The Firm-Level Effects of Political Connections to State Attorneys General

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April 22, 2022

Abstract

Legal investigations and penalties can pose a considerable risk and uncertainty for firms, and there is anecdotal evidence that political connections to the judicial branch may contribute to lowering such risk. Yet, extant literature has not focused on connections to that branch of government. I leverage state-level campaign contribution data and employ a regression discontinuity design around state attorneys general elections to provide suggestive evidence that political connections lead to lower investigation probabilities and lower penalties for firms. Moreover, I demonstrate that connections lead to increases in firms' investments. Thus, I shed light on a new channel for firms' political activities, and show that connections to the judicial branch entails private benefits for companies. The observed investment effects may be inefficient as capital may be misallocated across firms.

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[†]I thank Frances Rosenbluth, Giovanni Maggi, Ebonya Washington, Susan Rose-Ackerman, Tim Armstrong, Thomas Braeuninger, In Song Kim, Christian Cox, Daniel Goldstein, and the participants of the 2020 APSA panel on business and politics, the 2022 MPSA panel on firms and politics, as well as the participants of the Yale Political Economy Student Workshop and the Mannheim SFB Political Economy of Reforms Early Insights Workshop for helpful feedback.

1 Introduction

"It's time to reset the clock, to turn back the dial and to reconsider how attorney general offices are operating... The agenda in these offices has to be driven by the most important issues facing the attorneys general, not by contributors." - James E. Tierney, former Maine attorney general (Lipton (2014a))

Individual and corporate campaign contributions to candidates for state attorney general (state AG henceforth), and their potential to influence the prosecutorial agenda of state AGs have been highlighted as problematic in journalistic accounts. Due to the nature of the subject, the direct effects of campaign contributions and political connections are hard to detect. Anecdotal evidence however supports the presumption that law enforcement by state AGs may be affected by donations and political connections: in mid 2013, New York AG Eric Schneiderman had filed a lawsuit against Trump University. On September 13, 2013, Pam Bondi - then state AG in Florida - announced that her office considered joining the lawsuit since 24 complaints had been filed against Trump University in the state of Florida. Yet, roughly a month later, the Florida state AG office publicized the decision not to act on these complaints against Trump University however. What had happened in the meantime? Just four days after the initial announcement - on September 17, 2013 - Pam Bondi's political action committee had received a $25,000 donation from the Donald Trump Foundation (see Sack and Eder (2016)). While a definitive quid-pro-quo could not be established in this case, it is nevertheless suggestive of how corporate political activities might affect prosecutorial priorities of state AGs. A better understanding of these dynamics is warranted given that state AGs' decisions can have far-reaching consequences, as becomes obvious in light of high stakes lawsuits that state AGs have been pursuing in recent years: pharmaceutical company Johnson & Johnson for instance was ordered by an Oklahoma judge to pay $465 million for its role in the opioid crisis in a case that had been brought forward by Oklahoma state AG Mike Hunter (see Dwyer and Fortier (2019)) Exxon Mobil, the American Petroleum Institute, and Koch

Industries have been sued by the Minnesota state AG Keith Ellison in 2020 in a climate liability case,[1] and the New York state AG agreed with Intel on a $ 6.5 million settlement in an antitrust lawsuit in 2012.[2]

In this paper, I examine the firm-level effects of political connections to state AGs. In line with the above outlined anecdotal evidence, one may expect that political connections to state AGs can benefit companies in a quid-pro-quo sense, for example by lowering the probability of being investigated or paying lower penalties in the case of convictions or settlements. State AGs generally pursue cases against firms in the realms of consumer protection, antitrust, and environmental law enforcement, and - as mentioned previously - such cases my pose sizable risks for companies. On the other hand, campaign contributions in particular have primarily been understood by scholars as a "consumption good" (Ansolabehere et al. 2003; see also "Tullock's puzzle" Tullock (1972)), with contributions being ideologically motivated rather than perceived as an investment. If that were the case with contributions to state AGs, then one may not expect to observe any benefits for companies that established a political connection to a state AG by means of contributing to the campaign of a candidate that ended up winning a state AG election.

