Corruption and Cash Policy:

Evidence from a Natural Experiment*

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Abstract:
We study the effects of a 2016 U.S. Supreme Court decision that made it harder for prosecutors to bring corruption cases against public servants. We argue that this exogenous shock to anticorruption enforcement created a "protection racket": regulated firms headquartered in high-corruption states increased cash reserves in the years after the decision, presumably to make illicit payments to local politicians. These firms experienced negative abnormal returns near the decision, indicating that reduced anticorruption enforcement decreased firm value. Consistent with the protection hypothesis, regulated firms in high-corruption states became less likely to be penalized by government agencies after the decision.

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1. Introduction

> "[O]ur concern is not with tawdry tales of Ferraris, Rolexes, and ball gowns. It is instead with the broader legal implications of the Government's boundless interpretation of the federal bribery statute."
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> –Chief Justice John Roberts, *McDonnell v. United States*

On June 27, 2016, the Supreme Court dramatically changed anticorruption law and enforcement in the United States. In *McDonnell v. United States*, the Court reversed the corruption conviction of the former Governor of Virginia and considerably constricted the legal definition of "bribery."[1] As a result, several corruption cases were decided in favor of defendants, and federal and state prosecutors declined to bring many anti-bribery cases that would have been filed pre-*McDonnell*. *McDonnell* represents a unique, exogenous decrease in the probability of corruption enforcement in the United States. In this paper, we argue that this decrease in anticorruption efforts led to the creation of a "protection racket": heavily regulated firms in high-corruption states increased their cash reserves after *McDonnell*, presumably to bribe local politicians. We hypothesize that these companies are the most likely to be affected by increased bribery since they are dependent on government connections and are located in states with high levels of public corruption. We evaluate how the post-*McDonnell* protection racket affected cash policy for these companies and whether this change was ultimately beneficial or harmful for firm value.

It is not ex ante obvious whether reduced antibribery enforcement would be value-increasing or destroying for affected firms since our results show that it carries both costs and benefits. The primary cost we identify for regulated firms in high-corruption areas post-*McDonnell* is that they carry excess cash on their balance sheets, presumably to bribe local politicians and regulators. Simply holding more cash to buy protection, rather than to pursue profitable growth opportunities, could hurt firm value and shareholder welfare. However, we also find a benefit for these companies after the *McDonnell* decision: consistent with the protection hypothesis, they became less likely to be penalized by government agencies after the decision. More specifically, local agencies in corrupt states are less likely to penalize heavily regulated firms after *McDonnell*, while the probability of federal agency enforcement remains the same.

[1] We explain the institutional setting and legal background of the *McDonnell* case in further detail in Section 2.

Overall, we find evidence that the costs of the protection racket outweigh its benefits for regulated firms in high-corruption areas. These firms experienced negative abnormal returns around the time of the Supreme Court decision, indicating that net firm value thus decreased due to reduced anticorruption enforcement. We also find some evidence that the value of corporate cash holdings decreased immediately after *McDonnell*. These findings suggest that the costs of bribery exceed the benefits of reduced government enforcement for the firms most vulnerable to political expropriation. Therefore, in addition to establishing the causal effect of corruption on cash policy, we are able to estimate that the costs of bribery-related political expropriation exceed any benefits enjoyed by affected firms.

Our results persist in a battery of robustness tests. We use two different measures of firm regulation in all our specifications and include firm fixed effects. Our findings are unchanged in specifications allowing for industry-specific or state-specific time trends. Placebo and pretrend tests reveal that the increase in cash ratio for regulated firms in high-corruption areas is concentrated in the aftermath of *McDonnell* rather than alternative time periods. The results also cannot be explained by the election of Donald Trump in the presidential election more than four months after the court case, since affected companies (regulated firms in high-corruption areas) showed negative abnormal returns after *McDonnell* but not after the 2016 election, increase in cash ratios appears uncorrelated with regulated industries being located in states with high levels of support for Trump, and the slowdown in government enforcement is restricted to state and local agencies (over which Trump, as head of the *federal* government, had little or no control). The findings persist when we rerun our specifications after matching regulated firms with comparable firms from less regulated industries. We see no evidence that affected firms used excess cash after *McDonnell* to gain political influence through official legislative lobbying rather than illicit payments. Finally, we find evidence suggesting that regulated firms in high-corruption states could be hiding increased levels of illicit payments after *McDonnell* by misclassifying them as operating expenses. This is consistent with forensic accountants' findings about public firms concealing illegal bribes as miscellaneous operating expenditures.

We should emphasize that, unfortunately, it is impossible for us to obtain data about the amounts of bribes actually paid by firms to politicians and regulators. However, the fact that our results are highly specific to regulated firms in high-corruption areas, which are the companies most vulnerable to corruption and expropriation, strongly supports the protection racket hypothesis. Furthermore, we hand-code court decisions citing the federal antibribery statute for the pre-*McDonnell* and post-*McDonnell* periods. Using judicial opinions and court filings, we find that the amounts of bribes allegedly taken by elected officials and regulators have been significantly higher since the Supreme Court's 2016 decision, even as federal prosecutors have brought far fewer antibribery cases. This analysis of court decisions suggests either that the average bribes exacted by U.S. officeholders have risen after *McDonnell* reduced antibribery enforcement or that the federal prosecutors are only able to target especially egregious cases since 2016, allowing other previously penalized bribery to go unchecked. Either of these interpretations is consistent with the development of a protection racket after *McDonnell*.

This paper uses a novel identification strategy centered on the *McDonnell* decision and adds to many areas of scholarship. First, the paper contributes to the literature on corruption. Several papers have examined the effect of corruption on corporate performance. This literature has found that higher levels of public corruption are associated with lower firm innovation (Ellis et al. 2020, Huang and Yuan 2021, Lu et al. 2022), higher corporate tax avoidance (Al-Hadi et al. 2022), and an increased likelihood of earnings restatements (Jha et al. 2021). Fewer papers have examined the relationship between corruption and cash policy. Ex ante, one could imagine firms vulnerable to political expropriation could *decrease* cash ratios when corruption becomes more likely since they want to avoid losing assets to public officials. Indeed, this is what the papers on this topic have argued. Caprio et al. (2013), using cross-country data, find a negative correlation between cash holdings and the threat of political extraction in a firm's home nation. Closest to our setting, Smith (2016) examines domestic US data and finds that firms located in more corrupt areas hold less cash.

These previous studies, however, did not have the advantage of an exogenous shock to corruption levels, and thus cannot address the possibility that firms endogenously headquarter in areas with different levels of corruption, or that corruption correlates with local demographic and economic factors. We use the sudden shift in anticorruption law and policy brought about by *McDonnell* and find a result opposite to that of Caprio et al. (2013) and Smith (2016): firms most affected by an increase in bribery *increase* their cash ratios to presumably buy political protection. We also assess both sides of the cost-benefit ledger when estimating the effects of reduced anticorruption enforcement: firms vulnerable to political expropriation had to keep excess cash to presumably bribe officials but also benefitted from reduced government enforcement. Our event study results allow us to conclude that regulated firms in high-corruption areas suffered a *net* decrease from participating in the post-*McDonnell* protection racket. Furthermore, we find some evidence that the value of corporate cash holdings decreased immediately after the Supreme Court's decision. Therefore, while it is theoretically plausible that affected firms respond to reduced corruption enforcement by reducing cash holdings, we instead find that they participate in a protection racket and increase cash ratios. Furthermore, while reduced anticorruption enforcement has both costs ad benefits, we find that affected firms suffered a net decrease in value by participating in the protection racket.

Second, the paper adds to work on regulated industries and their strong dependence on government authorities. Industries such as transportation, utilities, and communications, have been especially heavily regulated by U.S. governments, with special statutes and regulations about these sectors dating back to the Interstate Commerce Act of 1887 (Kearney and Merrill 1998). Firms operating in these industries hence need more permissions, ongoing relationships, and goodwill from their local regulators and elected officials. Colonnelli and Prem (2022) find that firms that are highly dependent on the government pay a large "corruption tax." Our findings, which show that, even in high-corruption states, it is firms in regulated industries that are affected by the sudden rise in the probability of corruption, are consistent with this corruption tax existing for firms dependent on political authorities. By focusing on the firms most vulnerable to political expropriation, we are able to estimate a more precise effect of corruption on cash policy than the current literature, which has looked at the entire cross-section of firms (Smith 2016). In our results, depending on which proxy of regulation we use, an increase in regulation as well as a one standard deviation increase in home state corruption is associated with a rise in cash ratio of 1.87-4.65% of the sample mean. This is a significant increase in cash ratio but is realistic given our claim that it only applies to regulated firms, which depend heavily on government contacts.

Third, this paper helps us understand the factors affecting the detection of corporate misconduct. Previous studies have examined how litigation (Johnson et al. 2007), whistleblowing (Dyck et al. 2010), social and professional ties between the CEO and directors (Khanna et al. 2015), and government crackdowns (Colonnelli and Prem 2022) affect the probability of managerial wrongdoing being detected. In this paper, we add a "protection racket" explanation for why corporate misconduct is detected at varying rates in different geographies and time periods. State authorities in high-corruption areas became more likely to demand (and receive) bribes without fearing anticorruption enforcement after *McDonnell*. In return, regulated firms located in these areas benefitted from fewer governmental investigations and penalties for their misconduct. Therefore, higher levels of corruption can hamper the detection of corporate malfeasance since government agencies responsible for penalizing companies may turn a blind eye in return for bribes.

Finally, this paper adds to the literature on governmental federalism and its effects on private enterprise. Scholars have advocated for subnational regions making their own laws, rather than having uniform rules for the entire nation, for both legal (Bickel 1970) and efficiency (Tiebout 1956) reasons. In the U.S. context, the ability of each state to set its own local policies has been described as facilitating policy innovation and a "race to the top," such as in the interjurisdictional competition for corporate charters (Romano 1993, Winter 1977). Our results present a less attractive feature of federalism in the United States: since some states have more entrenched cultures of corruption, a decline in anticorruption enforcement leads to capital structure distortions for regulated firms located in those areas. The federalism literature also notes that elected officials, regulators, and judges working for the federal government typically have better credentials, higher integrity, and greater competence than their state and local counterparts (Glaeser and Saks 2006, Neuborne 1976). Our results on the decline in government enforcement against regulated firms in high-corruption states are consistent with these observations: the decrease in enforcement is concentrated in actions by local agencies, not federal authorities. This could be because federal agencies are less corruptible or more willing to bring enforcement actions even after *McDonnell*.

The rest of this paper is structured as follows. Section 2 explains the institutional setting and legal background of the *McDonnell* case and its extensive effects on U.S. anticorruption law and policy. It also lays out our protection racket hypothesis, explaining why we should expect regulated firms in high-corruption areas to increase cash ratios after the Court's decision. Hand-coding court decisions applying the corruption statute before and after *McDonnell*, we find support for the protection racket hypothesis from actual litigation and government statistics. Section 3 describes the data used in the paper and presents summary statistics. Section 4 examines changes in cash policy of firms affected by *McDonnell*, finding an increase in cash ratio for these companies, per our argument. Section 5 shows that this increase in cash was inefficient for regulated companies in high-corruption areas: these firms exhibited negative abnormal returns around the time *McDonnell* was announced. We also find some evidence that the value of cash went down immediately after the decision. Section 6 explores the benefits firms received as part of the post-*McDonnell* protection racket in the form of a lower probability of government penalties for regulated firms in high-corruption areas. This reduction in enforcement was concentrated in state agencies in high-corrupt areas, and did not extend to federal authorities. Section 7 shows that our results are robust to a variety of robustness tests. Section 8 concludes the paper.

2. The *McDonnell* Decision and the Protection Racket Hypothesis

Bribery and corruption by public officials are criminal acts under both federal and state law everywhere in the United States. At the federal level, the most important statute governing bribery is 18 U.S.C. §201, which criminalizes any transfer of anything of value to influence an "official act" by a public official. While states also have their own anti-bribery statutes, federal prosecutors have significantly expanded the interpretation of national statutes after the Watergate scandal in the early 1970s, so federal laws are used to prosecute state and local corruption (Beale 2000). In 1984, the Supreme Court decided that 18 U.S.C. §201 could be used to prosecute state and local officials involved in bribery related to official acts.[2] Federal antibribery law, and specifically 18 U.S.C. §201, therefore play a central role in anticorruption efforts in the U.S.

In 2016, however, the Supreme Court suddenly changed the definition of "official act" in 18 U.S.C. §201, dramatically limiting the reach of this statute. In a case called *McDonnell v. United States*, the Court was presented with the case of Bob McDonnell, the former Governor of Virginia. Federal prosecutors accused McDonnell and his wife of receiving $177,000 from a local businessman in the form of loans, golf sessions, wedding gifts, designer clothes, and a Rolex watch. In exchange, prosecutors alleged, McDonnell arranged meetings for the businessman with other government officials, hosted events for the businessman's company at the governor's mansion, and called researchers at Virginia's state universities about initiating research studies testing the businessman's tobacco products. The government classified all these actions as "official acts" per the definition in 18 U.S.C. §201. The U.S. district and appellate courts agreed and sentenced McDonnell to two years in federal prison.

The Supreme Court overturned McDonnell's corruption conviction on June 27, 2016, and established a new definition of "official acts" under federal bribery law. Actions such as setting up meetings, organizing events for private parties, or calling other government officials on their behalf were no longer considered "official acts" under 18 U.S.C. §201, even if the public official was given large sums of money for performing them. An official act now must involve a public official who *makes a decision* related to his official position in exchange for a thing of value. Before *McDonnell*, prosecutors could characterize actions such as calling university researchers about starting tests as official acts that could be prosecuted under anticorruption laws. However, after *McDonnell*, prosecutors would have to additionally show that the defendant actively pressured other officials

[2] The case was *Dixson v. United States*, 465 U.S. 482 (1984).

(such as the university researchers) to make a decision (such as starting the tests), or made the decision themselves (such as if McDonnell signed an order starting the tests).

