lenient state-level enforcement. My results are robust to several alternative specifications and are supported by a variety of placebo tests. My results are not driven by the presence of firms' political contributions and lobbying efforts, suggesting a distinction between *ex ante* attempts to develop political connections and the *ex post* realization of political connections between firm and state.

My study has implications for states' practice of awarding economic development subsidies as a means of growing or retaining business activity. Although the issue of whether these subsidies are ultimately a net positive for the state is beyond the scope of this paper, my study highlights a potential adverse consequence of subsidies when awarding officials may be willing to ignore statutory provisions in order to maximize job creation. My findings thus suggest potential stakeholder benefits to including, and strengthening enforcement of, corporate conduct-related provisions in subsidies.

APPENDIX A: VARIABLE DEFINITIONS

The table below presents definitions of variables used in the paper's empirical analyses.

Variable	Definition
$Misconduct_{i,t}$	Indicator variable that equals 1 if firm i faced federal sanctions, from any of the 54 agencies with violation data covered in Violation Tracker, for misconduct that it committed in state j in year t . Violations without a location given (typically violations deemed to be "firm-wide" rather than having occurred in any specific location, such as securities violations) are excluded.

Variable	Definition
<i>ConnectedSub_{ijt}</i>	Indicator variable that equals 1 if firm <i>i</i> has received a subsidy from state <i>j</i> in year <i>t</i> or prior and while the current governor of state <i>j</i> was in office. For example, if the governor of State <i>s</i> is in office from 2007–10 and Firm <i>x</i> receives a subsidy in 2008, then <i>ConnectedSub_{xst}</i> equals 0 for <i>t</i> = 2007, 1 for <i>t</i> ∈ {2008, 2009, 2010}, and 0 for <i>t</i> = 2011.
<i>CorporateStatePAC_{ijt}</i>	Indicator variable that equals 1 if firm <i>i</i> made political contributions to any state officials (governor, state House or Senate, elected commissioners, etc.) while current governor of state <i>j</i> was in power in or prior to year <i>t</i> .
<i>CEOStatePAC_{ijt}</i>	Indicator variable that equals 1 if firm <i>i</i> 's CEO made political contributions to any state officials (governor, state House or Senate, elected commissioners, etc.) while current governor of state <i>j</i> was in power in or prior to year <i>t</i> .
<i>CorporateFederalPAC_{it}</i>	Indicator variable that equals 1 if firm <i>i</i> made political contributions to any federal candidates for office while current federal administration was in power in or prior to year <i>t</i> .
<i>CEOFederalPAC_{it}</i>	Indicator variable that equals 1 if firm <i>i</i> 's CEO made political contributions to any federal candidates for office while current federal administration was in power in or prior to year <i>t</i> .
<i>FederalLobbying_{it}</i>	Indicator variable that equals 1 if firm <i>i</i> lobbied the administration currently in power in or prior to year <i>t</i> .
Log establishments	Natural logarithm of total business establishments firm has in a given state
Log assets	Natural logarithm of firm's total assets
ROA	Ratio of net income to lagged assets
Sales growth rate	Year-over-year change in sales divided by lagged sales
Market to book	Ratio of firm's market value of equity to book value of equity
Log market value	Natural logarithm of firm's market value of equity
Annual returns	Fiscal-year buy and hold stock returns in the last full fiscal year preceding a merger announcement.
Log R&D	Natural logarithm of one plus firm's research and development expenditures; set to zero when missing.
Leverage	Ratio of long-term debt to assets
PP&E	Ratio of property, plant, and equipment to assets
<i>StateViolation_{ijt}</i>	Indicator variable that equals 1 if firm <i>i</i> faced state-level sanctions from any state-level agencies (including lawsuits brought by the attorney general) in six states with sufficient state-level data (California, Illinois, Kentucky, Massachusetts, New York, and Washington) in state <i>j</i> in year <i>t</i> .
Firm acquired indicator	Indicator that equals one if firm acquired in year <i>t</i> + 1 (to match to firm's last set of premerger financial statements published in year <i>t</i>).
Merger premium	Ratio of merger price to stock price 30 trading days prior to merger announcement, minus 1.
<i>InheritedSub_{ijt}</i>	Indicator variable that equals 1 if firm <i>i</i> inherited a subsidy through a merger in state <i>j</i> in or prior to year <i>t</i> , where the subsidy was granted under the administration of the current governor of state <i>j</i> .

Variable	Definition
$AGGovOpp_{jt}$	Indicator variable that equals 1 if attorney general and governor are from opposing political parties in state j in year t .
$CommissionerOpp_{jt}$	Indicator variable that equals 1 if governor and at least one elected commissioner with enforcement power (labor, natural resources, or agriculture) are from opposing political parties in state j in year t .
$ElectionYear_{jt}$	Indicator variable that equals 1 if it is an election year in state j in year t .
$CompetitiveElection_{jt}$	Indicator variable that equals 1 if it is a competitive election year in state j in year t . To identify competitive elections I first calculate contributions to the losing party's candidate for governor (a) and contributions to the winning party's candidate for governor (b); a competitive election is then one for which the ratio $\frac{a}{b}$ is in the top quartile relative to all election years.
$StateFederalOpp_{jt}$	Indicator variable that equals 1 if federal administration in power is of the opposition party to the governor of state j in year t , and firm i did not make political contributions to federal politicians of the party currently in power.
$UnifiedGov_t$	Indicator variable that equals 1 if U.S. Senate, House, and presidency are all controlled by the same party in year t .
$ConnectedFederalSub_{it}$	Indicator variable that equals 1 if firm i has received a subsidy from the federal administration currently in power.

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