I study the effects of political connections of firms to state AGs empirically by focusing on publicly traded companies that made campaign contributions in state AG races between 1990 and 2018 in the 43 states where state AGs are elected.[3][4] I operationalize firms' political connections to state AGs by whether a company contributed to a candidate running for state AG in a given race.[5]

I find that the correlations between political connections to state AGs and firms' investigation probabilities and firms' investments, respectively, are neither statistically

[1]Seehttps://www.ag.state.mn.us/Office/Communications/2020/06/24_ExxonKochAPI.asp

[2]See https://www.reuters.com/article/us-intel-antitrust-idUSTRE8182HX20120209

[3]In the remaining seven states, state AGs are appointed by either the governor or the legislature.

[4]Note that campaign contributions data for the time period 1990 until 2000 were not available for all states.

[5]For the main specifications, I focus on direct corporate contributions or company PAC contributions. In a robustness check in the appendix, I also present results that operationalizes connections by whether the company, its PAC, or a member of the C-suite made contributions in a state AG race.

significant nor substantively sizable. However, as these correlations may be prone to bias due to omitted variables and reverse causality, I employ a regression discontinuity design (RDD) in order to estimate causal effects of firms' political connections to state AGs. I therefore zero in on observations where companies made contributions in close elections, thus comparing companies that were connected to candidates that won a state AG election by a narrow margin to companies that were connected to candidates that lost a state AG election by a narrow margin. By focusing on this set of observations, the RDD estimates local average treatment effects around the cutoff, i.e. for companies that contributed to state AG candidates in close races, as the treatment and control units in this neighborhood should be similar under some conditions that will be discussed in section 5.[6] Based on an RDD, I show that companies' political connections lead to reduced firm-level probabilities of being investigated by state AGs. The effects appear to be sizable, reducing the investigation probability by 3-6 percentage points depending on the specification; however, the results are not statistically significant in all specifications. Furthermore, at the intensive margin companies appear to benefit from connections by paying lower fines when they are convicted or settle cases brought forward by state AGs. Convicted or settling firms that were politically connected to state AGs pay two to three times lower fines than non-connected firms. Note, however, that these firm investigation and penalties results are based on relatively few cases within the RDD bandwidth where companies were investigated.

I complement these results by examining the effects of firms' political connections to state AGs on firm investment. Based on finance literature that suggests that firm-level uncertainty may depress investment decisions, one may expect that if political connections to state AGs entail private benefits to firms in the form of reduced investigation

[6] Importantly, if close elections are decided in an as-if random manner, then the RDD identification strategy should mitigate concerns about omitted variable bias such as some companies being inherently at greater risks of investigations by state AGs, which may endogenously affect their contribution behavior.

probabilities, such firms should increase their investments.[7][8] Based on RDD results, I indeed find that companies' political connections cause firm-level increases in net investment rates. Politically connected companies increase their investment rates by 4 to 7 percentage points more than non-connected firms; given a baseline net investment rate of 8-9 percent, this amounts to an increase in investment rates by more than 50 percent for politically connected companies. Based on the investigation, penalties, and investment RDDs taken together, I therefore conclude that firms' political connections to state AGs lead to private benefits for companies.

In the discussion section, I examine heterogeneous effects with respect to partisanship and accountability. I then show that the investment increases are not paralleled by firm-level productivity increases which may suggest that political connections to state AGs may come at welfare costs due to resource misallocation across firms.