Legal scholars and practitioners immediately recognized the significant pro-defendant change in anticorruption law because of *McDonnell*, as well as an actual decrease in prosecutions of state and federal officeholders after the case. Even before the highly anticipated decision came down, experts stated that a decision in favor of McDonnell would "substantially narrow what is considered criminal public corruption and put the brakes on investigations of allegedly unscrupulous politicians across the country" (Zapotosky 2016). The day after the Court's ruling, media outlets such as the *New York Times*, *Wall Street Journal*, and *Washington Post* carried full analyses of the case and speculated that the new anticorruption doctrine would lead to pro-defendant rulings in corruption trials across the country, as well as prosecutors becoming unwilling to bring as many public corruption cases (Parker 2018). A former prosecutor in the U.S. Attorney's Office in the District of Columbia, who had specialized in bribery trials, called *McDonnell* "a real setback in the prosecution of public corruption" and asserted that "politicians are now free to sell . . . access to the highest bidder" (Elliason 2016).

Applying *McDonnell*, several federal and state courts have reversed corruption convictions or ruled for defendants, citing the new Supreme Court doctrine. For example, *McDonnell* played a significant role in the termination of a later corruption case against U.S. Senator Bob Menendez (D-N.J.). Press coverage of the Menendez trial stated that the *McDonnell* precedent "loomed throughout the trial" and that prosecutors were struggling to meet "the high bar for corruption that was set by the Supreme Court's decision" in *McDonnell* (Corasaniti and Schweber 2017). Moreover, *McDonnell* had an ex ante effect on prosecutors' willingness to bring cases. Both federal and state-level prosecutors dismissed charges they had filed against officials and reported a decrease in the number of situations the antibribery statute would apply post-*McDonnell* (Ely 2018). *McDonnell* thus represented a sudden change in U.S. antibribery law and led to a decrease in prosecutors' ability to bring corruption cases against public officials.

However, not all firms were equally affected by the decrease in anticorruption enforcement. We hypothesize that heavily regulated firms in high-corruption states were likely to enter a "protection racket" after *McDonnell*: they increased their level of cash ratios to presumably purchase protection from local politicians. The term "protection racket" refers to an economic enterprise that "produces, promotes, and sells private protection" (Frye and Zhuravskaya 2000, Gambetta 1998: 1). We base our hypothesis on the formal model provided by Johnson et al. (1997), which stipulates that firms make payments to the government and operate in the official economy in exchange for government protection, i.e., the enforcement of property rights. These payments may be in the form of taxes, regulatory costs, or bribes, but if it is easier for the entrepreneur to make a bribe than follow regulations or pay taxes, they will make illegal payments to government officials.

We should expect bribes to be the method through which firms purchase government protection if: (1) the relative cost of following regulations is high, or (2) the entrepreneur can pay bribes easily, and state officials are likely to accept illicit payments. Condition (1) implies that bribery will be more frequent in regulated industries, which is borne out by recent empirical studies finding that government-dependent firms pay a large corruption tax (Colonnelli and Prem 2022). It is also consonant with recent cases involving regulated entities in the United States, such as a major 2020 scandal where a publicly traded electric utilities company was accused of paying the Ohio state house speaker $60 million to secure a government bailout (Diaz 2021). Condition (2) implies that bribery will be more common in areas where there is an entrenched culture of officials accepting such payments. This is consistent with the U.S. experience: states with histories of corrupt political practices, such as Louisiana, continue to see far higher levels of bribery than states without such past experiences, such as Colorado (Smith 2016).

The protection racket hypothesis holds that regulated firms in high-corruption states use bribes rather than taxes to purchase government protection since it is the cheapest way for them to enforce their property rights. After an exogenous decrease in the cost of bribery (due to reduced enforcement of antibribery enforcement), as in the *McDonnell* case, these firms will become further incentivized to participate in the racket. Since cash is anonymous and liquid, it is the preferred asset for making illegal payments such as bribes (Myers and Rajan 1998). Therefore, affected firms will raise their cash reserves to make payments to local politicians, and we will observe higher cash ratios in their capital structure. We emphasize that *McDonnell* created no new duties or compliance burdens for public firms. The decision only decreased antibribery enforcement. Therefore, from the firm's perspective, *McDonnell* simply increased the illegal influence of politicians and regulators rather than the formal set of legal rules governing companies.[3]

However, since these cash holdings will be used to buy protection rather than pursue profitable growth opportunities, shareholders would view the post-*McDonnell* rise in cash ratios negatively (Opler et al. 1999). Moreover, part of the protection offered by government officials could

[3] Similarly, *McDonnell* did not only affect firms that had formal sales relationships with the government, such as companies with government contracts. We re-run our main cash policy regression excluding firms with any sales to the government (using data from the Compustat Segment Customer file) and find that our results remain the same.

be shielding managers at firms from investigations and penalties over actual misconduct. The decrease in government oversight of misconduct at these firms can be viewed as the payoff from participating in the protection racket. This could exacerbate agency costs at regulated firms in high-corruption areas, reducing firm value. Even if protection racket participation does not increase agency costs, the opportunity cost of holding cash for bribes rather than spending it on more lucrative investments could simply outweigh the benefits of lower government enforcement. Indeed, the literature on the relationship between corruption and corporate performance reviewed in the previous section suggests a negative association between corruption and firm value. Therefore, the protection racket hypothesis leads us to three predictions:

Prediction 1. Regulated firms in high-corruption areas will increase their cash ratios after *McDonnell*.

Prediction 2. Regulated firms in high-corruption areas experienced a loss in firm value around the time of the *McDonnell* decision.

Prediction 3. Regulated firms in high-corruption areas face fewer governmental investigations and penalties after *McDonnell*.

To motivate our analysis and illustrate the ways in which *McDonnell* could have created a protection racket, we hand-code reported cases from U.S. federal courts that cite 18 U.S.C. §201, the antibribery statute at issue in *McDonnell*. To collect these cases, we use the Westlaw legal research search engine. We restrict the sample to cases involving bribery of federal, state, and local elected officials and regulators. This leads us to exclude cases where: (1) The court is citing 18 U.S.C. §201 for a purely procedural or statutory interpretation issue; (2) Corrections officers are being prosecuted under the bribery statute for showing leniency toward inmates in return for small cash payments or sexual favors; (3) Armed forced personnel are being prosecuted for accepting bribes on military bases; and (4). The alleged bribery involves foreign officials. For each case, we use the court's opinions, DOJ filings in the case docket, and Internet resources to calculate the amount of bribes allegedly exchanged. In the immediate aftermath of *McDonnell*, the federal courts were flooded with appeals from incarcerated ex-officials arguing that their convictions should be reversed given the Supreme Court's sudden constriction of the definition of bribery. For a random sample of ten cases from 2018 citing 18 U.S.C. §201, nine involved defendants who were indicted before *McDonnell*. *All* of these defendants argued that their convictions ought to be reversed given the Supreme Court's new decision. *McDonnell* therefore, clearly led to previously criminal activity becoming less likely to be punished through the anticorruption statutes.

To better estimate the impact of *McDonnell* on litigated bribery cases, we look at two periods significantly before and after the Supreme Court's 2016 decision. Specifically, we collect all cases that meet our criterion on Westlaw Precision decided in 2011-12 (pre-*McDonnell*) and 2021-22 (post-*McDonnell*). We manually analyzed a total of 35 reported cases. Panel A of Table 1 lists the details for cases in 2011-12, while Panel B contains information for the cases from 2021-22. Since the Supreme Court made corruption convictions less likely in 2016, we can imagine that an elected official or regulator would anticipate that the expected "cost" of corrupt behavior has fallen, reducing the deterrence of U.S. antibribery laws (Becker 1968, Polinsky and Shavell 2007: 427). Given this fall in deterrence, we could expect public officials to engage in more monetarily rewarding corruption and the level of bribery to increase. The alleged bribes from our sample are consistent with this thesis. The average alleged bribe in pre-*McDonnell* cases was $188,782, less than half the mean alleged bribe in the post-*McDonnell* era ($464,487). Panel C of Table 1 shows the result of a simple linear regression of the logged amount of bribe against an indicator for whether the indictment was after *McDonnell*. There is a significant increase in the amount of bribes from the 2011-12 sample to the 2021-22 period. Panel C also contains the results of nonparametric tests showing that the mean and median for bribes in the post-*McDonnell* sample are significantly larger than the corresponding figures for the pre-*McDonnell* cases. These results suggest that the amount of bribes allegedly extracted by public officials rose after the Supreme Court's 2016 decision, consistent with the generation of a protection racket.

An alternative understanding of the descriptive results in Table 1, which is also consistent with the protection racket thesis, is that federal prosecutors became more reluctant to bring antibribery cases after the doctrinal change in *McDonnell*. Since the probability of conviction in corruption cases went down, prosecutors' expected payoff from many antibribery investigations perhaps no longer offset the potential costs of bringing charges against public officials (Becker 1968, Kaplow 2011). DOJ prosecutors would not want to potentially lose a case given the pro-defendant change in the antibribery doctrine in *McDonnell*, since this could create a legal precedent against the federal agencies that would harm future prosecutions of public figures (Baker and Biglaiser 2014). More self-interestedly, individual prosecutors are rewarded in their government careers for being able to secure long prison sentences for defendants (Boylan 2005). They may thus not want to jeopardize their reputations or careers within the DOJ by bringing a case they could lose. Thus, after *McDonnell*, there may simply be fewer public corruption prosecutions.

Consistent with this understanding, we found 25 cases fitting our criteria for 2011-12, but only 10 in 2021-22. The reason average bribes are larger in the 2021-22 sample could be that prosecutors only want to bring cases involving egregious instances of corruption, which can lead to convictions even under the post-*McDonnell* doctrine. This understanding of the data is *also* consistent with the protection racket hypothesis, as it implies that a large amount of "petty" corruption that was formerly prosecuted now goes unchecked by the legal system, while prosecutors use their limited resources to only penalize "grand corruption" (Rose-Ackerman 2018). Therefore, under either understanding of the coded cases, the results from Table 1 about the rise in alleged bribes are consistent with the proliferation of bribery after the *McDonnell* decision.

We should caution that we are only able to access cases that are litigated and reported in case reporters and accessible on Westlaw, which may not be representative of the total population of public corruption prosecutions (Priest and Klein 1984). Most importantly, these cases may be systematically different from those where defendants accept plea bargains. However, we would expect any such selection effect for litigation to bias *against* finding higher alleged bribes post-*McDonnell*. Since it is now easier for defendants to prove their innocence in court, even officials charged with less serious corruption and lower bribe amounts could be incentivized to refuse plea bargains and attempt to prove their innocence in the post-*McDonnell* era. Thus, while we cannot observe the sample of non-litigated (and non-reported) cases, we have no reason to believe this affects our interpretation of the rise in alleged bribes from 2011-12 to 2021-22.

Nevertheless, to ensure that our hand-coded data is not affected by selection effects in either litigation or Westlaw reporting, we look at two official sources of information regarding federal antibribery prosecution. First, we look at the total number of DOJ corruption *indictments* of public officials, as reported in the Attorney General's report. These indictments are the first step of the legal process by which prosecutors seek to hold public officials to account and are thus free of the selection effects of litigation. We see a sharp decline in antibribery enforcement. There were 646 officials charged in 2021, compared to 1,082 in 2011: a 40.3% decrease in federal corruption indictments in just a decade.[4] Second, we obtained information from a United States Sentencing Commission (USSC) report on bribery cases involving public officials between 2013 and 2017.[5] The report's data is consistent with our analysis of the hand-coded sample. It records a 29.64% decrease in the number of officials sentenced for corruption between 2013 and 2017.

Moreover, the median monetary value of the bribes in cases reported to the USSC rose by 122.4% between 2013 and 2017. While the USSC data is also vulnerable to the selection effects of

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[4] The Attorney General's yearly reports are available at https://www.justice.gov/criminal-pin/annual-reports.
[5] See https://www.ussc.gov/sites/default/files/pdf/research-and-publications/quick-facts/Bribery_FY17.pdf.

litigation, its consistency with our analysis confirms that the above results are not driven by Westlaw reporting practices or our methodology for selecting cases. These government statistics confirm that, since *McDonnell*, there has been a decline in prosecutorial punishment of corruption and an uptick in the monetary value of bribes in litigated cases.

3. Dataset and Summary Statistics

We calculate our main outcome of interest, cash ratio, as well as firm control variables (profitability and size as proxied by the natural logarithm of assets) from Compustat. For our event study, we obtain stock prices and cumulative abnormal returns from CRSP, using the market-adjusted model. To estimate the value of cash for firms after *McDonnell* per the method in Faulkender and Wang (2006), we download additional data (Fama-French portfolio returns for size and book-to-market ratio, as well as breakpoints for the quintile portfolios based on size and book-to-market ratio) from Kenneth French's website.[6]

We follow the prior literature in defining the two variables that together define our group of interest: firm regulation and state-level corruption levels. For regulation, we first create an indicator variable *Regulated1*, which equals one if the 2-digit SIC code equals 37, 40, 45, 47, 48, 49, 60, 61, 62, 63, or 64. To choose which SIC groupings to count as regulated, we look at both the previous empirical finance literature (Agrawal and Jaffe 2003, Billio et al. 2012, Masulis and Reza 2015) as well as Kearney and Merrill (1998), who provide a historical survey of U.S. industrial regulation. Collectively, these sources identify utilities, transportation, communications, insurance, and financials as heavily regulated sectors. For robustness, we also create an alternative continuous variable, *Regulated2*, based on the measures for political risk developed by Hassan et al. (2019). We compute the average political and institutional risk for each SIC 2-digit code and take its natural logarithm.[7] Both these variables measure an industry's vulnerability to government influence and political expropriation. We emphasize that these measures should be understood as proxying political risk rather than *compliance* risk. The former can be understood as the cost of political *influence* (which plays a role after *McDonnell* reduced antibribery enforcement), while the latter is a measure of paperwork and other formalities, which were not implicated by the Supreme Court decision since *McDonnell* did not create new regulatory responsibilities for firms.