To my knowledge, my paper is the first that focuses on political connections to state AGs as a channel through which firms in the US might arguably influence law enforcement. The focus on contributions and connections to law enforcement contrasts with the existing literature on firm-level political activities targeted at the legislative or executive branch. This institutional setup comes with the advantage of enabling me to reasonably trace the direct benefits - firm prosecutions and imposed fines by state AGs - and the more indirect downstream effects - increased firm-level investments - that companies may receive from connection to state AGs. The perspective of campaign contributions in my paper - understood as a vehicle to establish political connections - leads to results that contrast to previous literature that perceives of campaign contributions primarily as a "consumption good" (Ansolabehere et al. 2003). Lastly, my paper speaks to firms' political activities on the state level, which thus far has only been studied to a limited extent in the literature.

The paper is structured as follows: first, I will discuss the relevant literature, and give

[7] In appendix section section §C, I present a toy model that aims to formalize the link between firms' political connections to state AGs and firms' investment decisions.

[8] Firms' capital decisions are usually seen as more irreversible or lumpy than labor decisions, which is why one may particularly see uncertainty reductions to affect capital investment decisions of firms.

an overview of the role and responsibilities of state AGs in the US. I then present RDD results on firms' political connections and state AG investigations and penalties as well as firm-level investment, before discussing heterogeneous effects and welfare implications.

2 Literature

This paper is related to several strands of the literature. First, there is a large literature on firm-level political connections and campaign contributions. While the former have been shown to be beneficial for companies in various contexts (see e.g. Fisman (2001), Khwaja and Mian (2005), Faccio (2006), Fisman and Wang (2015), and Szakonyi (2018)), the role of the latter are less clear. In their seminal paper, Ansolabehere et al. (2003) argue that campaign contributions should be seen as consumption goods, rather than investments.[9] The idea is that contributors, among them executives in charge of companies' donations and corporate PAC donations, may simply enjoy expressing their political views through this avenue without expecting a return to the spent money. My findings on the other hand suggest that firms' campaign contributions - at least to candidates running for state AG - indeed should be seen as investments in political connections that reduce firms' latent probabilities of being investigated and lead to firm-level investment increases.

My paper is also related to the literature investigating lobbying strategies beyond the lobbying the legislature. McKay (2011) examines conditions under which interest groups lobby the bureaucracy rather than the legislature, Bertrand et al. (2018) examine the channel of charitable giving, and You (2017) suggests that actors might resort to influencing ex-post rule-making on the federal level in the US when particularistic benefits are at stake. In this sense, political connections to state AGs might serve a similar function: while state AGs do not affect the legislative process, connections to them may affect the stringency with which rules are being applied to specific companies ex-post. I am arguing that this potentially reduced stringency in law enforcement by state AGs

[9] This view is supported by Fowler et al. (2020)'s findings.

should affect firm-level outcomes, such as investment and size, through the reduction of uncertainty in firms' business environments. Here, my paper is related to work mainly in finance that shows how firm-level uncertainty depresses firms' investment decisions (see Panousi and Papanikolaou (2012), Kang et al. (2014), and Ovtchinnikov et al. (2019)).

Moreover, to the extent that I speculate about efficiency effects of political connections to state AGs, my paper speaks to a large (macro)economic literature on resource misallocation (see e.g. Restuccia and Rogerson (2008), Hsieh and Klenow (2009), and Restuccia and Rogerson (2017)), and the literature on its determinants. While several causes, such as financial frictions (see Midrigan and Xu (2014)) and property rights (see Adamopoulos and Restuccia (2014)) have been investigated in the literature, to my knowledge only Huneeus and Kim (2018) and Akcigit et al. (2018) touch upon political determinants of resource misallocation. While the former establishes a causal effect of lobbying on the federal level in the US on misallocation, the latter find that political connections to local government officials in Italy lead to misallocation of labor.

Lastly, I contribute to the literature on the role of state AGs more generally (see Provost (2006) and Silverman and Wilson (2016)), as well as to the growing literature on companies' political influence on the state level. For instance, Figueiredo Jr and Edwards (2007) study utility regulatory commissions decisions, Holburn and Vanden Bergh (2014) look at mergers and acquisitions in the electricity sector, and Do et al. (2015) study the effects of political connections to governors.

3 State AGs: Role and Responsibilities

The role and responsibilities of state AGs, as well as the requirements for candidates to run differ somewhat across states (Myers and Ross (2007)). Common across all states, state AGs are states' chief legal advisers and chief law enforcement officers. Contrary to the US federal attorney general, state AG is not a cabinet position in most states. In fact, 43 out of 50 states hold elections for state AG. In 6 of the remaining 7 states, the governor appoints the state AG, while in Maine the legislature elects the state AG. In the empirical part, I will focus on states that elect their state AGs, as my causal identification strategy hinges upon this institutional feature. Candidates for state AG may receive campaign contributions. While some states ban contributions from corporations, corporate PACs may donate to campaigns in all 43 states. The exact rules and limits once again differ across states (see e.g. NCSL (2010)). While competencies, budget and staff size of state AG offices also vary across states, the main fields almost all of them are active in include consumer protection, antitrust, and environmental law enforcement. As alluded to in the introduction, state AGs' actions are potentially very costly to prosecuted companies. These costs then may also have contributed to the considerable spending on recent state AG races in 2018 (e.g. totals of $10 million in Texas, $6.9 million in California, $6.6 million in Ohio, see Bennett (2018)). While my paper focuses on firms' contributions to state AG candidates, it should be noted though that there is also anecdotal evidence for further ways to influence state AGs, such as revolving doors, and donations by business associations and companies to the Republican and Democratic AG Associations (with the acronyms of RAGA and DAGA) (see Lipton (2014b)).

4 Data

For my quantitative analysis, I draw on campaign contribution data for state AG races that were kindly provided by the National Institute on Money in Politics (see NationalInstituteonMoneyinPolitics (2020)). The data has been consistently available for all states since 2000, and for selected states since 1990. The years covered in my study therefore are 1990 to 2018. Overall, my data builds on campaign contributions in 230 unique elections in all of the 43 states that elect state AGs.[10] The data allows me to identify direct con-

[10] As discussed below, the number of close elections is much smaller at 33 elections that were decided with less than a 5 percent margin, and 66 elections that were decided with less than a 10 percent margin.

tributions from firms as well as from corporate PACs; candidate characteristics such as partisan affiliation are also included in the data. I aggregated multiple donations of one company to the same candidate in the same election cycle. A firm in my dataset is coded as having a connection to a candidate running for state AG if the firm or an affiliated PAC had donated to the state AG's campaign before the election. I only kept US companies, and dropped companies that donated to both the winner and the loser in a given state AG race.[11] The question why - given often times low upper limits on corporate campaign contributions - firms do not always just donate to both candidates in the race in order to hedge their bets is one that is not easy to answer. While this happens, it only happens for a minority of firms. One could speculate that candidates do not see such contributions as credible signals of commitment, and would thus withhold benefits to the firm - such as turning a blind eye in case of regulatory violations on the firm's behalf - if elected as state AG. It should be noted, however, that this puzzle does not affect the validity of my causal identification strategy, which is outlined below.

Data on race-level electoral margins were taken from Dave Leip's US Election Atlas (Leip (2020)). Data regarding the partisanship of governors and state legislators in the respective states during the respective years come from ballotpedia.org (Ballotpedia (2020)).

Furthermore, I gathered data on state AGs' investigations and fines. These data come from GoodJobsFirst (2020)' Violation Tracker, Nolette (2020)'s Multistate Litigation Database, and NAAG (2020)'s state AG antitrust database. I connect a firm-candidate-election observation with data on investigations and fines if the firm was prosecuted by the state AG within the 4 years after the election.

Lastly, I draw on firm-level financial data from Compustat (see StandardandPoors (2020)). The main variable of interest in my analysis is the net investment rate [12].

[11] Note however that my results are robust to keeping these in my sample, or to coding as a connection only the firm-candidate-year observation per race with the higher donation amount.