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[6] See https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

[7] There are no zero values for the SIC 2-digit averages, so we do not need to add a constant such as 1 before taking the logarithm.

Our state-level measure of corruption comes from the United States Department of Justice (DOJ), which is the federal agency responsible for prosecuting corruption by public officials. In the 1970s, after the Watergate scandal, the DOJ set up its elite Public Integrity Section, whose mandate is to investigate corruption by federal and state officials in the executive, legislative, and judicial branches of government. In compliance with a federal statute called the Ethics in Government Act of 1978, the Attorney General of the United States must submit an annual report to Congress detailing the activities of the Public Integrity Section, including the details of recent investigations and convictions of public officials.

The number of public corruption convictions reported in these documents (normalized by population) has become the standard measure of state-level corruption in the economics and finance literature (Cordis and Warren 2014, Glaeser and Saks 2006, Smith 2016). We follow this literature and define state-level corruption as the number of public corruption convictions reported by the Attorney General between 1976 and 2010 per 10,000 population.[8] Figure 1 graphically depicts the prevalence of corruption across the United States. There is substantial geographical variance in the number of public corruption convictions. The most corrupt state, Louisiana, had two corruption convictions per 10,000 population, which is more than eight times the figure for the least corrupt state, Oregon, for whom this figure is 0.24.[9]

Finally, we collect data on penalties assessed for companies in the sample by federal and state agencies. We use a novel dataset called Violation Tracker, developed by the Good Jobs First project. Violation Tracker summarizes more than 300,000 penalties issued by 506 agencies across the country and provides information about settlement amounts. We focus on financially material penalties, which we define as enforcement actions that lead to settlements of at least $500,000. Our results are robust to choosing alternative penalty thresholds of $100,000, $250,000, or $1 million. The Violation Tracker data has been used by many recent papers in finance and accounting, including Heese and Pérez-Cavazos (2020), Heese et al. (2022), Raghunandan (2021), and Stubben and Welch (2020). Part

[8] The Attorney General's reports regarding the activities of the Public Integrity Section are available at https://www.justice.gov/criminal-pin/annual-reports. Simpson et al. (2012) aggregate this data at the state level and normalize it by state population. While the Ethics in Government Act was passed in 1978, the data begins in 1976, since each annual report contains information about prosecutions from a few prior years.

[9] One may worry that in a truly corrupt state, convictions would be *lower*, since misconduct by public officials would never be investigated. However, this concern is addressed by the fact that the federal DOJ agency uses national statutes like 18 U.S.C. §201 to prosecute state and local officials. The DOJ's representatives in the states are appointed by the federal government, and are completely independent of local officials (Maass 1987). Therefore, in following the literature and using DOJ data to proxy for state-level corruption, we avoid the problem of high corruption potentially reduce rates of bribery convictions.

of our analysis in section 6 involves investigating the difference in post-*McDonnell* enforcement between federal and state agencies. Therefore, for all penalties against the firms in the sample, we hand-coded whether the responsible agency was federal (such as a U.S. Attorney from the DOJ or the Occupational Safety and Health Administration) or state/local (such as a state attorney general, city district attorney, or state labor department).

Table 2 presents the definitions of the variables used in our analysis. Table 3 presents the summary statistics. We display summary statistics for cash ratio, firm financials (size and profitability), two proxies for firm regulation, state-level corruption, cumulative abnormal returns for both our event study windows and penalty data from Violation Tracker. To ensure that our results are not driven by outliers, we winsorize all continuous financial, regulation, and corruption variables at the 2% level.

4. Rise in Cash Ratios after Decreased Anticorruption Enforcement

In this section, we document the effect of decreased antibribery enforcement on the cash policy of regulated firms in high-corruption states. These companies, which are most vulnerable to political expropriation, increased their cash ratios in the years immediately following *McDonnell*. Since cash is highly liquid and anonymous, it is the asset most likely to be used for making illegal payments such as bribes. The rise in cash ratios for the firms most likely to be affected by corruption supports our hypothesis that the Supreme Court decision potentially created a "protection racket" where these companies bought protection from public officials.

We estimate the following regression model for firm *i* belonging to industry *j* and headquartered in state *s* at time *t*:

$$Cash\ Ratio_{i,t} = \beta\left(Regulation_j\ x\ \ Corruption_s\ x\ Post_t\right) + \alpha Post_t\ x\ Regulation_j + \pi Post_t\ x\ Corruption_s + \gamma Post_t + \delta X_{i,t} + \mu_i + \epsilon_{it}. \qquad (1)$$

$Regulation_j$ is a variable for firm *i* regulation at the industry level, *j*, which is either an indicator variable in the case of *Regulated1* or a continuous measure when we use the alternative proxy *Regulated2*. $Corruption_s$ is a measure of the corruption level in the firm's home state *s*, and equals the number of public corruption convictions per 10,000 population in the jurisdiction in 1976-2010. $Post_t$ is an indicator variable that equals 1 for years after the *McDonnell* decision (i.e., 2016 onward). X_{it} is a vector of firm financials: size as proxied by the natural logarithm of assets, and profitability (return on assets). The μ_i is a firm fixed effect, and ϵ_{it} is the regression error. The inclusion of firm fixed effects leads to the main effects $Regulation_j$ and $Corruption_s$, as well as their interaction with each other, dropping off the regression. We use data from the time period 2013-19 (i.e., three years before and after *McDonnell*) and cluster standard errors at the firm level. There are 6,703 unique firms in the regression sample.

Table 4 presents the results of this regression model. Regardless of the choice of regulation proxy, the coefficient for the triple interaction variable $Regulation_j \ x \ Corruption_s \ x \ Post_t$ is positive and highly significant. For column (1), a one standard deviation increase in corruption for a firm's headquarter state is associated with a 0.82 percentage point (i.e., less than a percentage point) increase in cash ratio after *McDonnell* if the firm regulated, i.e., if *Regulated1* equals 1. This effect is economically significant, with the rise in cash ratio equaling 4.65% of the mean cash ratio in the regression sample and 11.54% of the median cash ratio. In column (2), a one standard deviation increase in both headquarter state corruption and the continuous measure *Regulated2* is associated with a 0.30 percentage point (i.e., less than a third of a percentage point) rise in cash ratio post-*McDonnell*. The effect remains economically meaningful, representing 1.87% (4.65%) of the mean (median) cash ratio in the regression sample. There may be some concern that the 2016 data may not fully capture the effect of *McDonnell*, since the Supreme Court decision was announced about midway through the year. To address this concern, we rerun the regression in Table 4, excluding observations from 2016, and find that our results remain unchanged in magnitude and significance. We also re-run the analysis after excluding firms that relocated headquarters during the sample period (relocation only applies to 62 firms, of which nine belong to regulated industries per the *Regulated1* measure).[10] Our results remain virtually unchanged using either *Regulated1* or *Regulated2* as a proxy for firm regulation.

The decrease in anticorruption enforcement after *McDonnell*, therefore, seems to have led to an increase in cash ratios for the firms most vulnerable to bribery: regulated companies headquartered in high-corruption states. We find significant and economically meaningful rises in cash ratios for these firms using either of our proxies for regulation. It remains to be seen whether it is advantageous for regulated firms in high-corruption areas to hold extra cash after anticorruption enforcement was relaxed. On the one hand, as explained in Section 2, the excess cash is not being used for growth opportunities or valuable projects. It may even be used to buy political protection for managers

[10] The data for historical headquarters comes from Gao et al. (2021).

engaged in misconduct and destroying shareholder value. These factors would imply that holding excess cash would reduce firm value for affected firms post-*McDonnell*. On the other hand, regulated firms depend on political connections and could benefit from the opportunity to "purchase" useful connections with their additional cash reserves. The following section, therefore, takes on the open empirical question of whether regulated firms in high-corruption states gained or lost value by entering into a protection racket after the legal shock to anticorruption enforcement in 2016.

5. The Costs of Protection Rackets: Abnormal Returns and Value of Cash

To determine if the *McDonnell* decision impacted firms' value, we turn to an event study of the cumulative abnormal returns around the decision date (June 27, 2016). We use the event windows [-1,1] and [0,1] to calculate cumulative market-adjusted returns for each firm, $r_{i,t}$, as is common in other event studies. Consistent with our predictions, we expect that regulated firms in more corrupt states will lose market value because of capital misallocation from profitable investments into bribes to local politicians. Additionally, *McDonnell* could increase the agency cost of managing such firms, as CEOs engaged in bribery are more likely to engage in self-dealing activities that further lower shareholder returns. Therefore, we study the following regression:

$$r_{it} = \beta\left(Regulation_j \ x \ \ Corruption_S \ x \ Post_t\right) + \gamma Post_t \ x \ Regulation_j + \delta Post_t \ x \ \ Corruption_s + \theta Post_t + \vartheta X_{it} + \epsilon_{it} \ , \qquad (2)$$

whereby β captures the effect of *McDonnell* on firm *i* value (i.e., announcement returns) for regulated firms (indexed by industry *j*) in states *s* with high levels of corruption. For robustness, we include two separate proxies for regulation, i.e., *Regulated1* (Agrawal and Jaffe 2003, Billio et al. 2012, Masulis and Reza 2015) and *Regulated2* (Hassan et al. 2019) and cluster standard errors at the industry (SIC-2) level, as is typical for event studies. Our results are shown in Table 5 and confirm the null hypothesis that among regulated firms, those headquartered in more corrupt states and hold greater cash balances experience a more negative and significant market-adjusted announcement return following the *McDonnell* decision. For example, a standard deviation increase in the cash ratio and corruption is associated with an additional decline in the [-1,1] announcement returns for regulated firms by 0.53 percentage points (0.21 percentage points) if we use *Regulated1* (*Regulated2*) as proxies for regulation. The results are significant even if we examine the short event window [0,1] (then, the relevant percentage declines are 0.31 percentage points and 0.15 percentage points). Results are also robust if we bootstrap (50 iterations) standard errors instead of clustering at the industry level. Overall, we conclude that Table 5 test confirms the hypothesis that *McDonnell* decision harmed shareholder wealth, perhaps due to capital misallocation and increased agency costs in its aftermath. Notably, this finding suggests that the costs of participating in the post-*McDonnell* protection racket outweigh any benefits, since regulated firms in high-corruption areas lost value around the time of the Supreme Court decision.

The event study results give us insight into the immediate market valuation of *McDonnell*'s effect on the value of regulated firms in high-corruption states. However, if holding greater cash balances following *McDonnell* decision harmed shareholders, the marginal value of such cash balance would also decline. From the shareholders' standpoint, it is inefficient to hold more cash as cash is not a productive asset and can further increase agency costs. To test for this effect, we estimate a regression derived from Faulkender and Wang (2006) as follows:

$$r_{i,t} = \gamma_1 \Delta C_{i,t}/M_{i,t-1}\, x\, Regulation_j\, x\, Corruption_s\, x\, Post_t + \gamma_2 \Delta C_{i,t}/M_{i,t-1}\, x\, Regulation_j\, x\, Corruption_s + \gamma_3 \Delta C_{i,t}/M_{i,t-1}\, x\, Regulation_j\, x\, Post_t + \gamma_4 \Delta C_{i,t}/M_{i,t-1}\, x\, Post_t\, x\, Corruption_j + \gamma_5 \Delta C_{i,t}/M_{i,t-1}\, x\, Regulation_j + \gamma_6 \Delta C_{i,t}/M_{i,t-1}\, x\, Corruption_j + \gamma_7 \Delta C_{i,t}/M_{i,t-1}\, x\, Post_t + \gamma_8 Regulation_j\, x\, Corruption_s + \gamma_9 Regulation_j\, x\, Post_t + \gamma_{10} Corruption_s\, x\, Post_t + \gamma_{11} Corruption_s + \gamma_{12} Post_t + \gamma_{13} Regulation_j + \gamma_{14} X_{i,t} + \mu_i + \delta_{j,t} + \theta_{s,t} + \epsilon_{it}, \quad (3)$$

where γ_1 is the coefficient of interest associated with $\Delta C_{i,t}/M_{i,t-1}$ (the change in cash holdings divided by the previous fiscal period's market value of equity), $r_{i,t}$ is the realized stock return for firm i in year t minus the return of the relevant size- and BE/ME-excess portfolio, *s* indexes state of headquarters location, *j* indexes industry and $X_{i,t}$ includes all control variables in equation (7) from Faulkender and Wang (2006).[11] Because returns are forward looking, we limit our sample period for the Faulkender and Wang (2006) analysis to 2015-17, i.e., one year before and after the *McDonnell* decision on the assumption that the returns would have priced in the value of cash more than twelve months from the decision. We anticipate that coefficient γ_1 is negative, as allocation of capital to cash holdings decreases available capital for valuable investments and therefore decreases shareholder wealth. Table 6 provides qualified support for this hypothesis. In column (1), the coefficient associated with *Regulated1 x Post x Corruption* is negative and highly significant. However, in column

[11] Control variables include $\Delta E_{i,t}/M_{i,t-1}$(change in earnings before interest and extraordinary items), $\Delta NA_{i,t}/M_{i,t-1}$ (change in total assets net of cash), $\Delta RD_{i,t}/M_{i,t-1}$(change in R&D expenditure), $\Delta I_{i,t}/M_{i,t-1}$ (change in interest expense), $\Delta D_{i,t}/M_{i,t-1}$(change in dividends), $C_{i,t-1}/M_{i,t-1}$ (lagged cash), $NF_{i,t}/M_{i,t-1}$ (net financing), and $L_{i,t}$ (leverage).

(2), while the coefficient for *Regulated2 x Post x Corruption* is negative, it is not significant at traditional levels. Therefore, we find suggestive, but not conclusive, evidence that the increase in cash holdings decreased excess returns for the affected firms.

6. Firm Payoffs from Protection Racket Participation

Why would regulated firms in high-corruption states participate in a protection racket by holding extra cash reserves to (presumably) bribe public officials? We hypothesize that one potential payoff for these companies could be protection from government penalties or enforcement of applicable laws and regulations. Since at least Stigler (1971), economists have recognized that government can use regulation as a "threat to every industry in the society," using the "power to prohibit or compel" as a credible danger to the profitability of private enterprises. However, private actors can evade onerous regulations by making illicit payments to public officials in the form of bribes. Since government enforcement often involves large amounts of discretion, they can reward firms paying bribes by turning a blind eye to violations of environmental, labor, workplace safety and other laws. Such a finding would be consistent with Johnson et al. (1998), who argue that in high-corruption areas, officials "decide individual cases without effective supervision."

To test whether reduced enforcement is the reward regulated firms in highly corrupt states receive for participating in the post-*McDonnell* protection racket, we estimate the following regression:

$$Penalty_{i,t} = \beta\left(Regulation_j \; x \; Corruption_s \; x \; Post_t\right) + \alpha Post_t \; x \; Regulation_j + \pi Post_t \; x \; Corruption_s + \gamma Post_t + \delta X_{it} + \mu_i + \theta_{s,t} + \epsilon_{it}. \quad (4)$$

Here, $Penalty_{it}$ equals either an indicator for whether a governmental penalty was assessed against a firm in a given year, or the yearly number of penalties by the firm. As explained in Section 3, this information comes from the Violation Tracker dataset. Table 7 presents the results of this regression model. Columns (1) and (2) use *Regulated1* as a measure of firm regulation $Regulation_j$ at the industry *j*, while columns (3) and (4) uses *Regulated2*. Columns (1) and (3) show that irrespective of which measure of firm regulation we use, we find a significant decrease in the probability there was government enforcement against regulated firms in high-corruption areas after *McDonnell* was decided. A one standard deviation increase in home state corruption for a regulated firm (i.e., for a firm for which *Regulated1* equals 1) translates to a decrease in the probability of penalties equal to 48.8% of the average for the regression sample. Turning to the alternate measure of regulation, a one standard increase in both *Regulated2* and headquarter-state corruption translates to a decrease in the probability of penalties approximately equal to 18.92% of the mean in the regression sample. Columns (2) and (4) use the *number* of penalties as the dependent variable. The triple interaction between regulation, home state corruption, and *Post* continues to have a negative and significant coefficient when we use *Regulated1* as a proxy for firm regulation. This coefficient is negative but not significant at traditional levels using *Regulated2* as the proxy for regulation. Overall, the results in Table 7 suggest that firms affected by *McDonnell* were rewarded for participation in the protection racket with less stringent oversight by government agencies.

We emphasize that the government penalties in the Violation Tracker dataset are not directly associated with bribery and are thus not affected by the *McDonnell* decision. The most common categories of penalties are, in order: wage and hour offenses, environmental violations, consumer protection violations, and investor protection violations.[12] Therefore, there is no compelling reason why government enforcement should have slowed down for regulated firms in high-corruption states. The most plausible factor is the establishment of a possible protection racket after *McDonnell* weakened anticorruption efforts. Furthermore, Table 7 shows that the reduction in government penalties is *restricted* to firms affected by *McDonnell*: other companies, which are less vulnerable to political expropriation and thus less likely to participate in the protection racket, see no such decrease in penalties.

The regression model estimated in Table 7 has one drawback: it lumps together *all* enforcement actions against firms reported in the Violation Tracker, irrespective of the type of agency that handled these actions. More specifically, it does not distinguish between federal and state/local agencies. It is well established that federal officeholders and regulators are generally better-qualified and paid than their state counterparts. Neuborne (1976), for example, found that the skill level of the federal judiciary "dwarfs the competence" of less prestigious and lower-paid state judges and is better insulated from local political pressures than the state judiciary. Similarly, while the U.S. Constitution guarantees members of Congress a salary,[13] many state legislatures receive low pay or no salary at all (Carnes and Hansen 2016). Finally, state administrative agencies have less independence and are more firmly under the control of elected legislators than federal regulators (Stiglitz 2018).

[12] False Claims Act (FCA) lawsuits are often initiated by individual whistleblowers on behalf of the government through a unique legal mechanism called a "qui tam" lawsuit (Engstrom 2014). Therefore, there could be some concerns that these lawsuits represent the existence of private whistleblowers, rather than government agencies' proactiveness toward enforcement. We thus exclude FCA claims from the sample. However, our results regarding the post-*McDonnell* decline in state agency enforcement (and lack of any significant change in federal enforcement) persist when we include FCA violations in the sample.

[13] Article 1, Section 6, Clause 1 of the Constitution reads: "The Senators and Representatives shall receive a Compensation for their Services, to be ascertained by Law, and paid out of the Treasury of the United States."

Given these institutional differences between state and federal officeholders across branches of government, we might expect that state officials would be more likely to participate in a protection racket and reduce enforcement for participating firms. To see whether there was a difference in state and federal enforcement trends after *McDonnell*, we hand-code the agency associated with each penalty in the dataset as either federal or state/local. We then re-run regression model (4) separately using the number of penalties issued by federal and state agencies as the dependent variable. Table 8 displays the results for these regressions, with columns (1) and (2) using *Regulated1* and *Regulated2* as proxies for firm regulation, respectively. Consistent with the literature on institutional differences between local and federal officials, we find that regulated firms in highly corrupt areas only experienced a decrease in *state* agency penalties. There was no difference in the number of federal agency enforcement after *McDonnell*. Therefore, state and local agencies seem to be more likely to participate in the protection racket than their better credentialled and higher-paid federal counterparts.

This reduction in government enforcement against affected companies can be seen as the flipside of the rise in cash ratios at these firms, demonstrated in section 4. While the former is a benefit affected firms enjoy for participating in the protection racket, the latter is the cost they must bear for being vulnerable to political expropriation. It is an empirical question to assess whether regulated firms in high-corruption states experienced a *net* increase or decrease in value after *McDonnell*. The results in section 5, showing that affected firms experienced negative abnormal returns when the Supreme Court announced its decision (and some evidence that the value of cash holdings decreased), strongly suggest that the costs outweighed the benefits. Regulated firms in high-corruption areas hence lost value by participating in the post-*McDonnell* protection racket.

7. Robustness

A. Was it a Trump Effect?

A major political event in 2016, which could potentially be driving our results, is the November 8 election, which saw the election of Donald Trump as the 45th President of the United States. Some commentators have described the Trump administration, which was in office between 2017 and 2021, as being unusually corrupt by U.S. standards (Balkin 2017). Could it be that the election of Trump drove the increase in cash ratios for firms rather than the *McDonnell* case? This alternative theory would hold that Trump's election led to a rise in corrupt practices, which is why firms may hoard more cash.

There are at least five reasons to doubt that our results are driven by the U.S. presidential election. First, the *McDonnell* case was decided on June 27, 2016, 135 days before the election of Trump. Our event study results, which find abnormal returns for regulated firms in high-corruption states in two- or three-day intervals around the court decision, are thus unimpeached by any possible Trump effect. Furthermore, since Trump's election was a surprise to market participants even on election day (Child et al. 2021), it is implausible that his presidency had been priced into the market reactions to *McDonnell* in the summer of 2016. To see if the surprise election of Trump had an effect on our group of interest, regulated firms in high-corruption areas, we re-run the event study from Section 5, this time with the 2016 presidential election as the event. Following Child et al. (2021), we use November 9, 2016 (the day the results of the election were announced) as day 0. Table 9 presents the results of this event study. Using both *Regulated1* or *Regulated2* and for both event windows [0,1] and [-1,1], we fail to find significant abnormal returns for affected firms. Therefore, while regulated firms in high-corruption areas lost value around the time *McDonnell* was decided, they do not seem to have been similarly affected by the 2016 presidential election.

Second, the data shows that corruption does not have a partisan valence. There are high-corruption states leaning toward both parties (Louisiana is Republican-leaning, while Illinois is dominated by Democrats), as there are low-corruption states (for example, Utah for the Republicans and Oregon for the Democrats). We create an indicator variable called *Trump State*, which equals one if Trump won more than 50% of the votes cast in the state in the 2016 election.[14] The correlation between the corruption variable and *Trump State* is –0.03, indicating that an increase in cash ratios for firms located in highly corrupt states is not simply shorthand for an increased cash ratio for companies in Trump-leaning areas.

Third, we re-run equation (1), replacing $Corruption_s$ with *Trump State*. The results are displayed in Panel A of Table 10. Using either proxy for firm regulation, the triple interaction between *Trump State*, regulation, and *Post* is insignificant. There is, therefore, no evidence that the rise in cash ratio has anything to do with local support for Trump. Fourth, we obtain data about firms' geographic segments from the Compustat Historical Segments database. We separately re-run equation (1) for firms with a below sample mean share of foreign sales. These firms would be less impacted by the Trump administration's signature economic policies regarding tariffs and trade wars (Cavallo et al. 2021). Panel B of Table 10 shows that the increase in cash ratios for regulated firms in high-

[14] 2016 election data comes from the MIT Election Lab, https://electionlab.mit.edu/data.

corruption states persists even for these firms. Therefore, the rise in cash ratios is driven by firms' vulnerability to political expropriation as measured by regulation and home state corruption rather than exposure to Trump administration policies.

Finally, our results on the decrease in enforcement being concentrated in the state (and *not* federal) agencies also cast doubt on the Trump-centric alternative explanation. The federalist system of government in the United States means that the President, despite being immensely influential, has limited powers over the states (Calabresi 1995). In fact, during the Trump presidency, local politicians, judges, and regulators often defied and fought against his administration's controversial policies (Barry 2019). Therefore, while Trump may have wielded power over federal agencies such as the National Labor Relations Board or the regional U.S. Attorneys he appointed, he had very little or *no* influence over state agencies. To take one especially vivid example, for the entirety of Trump's presidency, a member of the opposing Democratic Party was the Governor of Louisiana (the most corrupt state in our sample), despite the state having heavily voted for Trump in the 2016 presidential race. This Governor had far more influence over Louisiana's state agencies than the President in Washington, D.C. As we saw in Table 8, the firms most affected by *McDonnell* saw a decrease in enforcement from the state, not federal, agencies. Therefore, it is highly unlikely that the election of Trump drove these results since regional officials such as governors are more likely to control state and local agencies.

B. Matching

Another concern with our results could be that regulated firms are intrinsically different from other companies that are less dependent on government contacts. While this does not detract from the exogenous effect of *McDonnell* on anticorruption enforcement, it could lead to misleading results using a traditional OLS difference-in-difference methodology. We re-run our regressions on two different matched samples to account for this possibility. First, we match each regulated firm (defined alternately as either *Regulated1* equaling one or *Regulated2* exceeding the sample median) to a less-regulated firm using nearest-neighbor matching without replacement, based on size and headquarter state. Second, we use the entropy balancing matching methodology developed by Hainmueller (2012) and used in papers such as Heimer and Simsek (2019) and Jacob et al. (2019). We balance the first three moments of the covariates for regulated and non-regulated companies. Table 11 shows the results from re-running equation (1) on both matched samples. Using either matching methodology and both proxies for firm regulation, the triple interaction between regulated status, corruption, and *Post* continues to have a positive and significant coefficient. Therefore, our results showing the increase in cash ratio for regulated firms in high-corruption states is robust to different matching methods.

C. Placebo Dates

To verify that the rise in cash ratios for regulated firms in high-corruption states was indeed the result of *McDonnell* being decided in 2016, and not some other shock in the same broad time period, we re-run the regression from the model (1) using two randomized event years: 2012 and 2018. Table 12 presents the results of these placebo regression models. For both these randomized event years, the triple interaction between regulation, corruption, and *Post* is insignificant. In other words, we fail to see a similar rise in cash ratios for regulated firms in high-corruption states for these placebo time periods. These null findings reinforce our hypothesis that the changes in the capital structure for firms most likely to participate in a protection racket are driven by *McDonnell*'s legal shock to anticorruption enforcement.

D. Additional Fixed Effects

We add a battery of additional fixed effects to test whether our results are driven not by *McDonnell*, but by industry-specific or state-specific time trends. As in our baseline models, we continue to include firm fixed effects and cluster standard errors at the firm level. However, we also add new fixed effects, in the following order: year fixed effects; industry-year fixed effects; state-year fixed effects; and both industry-year and state-year fixed effects. Table 13 presents the results of adding these additional fixed effects to the regression in model (1). Panel A uses *Regulated1* as a proxy for regulation, while Panel B uses *Regulated2*. For both regulation proxies and all combinations of fixed effects, the triple interaction between regulation, corruption, and *Post* continues to have a positive and significant coefficient. The magnitude of this coefficient is roughly similar to that of the coefficient for the triple interaction in Table 4. Therefore, our post-*McDonnell* cash policy results survive the addition of additional fixed effects and cannot be explained by industry-specific or state-specific time trends.

E. Did Affected Firms Use the Extra Cash for Political Lobbying?

Bribes and illegal payments are not the only way for firms to curry favor with politicians and regulators. Bertrand et al. (2014) document the importance of lobbying, i.e., lawfully trying to persuade policymakers regarding an interested party's point of view, in the U.S. regulatory process. As a robustness check, we thus check whether regulated firms in high-corruption areas responded to *McDonnell* not by making illicit payments to politicians but by spending more money on the lobbying process. Of course, bribes and lobbying are not mutually exclusive; a firm affected by the Supreme

Court decision may respond by increasing both. However, there is no ex-ante reason why firms should change their lobbying behavior after *McDonnell*, since the decision did not change the incentive structure for lobbying.

Our proxy for lobbying comes from the LobbyView dataset introduced by Kim (2018), and used in papers such as Pawliczek et al. (2021). We re-run the regression in equation (1), setting our dependent variable as the natural logarithm of (1 + the yearly dollar amount spent by the firm on lobbying). We add 1 to the amount to account for zero values; our results remain qualitatively unchanged if we simply use the amount of lobbying as the dependent variable. Table 14 presents the results of this regression. The triple interaction of regulation, corruption, and *Post* has no significant effect on corporate lobbying after *McDonnell*, regardless of which proxy for firm regulation we use. Therefore, it does not seem that extra cash being held by firms after the court decision is being used for formal political lobbying, consistent with the notion that *McDonnell* implicates bribery and illegal payments, not lawful processes like legislative lobbying.