[12] operationalized as net capital growth: $\frac{PPENT_{t+1}-PPENT_t}{PPENT_t}$; following Bai et al. (2019), I winsorized the investment rates at the 1% to 99% level.

I furthermore present specifications that use the variables assets (logged), market value (logged), and employment (logged). For additional results and placebo checks, I moreover examine productivity growth [13], labor growth[14], revenue growth[15], and sales growth[16].

I merged firm-level contribution data, investigations and fines data, and financial data based on firm names using a fuzzy merge algorithm. I end up with a dataset that contains firm-election observations as units of analysis.[17]

The reader is referred to section §ATable 1 and section §ATable 2, which present summary statistics for the full sample, and the sample of firms connected to state AGs that ran in close elections (margin closer than 5 percent) respectively.[18] The balance table in section §BFigure 3 suggests that most covariates are reasonably balanced for firms with political connections to losing and winning candidates for the state AG office in close elections; politically connected firms appear to have somewhat more employees than non-connected companies however.[19]

5 Results

While there are anecdotal accounts of how firms may benefit from political connections to state AGs, a systematic empirical account of the firm-level effects of political connections to state AGs is wanting. In this section, I will provide results that speak to whether

[13]productivity 'ACF' estimated by the method proposed in Ackerberg et al. (2015). Productivity growth is therefore operationalized as: $\frac{ACF_{t+1}-ACF_t}{ACF_t}$; following Bai et al. (2019), I winsorized the growth rates at the 1% to 99% level.

[14]operationalized as: $\frac{EMP_{t+1}-EMP_t}{EMP_t}$; following Bai et al. (2019), I winsorized the growth rates at the 1% to 99% level.

[15]operationalized as: $\frac{REVT_{t+1}-REVT_t}{REVT_t}$; following Bai et al. (2019), I winsorized the growth rates at the 1% to 99% level.

[16]operationalized as net capital growth: $\frac{SALE_{t+1}-SALE_t}{SALE_t}$; following Bai et al. (2019), I winsorized the growth rates at the 1% to 99% level.

[17]See appendix section section §D for a detailed account of how the data were collected and merged.

[18]Additionally, see section §ATable 3 for investigation probabilities and net investment rates for firms connected to the losing and winning candidates, respectively, by industry. For a graphical representation of mean investigation probabilities and net investment rates of connected vs unconnected firms respectively in close elections, see section §BFigure 1 and section §BFigure 2.

[19]In the results discussed below, I include employees (logged) as a covariate.

connections may indeed lead to quid-pro-quo benefits for companies. If that is the case, then one may expect political connections leading to lower investigation probabilities and fines for connected companies. The thus reduced business uncertainty should then translate into increased investments. If, on the other hand, campaign contributions to state AGs do not function as a means of establishing firm-level connections to state AGs that then yield quid-pro-quo benefits, but rather campaign contributions are consumption goods, then one should see null effects of political connections on the mentioned outcome variables.

First, I present correlations of connections and investigation probabilities and investment, respectively, for the full sample. Next, I discuss causal identification using regression discontinuity designs (RDD) and elaborate on investigation, penalty, and investment RDD results for firms.

Table 1 and Table 2 show the correlations of firms' political connections to state AGs and state AG investigations and firm investment, respectively, for the full sample of firms in my data. The tables suggest that there is no statistically significant correlation between political connections and firms' investigations and/or net investment, regardless of the specifications.[20] Moreover, the effects in all specifications is also substantively very small, with point estimates between -0.01 and 0.01.