F. Were Affected Firms Rewarded With Government Subsidies?

Section 6 shows that regulated firms in high-corruption areas benefitted after *McDonnell* via reduced penalties from state and local government agencies. However, another possible benefit these firms can derive from politicians and regulators in exchange for protection racket participation is increased amounts of government subsidies. Governments routinely give favored or politically connected firms subsidies, either in the form of direct grants or exemptions from tax obligations (Kornai 1979, Shleifer and Vishny 1994). To test whether this was the case, we obtain data about federal and state subsidies from Subsidy Tracker. Developed by the Good Jobs First project, Subsidy Tracker provides granular information about the amount of subsidy given to firms, as well as the name of the local, state, or federal program through which the subsidy was created. Subsidy Tracker has previously been used in papers such as Dong et al. (2022) and Huang (2022).

We re-run the model in equation (4), replacing $Penalty_{i,t}$ as the dependent variable with $Subsidy_{i,t}$, which equals the natural logarithm one plus the total subsidies enjoyed by a firm in a given year. We separately analyze subsidies from state/local governments and the federal government, given the argument in section 6 that there may be institutional differences in the corruptibility of local and national officials. Table 15 presents the results of this regression model. Using either *Regulated1* or *Regulated2* as a proxy for firm regulation, the coefficient for the triple interaction between regulation, home state corruption, and *Post* is insignificant, whether we look at federal or state subsidies. Therefore, we do not have compelling evidence that the non-enforcement of law for affected companies by state-level agencies explored in section 6 was coupled with an increase in subsidies from any level of government.

G. Pretrend Analysis

We make sure that the difference in cash ratio between firms affected by *McDonnell* (regulated companies in high-corruption areas) and other firms was a result of the decrease in anticorruption enforcement after the Supreme Court decision, rather than the continuation of a preexisting differential trend. In Figure 2, we plot the coefficient estimates for the triple interaction between firm regulation, home state corruption, and time dummies for individual years before and after the year of the *McDonnell* decision. Following Gopalan et al. (2021), we set the base year in the fully-saturated regression model equal to the year before our experiment, i.e., 2015. Panel A of Figure 2 uses *Regulated1* as the proxy for firm regulation, while Panel B uses *Regulated2*. Regardless of how we measure firm regulation, the figure shows there is no discernible differential trend in cash ratio between regulated firms in high-corruption areas and other companies. In both panels, the coefficient of interest only becomes statistically different from zero *after* the Supreme Court's decision. Therefore, it is unlikely that our results are the product of cash policy trends preceding the *McDonnell* case.

H. How do Firms Hide Bribes: The Operating Expenses Channel

As a final robustness test, we explore how publicly listed companies—which are exposed to the scrutiny of regulators, auditors, and investors—could possibly hide elevated levels of bribery after *McDonnell*. Companies usually mask illegal payments made to government officials by misclassifying them as miscellaneous operating expenses. An analysis of recent SEC bribery indictments against companies found that bribes were hidden in categories such as administrative expenses, consulting fees, commissions, and discounts (Scheck et al. 2019). Such expenses would be reflected in the selling, general and administrative expenses (SG&A) section of a firm's accounting statements. Therefore, we hypothesize that regulated firms in high-corruption states will report higher SG&A after *McDonnell*, due to the mischaracterization of bribes as operating expenses.

However, increased bribe payments after *McDonnell* are unlikely to be immediately reflected in the company's public disclosures. According to an article in the official publication of the Association of Certified Fraud Examiners, bribes paid by public companies usually take a few years to affect their publicly disclosed balance sheets (Zack 2015). This is because companies usually misclassify illicit payments under several categories across multiple fiscal years. For example, when the SEC charged Avon Products, a publicly listed firm, with paying $8 million in bribes, it was revealed that Avon had hidden the illegal payments in accounting statements over five fiscal years. The Avon example shows that there is a lag between the timing of the bribe payment and its appearance on a company's balance sheet.[15]

Given the relationship between bribery and SG&A, as well as the likely lag between actual illicit payments and their appearance on balance sheets, we estimate the following equation:

$$SG\&A_{i,t+n} = \beta(Regulation_j \: x \: \: Corruption_s \: x \: Post_t) + \alpha Post_t \: x \: Regulation_j + \pi Post_t \: x \: Corruption_s + \gamma Post_t + X_{i,t}\delta + \mu_i + \epsilon_{it} \quad (5)$$

$SG\&A$ equals the level of operating expenses normalized by sales, as reported in Compustat, and winsorized at the 2% level. The independent variables on the right hand side have the same meanings as before. The *n* in the subscript for $SG\&A$ represents how many years in the future we expect the effect of firm regulation and corruption to be reflected in operating expenses. We alternately fix *n* as 2, 3, or 4. In other words, equation (5) estimates how firm regulation and home state corruption affects the level of reported operating expenses two, three, or four years in the future.

Table 16 presents the results of this regression model. Irrespective of the proxy of firm regulation used, we find that regulated firms in high-corruption states report higher levels of SG&A after *McDonnell*. The only exception is the model where we use SG&A at time t+2 as the dependent variable and *Regulated1* as the regulation proxy: here too, the coefficient of interest is positive but only marginally significant. These findings are consistent with the theory advanced by government enforcement agencies such as the SEC in recent years that firms mask illegal bribes by misclassifying them as operating expenses. They also provide a possible mechanism through which public firms were able to increase illicit payments to government officials after *McDonnell*, despite being scrutinized by regulators, auditors, and shareholders.

8. Conclusion

We use a legal shock that led to decreased antibribery enforcement in the United States to investigate the causal effect of corruption on firm capital structure and performance. In a highly anticipated case, *McDonnell v. United States*, the U.S. Supreme Court greatly restricted the definition of bribery under federal law. The *McDonnell* decision immediately led to several federal and state corruption cases against public officials being thrown out and also led to an ex ante decrease in prosecutors' willingness to bring bribery cases against government figures. The Supreme Court

[15] This is further confirmed by an analysis of the accounting implications of bribery by the FCPA Blog, a leading forum on cases related to the Foreign Corrupt Practices Act: "it can take years before the corrupt payment ends up in an income statement account." See https://fcpablog.com/2018/03/06/risk-alert-are-bribes-lurking-on-the-balance-sheet/.

decision represents a rare, and as yet unexamined, natural experiment in which there was a natural shock to the probability of corruption in the United States. We use this natural experiment to explore the connection between corruption and firm capital structure and value. *McDonnell* allows us to overcome the endogeneity of firm financials that characterized the previous literature on this topic.

We propose a protection racket hypothesis: regulated firms in high-corruption states, most dependent on government connections and vulnerable to political expropriation, were most likely affected by the decrease in antibribery enforcement after *McDonnell*. These firms presumably purchased protection from government officials in exchange for bribes after the Supreme Court decision made these illicit payments easier. Consistent with this hypothesis, the affected firms increased their cash ratios in the years following *McDonnell*. Turning to the value implications of this excess cash, we find that regulated firms in high-corruption areas exhibited negative abnormal returns around the time of the *McDonnell* decision, suggesting that the decrease in anticorruption enforcement was destructive to shareholder wealth. We also find suggestive evidence that the value of cash declined for these companies immediately after the Supreme Court case.

Finally, we ask why these companies participated in the protection racket. One potential benefit of participation was that government agencies reduced enforcement of various laws and administrative rules against regulated firms in high-corruption areas after *McDonnell*. These companies saw a significant decline in government penalties assessed against them after the court decision. Hand-coding the types of agencies that appear in the dataset, we find that the decrease in enforcement is restricted to state and local administrative agencies; there is no change in federal agency enforcement after *McDonnell*. This result is consistent with the previous literature's claims about federal officeholders and regulators being less corruptible and more immune from political pressures. It also illustrates the importance of political geography and the variance in corruption levels between different states in studying the connection between corruption and firm value.

The *McDonnell* legal shock could be used in future work to examine how corruption influences different measures of firm performance, such as innovation and tax compliance. However, our results in this paper strongly suggest that the reduction in U.S. anticorruption enforcement after the Supreme Court decision led to the creation of a protection racket for regulated firms in high-corruption areas.

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Figure 1.
Unites States Department of Justice corruption convictions per 10,000 population, 1976-2010.

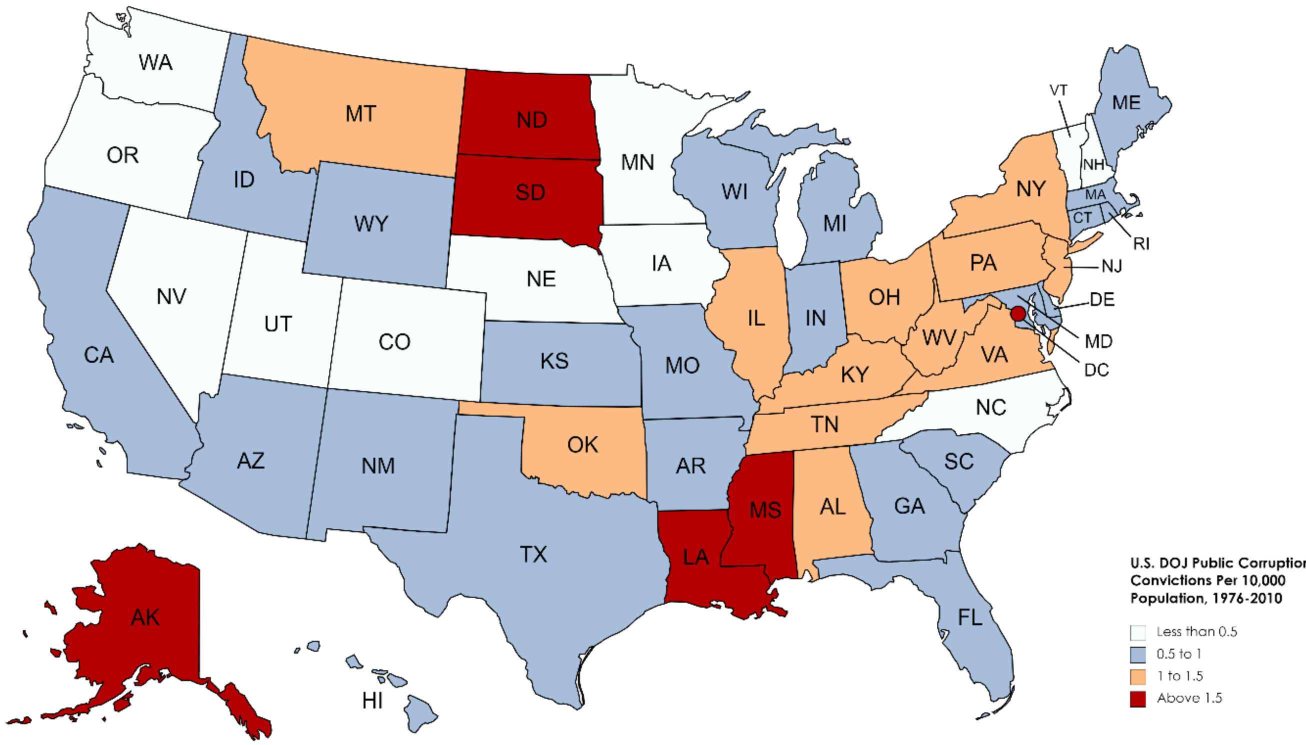

Figure 2.

Figure 2 plots in Panel A (B) the coefficient estimates from the cash ratio regressions of Event Indicator *x* Regulated1 *x* Corruption (Event Indicator *x* Regulated2 *x* Corruption) and their confidence intervals at 95% level, shown as vertical bars. The event indicators are coded individually for T-4, T-3, T-2, T-1, T, T+1, T+2, and T+3 where T=2016, i.e., the year of the *McDonnell v. United States*. The estimation uses a fully-saturated model where we set the base year as the year immediately before the *McDonnell v. United States* (i.e., year T-1, shown on the horizontal as a value of zero) by omitting the dummy variable for that year. Included in Panel A (B), but not shown, are controls for ROA, log firm assets, Corruption, Regulated1 (Regulated2), Event Indicators, Regulated1 *x* Corruption (Regulated2 *x* Corruption), Regulated1 *x* Event Indicators (Regulated2 *x* Event Indicators), Event Indicators *x* Corruption and firm fixed effects. All variables are defined in Table 2. All firm financials are winsorized at the 2% level in each tail. We include firm fixed effects. The dashed vertical line marks the event time.

Panel A: Regulation proxied by *Regulated1*:

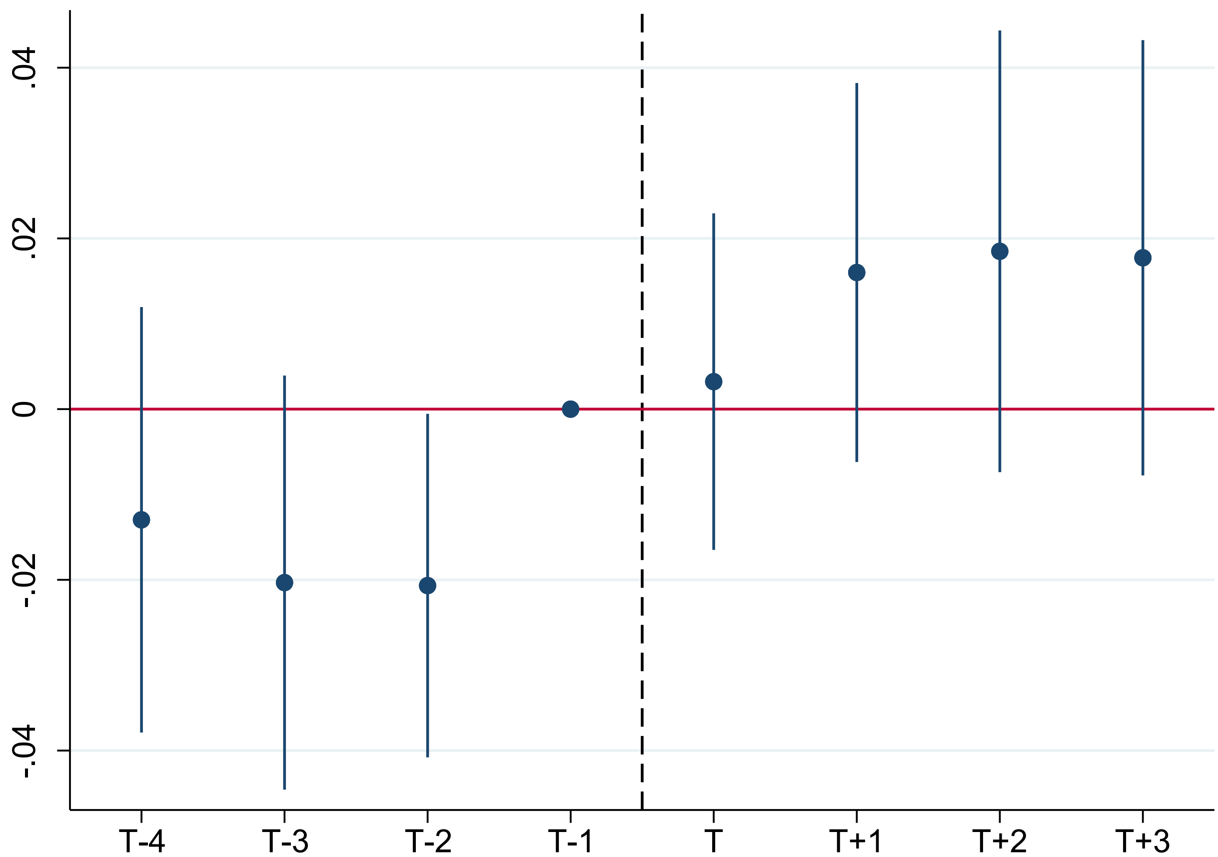

Figure 2. (Continued)

Figure 2 plots in Panel A (B) the coefficient estimates from the cash ratio regressions of Event Indicators *x* Regulated1 *x* Corruption (Event Indicator *x* Regulated2 *x* Corruption) and their confidence intervals at 95% level, shown as vertical bars. The event indicators are coded individually for T-4, T-3, T-2, T-1, T, T+1, T+2, and T+3 where T=2016, i.e., the year of the *McDonnell v. United States*. The estimation uses a fully-saturated model where we set the base year as the year immediately before the *McDonnell v. United States* (i.e., year T-1, shown on the horizontal as a value of zero) by omitting the dummy variable for that year. Included in Panel A (B), but not shown, are controls for ROA, log firm assets, Corruption, Regulated1 (Regulated2), Event Indicators, Regulated1 *x* Corruption (Regulated2 *x* Corruption), Regulated1 *x* Event Indicators (Regulated2 *x* Event Indicators), Event Indicators *x* Corruption and firm fixed effects. All variables are defined in Table 2. All firm financials are winsorized at the 2% level in each tail. We include firm fixed effects. The dashed vertical line marks the event time.

Panel B: Regulation proxied by *Regulated2*:

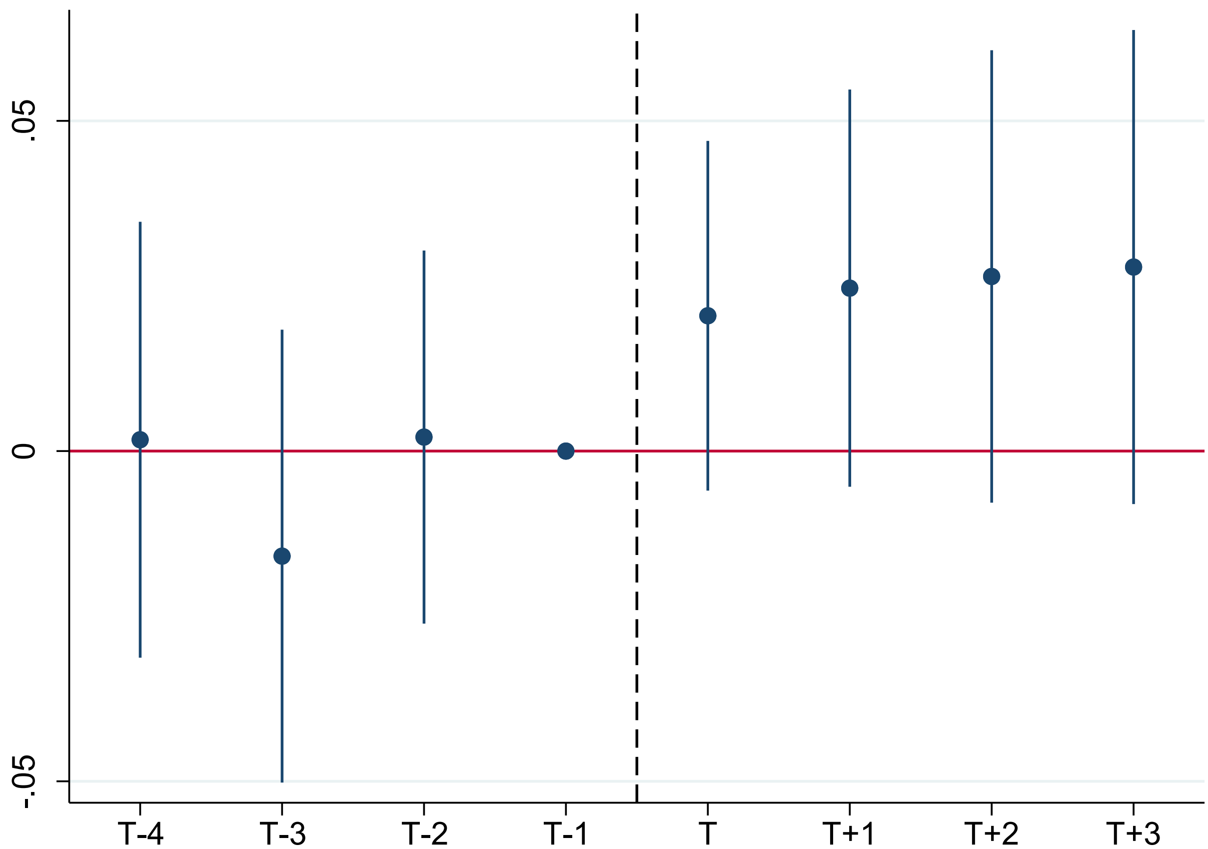

Table 1. Bribery Indictments Before and After McDonnell v. United States

We present a list of available federal cases of bribe indictments before (panel A) and after (panel B) the *McDonnell v. United States* decision. Panel C presents the non-parametric test of equality of the means and the medians of the two (before and after) samples of bribe indictments' amounts, along with their p-values. We also show the linear regression intercept of regressing log bribe amounts on the indicator for post-*McDonnell*.

Panel A: Before McDonnell (total of ***25*** *cases found on Westlaw Precision)*

Case Name	Circuit	Cite	Decision Year	Bribe Amount ($)
United States v. Dean	DC	629 F.3d 257	2011	1,275
United States v. Loza	DC	2011 WL 722376	2011	26,300
Harvey v. United States	4	2011 WL 450067	2011	43,000
Merker v. United States	11	2011 WL 13311839	2011	12,500
United States v. Ring	DC	811 F.Supp.2d 359	2011	915,000
United States v. Rezko	7	776 F.Supp.2d 651	2011	250,000
United States v. McGregor	11	2011 WL 1576950	2011	212,000
Morales v. United States	5	2011 WL 1528101	2011	9,000
United States v. Siegelman	11	640 F.3d 1159	2011	500,000
United States v. Bazezew	DC	783 F.Supp.2d 160	2011	110,000
United States v. Curry	4	2011 WL 3439942	2011	245,817
United States v. Turner	DC	818 F.Supp.2d 207	2011	1,000
United States v. Wilkes	9	662 F.3d 524	2011	625,000
United States v. Van Pelt	3	2011 WL 4925864	2011	10,000
United States v. Beldini	3	443 Fed. Appx. 709	2011	20,000
United States v. Bryant	3	655 F.3d 232	2011	113,167
United States v. Boender	7	649 F.3d 650	2011	38,000
United States v. Shoemaker	5	2012 WL 313620	2012	268,000
United States v. Manzo	3	851 F.Supp.2d 797	2012	10,000
United States v. Toth	6	668 F.3d 374	2012	62,000
United States v. Scruggs	5	916 F.Supp.2d 670	2012	50,000
United States v. Tremusini	8	688 F.3d 547	2012	20,000
United States v. Teel	5	691 F.3d 578	2012	140,000
United States v. Blackett	3	481 Fed. Appx. 741	2012	1,500
Gil Ramirez Group, LLC v. Houston Independent School Dist.	5	2012 WL 5633880	2012	36,000

*Panel B: Post McDonnell (total of **10** cases found on Westlaw Precision)*

Case Name	Circuit	Cite	Decision Year	Bribe Amount ($)
United States v. Burnette	11	2021 WL 5987025	2021	140,000
United States v. Percoco	2	13 F.4th 180	2021	320,000
United States v. McClain	7	2022 WL 488944	2022	2,800,000
United States v. Lindberg	4	39 F.4th 151	2022	500,000
United States v. Bailey	DC	2022 WL 4379059	2022	10,000
United States v. Gerace	2	2022 WL 17478270	2022	250,000
United States v. Hamilton	5	46 F.4th 389	2022	47,000
United States v. Bongiovanni	2	2022 WL 17481884	2022	250,000
United States v. Fernandez	11	2022 WL 3581793	2022	179,679
United States v. Charbonier-Laureano	1	2021 WL 4142403	2021	148,200

Panel C: Comparing Before- vs. After- McDonnell

Test	Bribe Average ($)	Statistic (p-value)
Mean for Before McDonnell	$188,782	
Mean for After McDonnell	$464,487	
Difference	$275,705	
Linear Regression (T-Statistic)		2.21^{**} (0.034)
*Rank Sum Non-parametric test of difference in **means** (Z-statistic)*		1.83^{*} (0.068)
*Non-parametric test of difference in **medians** (Pearson Chi-Square Statistic)*		5.54^{**} (0.019)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2. Variable Definitions

	Definition	Source
Cash Ratio Variables		
Regulated1	Regulated industry per Agrawal and Jaffe (2003), Billio et al. (2012), Masulis and Reza (2015). Defined at the SIC-2 level. Regulated SIC-2 codes include: 37, 40, 45, 47, 48, 49, 60, 61, 62, 63, or 64.	*Compustat-Capital IQ.*
Regulated2	Industry average per Q1 of each year of the political (institutions) risk measure from Hassan et al. (2019).	Hassan et al. (2019)
Cash Ratio	Cash/Assets.	*Compustat-Capital IQ.*
Corruption	Level of corruption per each state of location. Public corruption convictions per 10,000 population 1976-2010.	*U.S. Department of Justice.*
Ln(Assets)	Log(Assets).	*Compustat-Capital IQ.*
ROA	Net Income/Assets.	*Compustat-Capital IQ.*
Lobbying Expenditures	The natural logarithm of 1+the total yearly dollar amount of lobbying expenses by the firm.	*LobbyView.org*
Faulkender and Wang (2006) Variables		
$\Delta C/M_{i,t-1}$	Change in cash, t-1 to t.	*Compustat-Capital IQ.*
$\Delta E_{i,t}/M_{i,t-1}$	Change in earnings before interest and extraordinary items, t-1 to t.	*Compustat-Capital IQ.*
$\Delta NA_{i,t}/M_{i,t-1}$	Change in total assets net of cash, t-1 to t.	*Compustat-Capital IQ.*
$\Delta RD_{i,t}/M_{i,t-1}$	Change in R&D expenditure, t-1 to t.	*Compustat-Capital IQ.*
$\Delta I_{i,t}/M_{i,t-1}$	Change in interest expense, t-1 to t.	*Compustat-Capital IQ.*
$\Delta D_{i,t}/M_{i,t-1}$	Change in dividends, t-1 to t.	*Compustat-Capital IQ.*
$L_{i,t}$	Leverage, at t.	*Compustat-Capital IQ.*

$NF_{i,t}/M_{i,t-1}$	Net financing, at t.	*Compustat-Capital IQ.*
Event Study Variables		
[-1,1]	Cumulative market-adjusted return -1 to +1 trading days around 06/27/2016.	*CRSP.*
[0,1]	Cumulative market-adjusted return 0 to +1 trading days around 06/27/2016.	*CRSP.*
Violation Tracker Variables		
Number of penalties	Number of penalties in a year per company.	*Violation Tracker.*
Penalty indicator	One if the number of penalties>0, zero otherwise.	*Violation Tracker.*
Number of federal penalties	Number of federal penalties in a year per company.	*Violation Tracker.*
Number of state penalties	Number of state penalties in a year per company.	*Violation Tracker.*
Subsidy Tracker Variables		
Log(total federal subsidies)	Logarithm of the total federal subsidies plus one dollar, per firm year.	*Subsidy Tracker.*
Log(total state subsidies)	Logarithm of the total state subsidies plus one dollar, per firm year.	*Subsidy Tracker.*

Table 3. Descriptive Statistics

Table 3 shows aummary statistics for the main variables. We present the number of observations, mean, median and standard deviation.

	#	Mean	Median	St. Dev.
Cash Ratio Sample				
Cash Ratio	36,634	0.16	0.06	0.23
Regulated1	36,634	0.25	0.00	0.43
Regulated2	36,634	7.67	7.71	0.30
Post	36,634	0.55	1.00	0.50
Corruption	36,634	0.84	0.79	0.33
Ln(Assets)	36,634	6.01	6.45	2.90
ROA	36,634	-0.40	0.01	1.51
Event Study Sample				
CAR [-1,1]	3,099	-0.003	-0.003	0.056
CAR [0,1]	3,099	-0.008	-0.000	0.041
Regulated1	3,099	0.27	0.00	0.44
Regulated2	3,099	7.66	7.71	0.32
Cash Ratio	3,099	0.12	0.06	0.15
Corruption	3,099	0.85	0.83	0.33
Ln(Assets)	3,099	7.03	7.07	2.06
ROA	3,099	-0.03	0.02	0.19
Violation Tracker Sample				
Number Of Penalties	36,602	0.03	0.00	0.21
Penalty Indicator	36,602	0.02	0.00	0.14
Number of Federal Penalties	36,602	0.03	0.00	0.21
Number of State Penalties	36,602	0.02	0.00	0.14
Regulated1	36,602	0.25	0.00	0.43
Regulated2	36,602	7.67	7.71	0.30
Post	36,602	0.55	1.00	0.50
Corruption	36,602	0.84	0.79	0.33
Ln(Assets)	36,602	6.01	6.45	2.90
ROA	36,602	-0.40	0.01	1.51
Subsidy Tracker Sample				
Ln(1+Total Federal Subsidies)	36,633	0.14	0.00	1.41
Ln(1+Total State Subsidies)	36,633	0.91	0.00	3.31
Regulated1	36,633	0.25	0.00	0.43
Regulated2	36,633	7.67	7.71	0.29
Post	36,633	0.55	1.00	0.50
Corruption	36,633	0.84	0.79	0.33
Ln(Assets)	36,633	6.01	6.45	2.9
ROA	36,633	-0.40	0.01	1.51

Table 4. The Effect of *McDonnell v. United States* on Cash Policy

This table presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. All firm financials are winsorized at the 2% level in each tail. We include firm fixed effects. Standard errors are clustered at the firm level. Included in the regressions, yet omitted due to collinearity with the firm fixed effect, are: Regulated1 (Regulated2), Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). T-statistics are shown in parentheses. Model 1 (2) implies an economic effect of 0.75 (0.30) percentage points increase in cash ratio. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)
Dep. Variable:	*Cash Ratio, t, post-McDonnell*	
Variables:		
Post	-0.001	0.241***
	(-0.174)	(2.863)
Post *x* Corruption	-0.012*	-0.223**
	(-1.856)	(-2.328)
Regulated1 *x* Post	-0.008	
	(-0.988)	
Regulated1 *x* Post *x* Corruption	0.022***	
	(2.629)	
Regulated2 *x* Post		-0.032***
		(-2.931)
Regulated2 *x* Post *x* Corruption		0.029**
		(2.306)
ROA	0.007***	0.007***
	(2.864)	(2.856)
Ln(Assets)	-0.035***	-0.035***
	(-11.185)	(-11.164)
Observations	36,654	36,634
R-squared	0.787	0.787
Firm Fixed Effects	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5. McDonnell v. United States Event Study

This table presents the results of a linear regression model where the dependent variable is the cumulative abnormal returns over the event window specified in each column ([-1,1], [0,1]), on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. All firm financials are winsorized at the 2% level. Standard errors are clustered at the industry (SIC-2) level. T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)
Dep. Variable:	*CAR [-1,1]*	*CAR [0,1]*	*CAR [-1,1]*	*CAR [0,1]*
Variables:				
Regulated1	0.007	0.003		
	(0.449)	(0.396)		
Regulated2			0.018	0.012
			(0.853)	(1.241)
Regulated1 *x* Cash Ratio	-0.009	-0.011		
	(-0.136)	(-0.281)		
Regulated1 *x* Corruption	0.003	0.003		
	(0.275)	(0.515)		
Regulated1 *x* Cash Ratio *x* Corruption	-0.102**	-0.060*		
	(-2.476)	(-1.962)		
Regulated2 *x* Cash Ratio			-0.016	0.008
			(-0.204)	(0.160)
Regulated2 *x* Corruption			0.003	0.001
			(0.309)	(0.222)
Regulated2 *x* Cash Ratio *x* Corruption			-0.129**	-0.093*
			(-2.200)	(-1.740)
Cash Ratio	-0.056**	-0.020	0.067	-0.080
	(-2.576)	(-1.116)	(0.115)	(-0.209)
Cash Ratio *x* Corruption	0.041	0.029	1.011**	0.732*
	(1.471)	(1.148)	(2.228)	(1.735)
Ln(Assets)	-0.008***	-0.002**	-0.008***	-0.002**
	(-5.083)	(-2.393)	(-5.466)	(-2.537)
ROA	0.022***	0.005	0.019**	0.004
	(3.404)	(0.754)	(2.253)	(0.447)
Observations	3,101	3,101	3,099	3,099
R-squared	0.068	0.011	0.070	0.013

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6. The Value of Cash Following *McDonnell v. United States*

This table presents the results of a linear regression model as in Faulkender and Wang (2006) where the dependent variable is the excess stock return on the change in cash and its interactions with Post, Corruption, Regulated1 (Regulated2), and control variables as in Faulkender and Wang (2006) (change in earnings before interest and extraordinary items, change in total assets net of cash, change in R&D expenditure, change in interest expense, change in dividends, lagged cash, net financing, and leverage), defined in Table 2. All firm financials are winsorized at the 2% level in each tail. We include firm fixed effects (1,285 unique firms). Included but omitted due to collinearity with the fixed effects are: Regulated1 (Regulated2), Post, Corruption, Regulated *x* Post, and Post *x* Corruption. T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)
Dep. Variable:	*Change in Cash*	*Change in Cash*
Variables:	Regulated = Regulated1	Regulated = Regulated2
Delta Cash	1.985***	10.640
	(6.081)	(1.384)
Regulated *x* Delta Cash	-1.882***	-1.274
	(-4.644)	(-1.285)
Post *x* Delta Cash	-1.367***	-8.287
	(-3.727)	(-0.977)
Regulated *x* Post *x* Delta Cash	1.348***	1.025
	(2.761)	(0.936)
Delta Cash *x* Corruption	-1.408***	-6.075
	(-3.822)	(-0.722)
Regulated *x* Corruption	-0.006	-0.365
	(-0.010)	(-0.389)
Regulated *x* Delta Cash *x* Corruption	1.945***	0.765
	(4.312)	(0.705)
Post *x* Delta Cash *x* Corruption	1.243***	6.093
	(3.041)	(0.664)
Regulated *x* Post *x* Corruption	0.003	-0.085
	(0.051)	(-0.820)
Regulated *x* Post *x* Delta Cash *x* Corruption	-1.689***	-0.781
	(-3.213)	(-0.660)

(continued on next page)

Observations	3,783	3,783
R-squared	0.652	0.647
Firm Fixed Effects	Yes	Yes
Industry-Year Fixed Effects	Yes	Yes
State-Year Fixed Effects	Yes	Yes

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7. The Likelihood of Penalties

This table presents the results of a linear regression model where the dependent variable is a penalty indicator in columns (1) and (3) and the number of firm penalties in columns (2) and (4) per the Violation Tracker on Post, Corruption, Regulated1(Regulated2) in column 1 and 3 (2 and 4), their interactions, and controls, defined in Table 2. All firm financials are winsorized at the 2% level. Standard errors are clustered at the firm level, and we include firm fixed effects and state-year fixed effects. Included but omitted due to collinearity with the fixed effects are: Regulated1 (Regulated2), Corruption, Post, Regulated1 *x* Corruption, and Post *x* Corruption. The ***, **, and * denote significance at the 1%, 5%, and 10% levels. T-statistics are shown in parentheses.

	(1)	(2)	(3)	(4)
Variables:	*Penalty Indicator*	*Number of Penalties*	*Penalty Indicator*	*Number of Penalties*
Regulated1 *x* Post	0.024**	0.031*		
	(1.999)	(1.647)		
Regulated1 *x* Post *x* Corruption	-0.028**	-0.044**		
	(-2.232)	(-2.169)		
Regulated2 *x* Post			0.015	0.018
			(0.921)	(0.720)
Regulated2 *x* Post *x* Corruption			-0.035*	-0.040
			(-1.895)	(-1.496)
ROA	-0.001**	-0.001***	-0.001**	-0.001***
	(-2.244)	(-2.793)	(-2.285)	(-2.808)
Ln (Assets)	0.001	0.003**	0.001	0.003**
	(1.556)	(2.469)	(1.602)	(2.507)
Observations	36,622	36,622	36,602	36,602
R-squared	0.366	0.441	0.366	0.441
Firm Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8. Federal vs. State Penalties

This table presents the results of a linear regression model where the dependent variable is the number of federal (state) penalties in columns 1 and 3 (2 and 4), per the Violation Tracker on Post, Corruption, Regulated1 (Regulated2), their interactions and controls, defined in Table 2. All firm financials are winsorized at the 2% level. Standard errors are clustered at the firm level, and we include firm fixed effects and state-year fixed effects. Included in the regressions yet omitted due to collinearity with the firm fixed effects are: Regulated1 (Regulated2), Corruption, Post, Post *x* Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). The ***, **, and * denote significance at the 1%, 5%, and 10% levels. T-statistics are shown in parentheses.

	(1)	(2)	(3)	(4)
Variables:	*# Federal Penalties*	*# State Penalties*	*# Federal Penalties*	*# State Penalties*
Regulated1 *x* Post	-0.039	0.141**		
	(-0.159)	(2.386)		
Regulated1 *x* Post *x* Corruption	0.070	-0.160**		
	(0.247)	(-2.353)		
Regulated2 *x* Post			-0.244	0.265***
			(-0.799)	(3.175)
Regulated2 *x* Post *x* Corruption			0.145	-0.315***
			(0.409)	(-3.008)
ROA	0.005	-0.003**	0.005	-0.003**
	(0.580)	(-2.230)	(0.561)	(-2.241)
Ln (Assets)	-0.055**	0.008**	-0.054**	0.008**
	(-2.064)	(2.069)	(-2.043)	(2.036)
Observations	36,622	36,622	36,602	36,602
R-squared	0.199	0.241	0.200	0.242
Firm Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 9. Robustness Test: 2016 Presidential Election (Event Study)

This table presents the results of a linear regression model where the dependent variable is the cumulative abnormal returns over the event (i.e., Presidential Election in 2016) window specified in each column([-1,1], [0,1]), on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. All firm financials are winsorized at the 2% level. Standard errors are clustered at the industry (SIC-2) level. T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)
Dep. Variable:	*CAR [-1,1]*	*CAR [0,1]*	*CAR [-1,1]*	*CAR [0,1]*
Variables:				
Regulated1	0.008	0.017		
	(0.456)	(0.925)		
Regulated2			-0.026	-0.021
			(-1.365)	(-1.050)
Regulated1 *x* Cash Ratio	-0.043	-0.057		
	(-0.382)	(-0.498)		
Regulated1 *x* Corruption	0.004	-0.001		
	(0.447)	(-0.143)		
Regulated1 *x* Cash Ratio *x* Corruption	0.043	0.035		
	(0.545)	(0.486)		
Regulated2 *x* Cash Ratio			0.203*	0.231*
			(1.771)	(1.987)
Regulated2 *x* Corruption			0.006	0.001
			(0.492)	(0.110)
Regulated2 *x* Cash Ratio *x* Corruption			-0.021	-0.046
			(-0.243)	(-0.587)
Cash Ratio	0.024	0.017	-1.532*	-1.762**
	(0.782)	(0.502)	(-1.777)	(-2.017)
Cash Ratio *x* Corruption	-0.004	0.001	0.148	0.346
	(-0.126)	(0.036)	(0.226)	(0.583)
Ln(Assets)	0.001	0.001	0.002	0.002
	(0.529)	(0.358)	(1.040)	(0.903)
ROA	-0.049**	-0.052**	-0.052**	-0.057***
	(-2.050)	(-2.484)	(-2.389)	(-2.962)
Observations	3,181	3,181	3,179	3,179
R-squared	0.025	0.032	0.030	0.036

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10. Robustness Test: 2016 Presidential Election (Cash Policy)

Panel A presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Trump State, Regulated1 (Regulated2), and controls, defined in Table 2. All firm financials are winsorized at the 2% level in each tail. Standard errors are clustered at the firm level. We include firm fixed effects. Included in the regressions, yet omitted due to collinearity with the firm fixed effects are: Regulated1 (Regulated2), Trump State, and Regulated1 *x* Trump State (Regulated2 *x* Trump State). T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel A: Trump State effect

	(1)	(2)
Dep. Variable:	*Cash Ratio, t, post-McDonnell*	
Variables:	Main Regulation Proxy, Regulated1	Alternative Regulation Proxy, Regulated2
Post	-0.013***	0.041
	(-4.723)	(0.951)
Post *x* Trump State	0.008*	0.010
	(1.846)	(0.168)
Regulated1 *x* Post	0.011***	
	(3.004)	
Regulated1 *x* Post *x* Trump State	-0.002	
	(-0.344)	
Regulated2 *x* Post		-0.007
		(-1.191)
Regulated2 *x* Post *x* Trump State		-0.000
		(-0.035)
ROA	0.007***	0.007***
	(2.844)	(2.835)
Ln (Assets)	-0.035***	-0.035***
	(-11.146)	(-11.136)
Observations	36,654	36,634
R-squared	0.787	0.787
Firm Fixed Effects	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10. Robustness Test: 2016 Presidential Election (Cash Policy) (Continued)

Panel B presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. We include only firms with below-sample mean of foreign sales to total sales ratio. All firm financials are winsorized at the 2% level in each tail. We include firm fixed effects. Standard errors are clustered at the firm level. Included in the regressions yet omitted due to collinearity with the firm fixed effects are: Regulated1 (Regulated2), Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). T-statistics are shown in parentheses. Model 1 (2) implies an economic effect of 0.82 (0.297) percentage points increase in cash ratio. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel B: Firms with Low Share of Foreign Sales from Total Sales

	(1)	(2)
Dep. Variable:	*Cash Ratio, t, post-McDonnell*	
Variables:	Main Regulation Proxy, Regulated1	Alternative Regulation Proxy, Regulated2
Post	-0.001	0.230**
	(-0.095)	(2.211)
Post *x* Corruption	-0.015*	-0.246**
	(-1.722)	(-2.042)
Regulated1 *x* Post	-0.010	
	(-0.992)	
Regulated1 *x* Post *x* Corruption	0.029***	
	(2.772)	
Regulated2 *x* Post		-0.031**
		(-2.296)
Regulated2 *x* Post *x* Corruption		0.032**
		(2.047)
ROA	0.007***	0.007***
	(2.697)	(2.695)
Ln(Assets)	-0.036***	-0.036***
	(-9.997)	(-9.996)
Observations	26,592	26,572
R-squared	0.793	0.793
Firm Fixed Effects	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11. Robustness: Matching Regressions

This table presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. We use the matching of regulated firms to less-regulated firms by firm size and ROA. Columns (1) and (2) show matching by the nearest neighbor. Columns (3) and (4) use entropy balancing. Standard errors are clustered at the firm level. We include firm fixed effects. Included in the regressions, yet omitted due to collinearity with the firm fixed effect, are: Regulated1 (Regulated2), Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). All firm financials are winsorized at the 2% level in each tail. T-statistics are shown in parentheses.The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)
Dep. Variable:	*Cash Ratio, t, post-McDonnell*			
	Nearest Neighbor Matching	Nearest Neighbor Matching	Entropy Balancing	Entropy Balancing
Variables:				
Post	-0.002	0.188**	-0.003	0.149**
	(-0.318)	(2.302)	(-0.718)	(2.181)
Post *x* Corruption	-0.005	-0.174**	-0.004	-0.128*
	(-0.881)	(-1.974)	(-0.928)	(-1.732)
Regulated1 *x* Post	-0.008		-0.006	
	(-0.990)		(-0.833)	
Regulated1 *x* Post *x* Corruption	0.015*		0.014**	
	(1.937)		(2.031)	
Regulated2 *x* Post		-0.025**		-0.020**
		(-2.360)		(-2.235)
Regulated2 *x* Post *x* Corruption		0.023**		0.017*
		(1.983)		(1.725)
ROA	0.004	0.004	0.002	0.002
	(0.899)	(0.904)	(0.721)	(0.717)
Ln (Assets)	-0.034***	-0.034***	-0.035***	-0.035***
	(-8.421)	(-8.400)	(-11.616)	(-11.575)
Observations	29,702	29,689	36,654	36,634
R-squared	0.833	0.833	0.789	0.789
Firm Fixed Effects	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12. Placebo Tests: Alternative Event Dates

This table presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1, and controls, defined in Table 2. This table presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. All firm financials are winsorized at the 2% level in each tail. We include firm fixed effects. Standard errors are clustered at the firm level. Included in the regressions, yet omitted due to collinearity with the firm fixed effect, are: Regulated1 (Regulated2), Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels. Panel A assumes a placebo treatment event year of 2012, and Panel B assumes a placebo treatment event year of 2018.

Panel A: Randomized event year is 2012

	(1)	(2)
Dep. Variable:	*Cash Ratio, t, post-McDonnell*	
Variables:		
Post	-0.013**	0.029
	(-2.423)	(0.353)
Post *x* Corruption	0.008	-0.050
	(1.273)	(-0.577)
Regulated1 *x* Post	0.009	
	(1.050)	
Regulated1 *x* Post *x* Corruption	-0.006	
	(-0.761)	
Regulated2 *x* Post		-0.005
		(-0.495)
Regulated2 *x* Post *x* Corruption		0.007
		(0.655)
ROA	0.010***	0.010***
	(3.233)	(3.237)
Ln(Assets)	-0.025***	-0.025***
	(-7.854)	(-7.867)
Observations	38,431	38,408
R-squared	0.779	0.779
Firm Fixed Effects	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12. Placebo Tests: Alternative Event Dates (Continued)

This table presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1, and controls, defined in Table 2. This table presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. All firm financials are winsorized at the 2% level in each tail. We include firm fixed effects. Standard errors are clustered at the firm level. Included in the regressions, yet omitted due to collinearity with the firm fixed effect, are: Regulated1 (Regulated2), Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels. Panel A assumes a placebo treatment event year of 2012, and Panel B assumes a placebo treatment event year of 2018.

Panel B: Randomized event year is 2018

	(1)	(2)
Dep. Variable:	*Cash Ratio, t, post-McDonnell*	
Variables:		
Post	0.014**	0.049
	(2.101)	(0.531)
Post *x* Corruption	-0.010	-0.015
	(-1.521)	(-0.160)
Regulated1 *x* Post	-0.006	
	(-0.676)	
Regulated1 *x* Post *x* Corruption	0.008	
	(0.842)	
Regulated2 *x* Post		-0.005
		(-0.405)
Regulated2 *x* Post *x* Corruption		0.001
		(0.079)
ROA	0.014***	0.014***
	(3.944)	(3.939)
Ln(Assets)	-0.018***	-0.018***
	(-6.000)	(-5.994)
Observations	34,754	34,735
R-squared	0.788	0.788
Firm Fixed Effects	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13. Robustness Regressions: Additional Fixed Effects Controls

This table presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. Panel A utilizes Regulated1, while Panel B utilizes Regulated2. All firm financials are winsorized at the 2% level in each tail. Standard errors are clustered at the firm level. We also include firm fixed effects and year fixed effects (Column 1), firm fixed effects and industry-year fixed effects (Column 2), firm fixed effects and state-year fixed effects (Column 3), and firm fixed effects, industry-year fixed effects and state-year fixed effects (Column 4). Included in the regressions yet omitted due to collinearity with the firm fixed effect, are: Post, Regulated1 (Regulated2), Corruption, Regulated1 *x* Post (Regulated2 *x* Post), Post *x* Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel A

	(1)	(2)	(3)	(4)
Dep. Variable:	*Cash Ratio, t, post-McDonnell*	*Cash Ratio, t, post-McDonnell*	*Cash Ratio, t, post-McDonnell*	*Cash Ratio, t, post-McDonnell*
Variables:				
Regulated1 *x* Post *x* Corruption	0.022***	0.023***	0.027***	0.029***
	(2.647)	(2.760)	(3.110)	(3.247)
ROA	0.007***	0.007***	0.007***	0.007***
	(2.749)	(2.805)	(2.711)	(2.793)
Ln(Assets)	-0.034***	-0.032***	-0.033***	-0.032***
	(-10.504)	(-9.757)	(-10.145)	(-9.540)
Observations	36,654	36,639	36,653	36,638
R-squared	0.787	0.790	0.789	0.792
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	Yes
Industry-Year Fixed Effects	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13. Robustness Regressions: Additional Fixed Effects Controls (Continued)

This table presents the results of a linear regression model where the dependent variable is the cash ratio on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. Panel A utilizes Regulated1, while Panel B utilizes Regulated2. All firm financials are winsorized at the 2% level in each tail. Standard errors are clustered at the firm level. We also include firm fixed effects and year fixed effects (Column 1), firm fixed effects and industry-year fixed effects (Column 2), firm fixed effects and state-year fixed effects (Column 3), and firm fixed effects, industry-year fixed effects and state-year fixed effects (Column 4). Included in the regressions, yet omitted due to collinearity with the firm fixed effect, are: Post, Regulated1 (Regulated2), Corruption, Regulated1 *x* Post (Regulated2 *x* Post), Post *x* Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel B

	(1)	(2)	(3)	(4)
Dep. Variable:	*Cash Ratio, t, post-McDonnell*	*Cash Ratio, t, post-McDonnell*	*Cash Ratio, t, post-McDonnell*	*Cash Ratio, t, post-McDonnell*
Variables:				
Regulated2 *x* Post *x* Corruption	0.029**	0.024*	0.027**	0.023*
	(2.315)	(1.883)	(2.066)	(1.703)
ROA	0.007***	0.007***	0.007***	0.007***
	(2.741)	(2.806)	(2.700)	(2.791)
Ln(Assets)	-0.034***	-0.032***	-0.033***	-0.032***
	(-10.486)	(-9.762)	(-10.123)	(-9.546)
Observations	36,634	36,627	36,633	36,626
R-squared	0.787	0.790	0.789	0.792
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	Yes
Industry-Year Fixed Effects	No	Yes	No	Yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 14. Robustness Regressions: The Impact of *McDonnell v. United States* on Lobbying Expenditures

This table presents the results of a linear regression model where the dependent variable is the logarithm of 1 plus the aggregate annual dollar amount of lobbying expenses by each firm on Post, Corruption, Regulated1 (Regulated2), and controls, defined in Table 2. Column 1 utilizes Regulated1, while Column 2 utilizes Regulated2. All firm financials are winsorized at the 2% level in each tail. Standard errors are clustered at the firm level. We include firm fixed effects. Included in the regressions yet omitted due to collinearity with the firm fixed effects are: Regulated1 (Regulated2), Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). T-statistics are shown in parentheses. The ***, **, and * denote significance at the 1%, 5%, and 10% levels.

	(1)	(2)
Dep. Variable:	*Lobbying Expenses, t, post-McDonnell*	
Variables:		
Post	0.158	2.120
	(1.533)	(0.997)
Post *x* Corruption	-0.154	-3.489
	(-1.296)	(-1.512)
Regulated1 *x* Post	-0.082	
	(-0.436)	
Regulated1 *x* Post *x* Corruption	0.087	
	(0.446)	
Regulated2 *x* Post		-0.258
		(-0.927)
Regulated2 *x* Post *x* Corruption		0.436
		(1.451)
ROA	-0.065***	-0.065***
	(-6.587)	(-6.581)
Ln(Assets)	0.248***	0.248***
	(8.668)	(8.681)
Observations	36,654	36,634
R-squared	0.879	0.879
Firm Fixed Effects	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 15. Robustness Regressions: Federal and State Subsidies

This table presents the results of a linear regression model where the dependent variable is the natural logarithm of one plus the dollar amount of federal (state) subsidies in columns 1 and 3 (2 and 4), per the Subsidy Tracker on Post, Corruption, Regulated1 (Regulated2), their interactions and controls, defined in Table 2. All firm financials are winsorized at the 2% level. Standard errors are clustered at the firm level, and we include firm fixed effects and state-year fixed effects. Included in the regressions yet omitted due to collinearity with the firm fixed effects are: Regulated1 (Regulated2), Corruption, Post, Post *x* Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). The ***, **, and * denote significance at the 1%, 5%, and 10% levels. T-statistics are shown in parentheses.

	(1)	(2)	(3)	(4)
Dep. Variable:	*Ln(1+Tot.Federal Subsidies)*	*Ln(1+Tot. State Subsidies)*	*Ln(1+Tot.Federal Subsidies)*	*Ln(1+Tot. State Subsidies)*
Variables:				
Regulated1 *x* Post	-0.040	-0.280		
	(-0.333)	(-1.299)		
Regulated1 *x* Post *x* Corruption	0.068	0.218		
	(0.606)	(0.954)		
Regulated2 *x* Post			-0.101	-0.491
			(-0.751)	(-1.622)
Regulated2 *x* Post *x* Corruption			0.111	0.435
			(0.882)	(1.243)
ROA	0.004	-0.035***	0.003	-0.035***
	(1.244)	(-4.416)	(1.227)	(-4.403)
Ln(Assets)	-0.014	0.146***	-0.013	0.146***
	(-1.468)	(5.926)	(-1.449)	(5.940)
Observations	36,653	36,653	36,633	36,633
R-squared	0.610	0.609	0.610	0.609
Firm Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 16. Robustness Regressions: Rise in SG&A After *McDonnell*

This table presents the results of a linear regression model where the dependent variable is the ratio of sales, general and administrative expenses (SG&A) to total sales on Post, Corruption, Regulated1 (Regulated2), their interactions, and controls, defined in Table 2. All firm financials are winsorized at the 2% level in each tail. Standard errors are clustered at the firm level, and we include firm fixed effects. Included in the regressions yet omitted due to collinearity with the firm fixed effects are: Regulated1 (Regulated2), Corruption, and Regulated1 *x* Corruption (Regulated2 *x* Corruption). The ***, **, and * denote significance at the 1%, 5%, and 10% levels. T-statistics are shown in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Variable:	*SG&A/Sales, t+2*	*SG&A/Sales, t+3*	*SG&A/Sales, t+4*	*SG&A/Sales, t+2*	*SG&A/Sales, t+3*	*SG&A/Sales, t+4*
Variables:						
Post	-0.021	-0.003	0.052	1.479	1.151	1.196
	(-0.414)	(-0.059)	(1.026)	(1.496)	(1.395)	(1.592)
Post *x* Corruption	0.002	-0.016	-0.042	-2.246*	-1.722*	-1.420*
	(0.051)	(-0.338)	(-0.873)	(-1.866)	(-1.691)	(-1.802)
Regulated1 *x* Post	-0.089	-0.134	-0.096			
	(-0.767)	(-1.217)	(-0.862)			
Regulated1 *x* Post *x* Corruption	0.203	0.251*	0.189*			
	(1.354)	(1.855)	(1.689)			
Regulated2 *x* Post				-0.201	-0.155	-0.153
				(-1.510)	(-1.395)	(-1.499)
Regulated2 *x* Post *x* Corruption				0.301*	0.230*	0.186*
				(1.864)	(1.677)	(1.735)
ROA	-0.077*	0.081*	0.078*	-0.077*	0.081*	0.077*
	(-1.825)	(1.790)	(1.789)	(-1.827)	(1.780)	(1.775)
Ln(Assets)	-0.128***	-0.089***	-0.105**	-0.128***	-0.090***	-0.105**
	(-3.730)	(-2.607)	(-2.534)	(-3.725)	(-2.602)	(-2.518)

(continued on next page)

Observations	20,803	18,579	14,933	20,791	18,568	14,924
R-squared	0.809	0.823	0.839	0.809	0.822	0.839
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$