The thus obtained results can naturally not be interpreted as causal as the regressions are prone to omitted variable bias. Companies that perceive themselves to be at higher risk of state AG investigations may be more inclined to seek out political connections to state AGs. Firms' investment decisions on the other hand are endogenous, and one might expect that firms would adjust their net investment rates in response to political circumstances only if these circumstances could not fully be priced in ex ante. In other words, if future connections to state AGs are known with a high degree of certainty and thus the uncertainty in a firm's business environment is accordingly reduced, then these

[20]The reported specifications include combinations of the following: industry, state, and year fixed effects, and the covariates employees (logged), assets (logged), and market value (logged).

companies should adjust their investment decisions in prior years already. Against that backdrop, the null results for the entire sample appear plausible. In this paper, I try and mitigate omitted variable bias problems by employing RDDs around close state AG elections. This approach allows me to causally identify the effects of political connections of firms to state AGs under a reasonable set of assumptions.

5.1 Research Design

Before elaborating on the results, some remarks on the research design, i.e. the regression discontinuity designs, employed in this paper are in order. As discussed above, I operationalize firms' political connections by whether they contributed to a candidate in a given state AG race. To recap, the timing is as follows: first, firms decide on campaign contributions they give to candidates in state AG races, thus establishing a political connection to a candidate. Second, a state AG is elected out of two eligible candidates (one Republican and one Democratic usually, and in all cases where the elections are close). Focusing on close elections, I can then estimate a causal effect of political connections on firm-level variables in after the election, such as investigation probabilities or penalties in periods $t + 1$ through $t + 4$, or the net investment rate in period $t + 1$, by analyzing the subset of firms that had contributed to either the winning or the losing candidates in close races.[21] Hence, while campaign contributions are endogenous, the as-if-random assignment of companies to a political connections occurs by means of close and hard to predict elections.[22]

The necessary identification assumption for RDDs is that potential outcomes are con-

[21] As noted above, I drop companies that contributed to both the winning and losing candidates in a given elections.

[22] Note that an alternative understanding of the RD result is state AGs rewarding firms for ideological proximity. If firms contribute to candidates for ideological reasons, and state AGs favor ideologically aligned firms, then the RD picks up the firm-level effects of ideologically aligned state AGs on firms. In my view, thinking of firms as ideological donors is less plausible than perceiving of firms as profit-motivated donors. Additionally, in this world one may expect partisan differences in connections to state AGs: if one assumes that Republicans are "the party of business", then the treatment effects whenever a Republican candidate is elected state AG should be larger. That, however, is not the case as can be seen in columns 3 and 4 section §ATable 16 and section §ATable 18.

tinuous in the running variable around the threshold. Under this condition, the RD estimator identifies local average treatment effects around the cutoff point, and is defined as follows:

$$\tau_{RD} = lim_{x \to 0^+} E[Y_i|X_i = x] - lim_{x \to 0^-} E[Y_i|X_i = x] \tag{1}$$

(see Hahn et al. (2001) and Calonico, Cattaneo, and Titiunik (2014)). In order to estimate these limit points, the researcher has to make decisions with regard to the flexibility of the estimating functions on both sides of the threshold - including the choice of kernels when local linear functions are chosen -, as well as with regard to the bandwidth that is considered on each side for estimating purposes; these decisions have implications for the bias of the estimating functions. Local linear functions with triangular kernels have been shown to perform well in these contexts (see Armstrong and Kolesár (2018)), which is why I have opted for these choices in my specifications. Different ways to deal with bandwidth selection and bias correction have been suggested in the literature: I rely on the CER-optimal bandwidth choice and the bias-correction suggested by Calonico et al. (see e.g. Calonico, Cattaneo, and Farrell (2020)); this approach currently appears to be used widely in political science and economics.[23]

Furthermore, some remarks about the estimation of standard errors are in order: since my units of analysis - firms - are connected to candidates for state AG office, and since the electoral choice between the two respective candidates in each race serves as the treatment assignment mechanism for firms, one needs to take into account the fact that firms are clustered by election. This is achieved on the one hand by clustering standard errors on the election-level. Beyond that, one might be concerned about the relatively small number of clusters I analyze in this paper: for instance, I have 33 unique election-level clusters when only analyzing state AG elections that were decided by a margin of closer than 5 percent, and 66 unique election-level clusters when focusing on state AG elections

[23] Note that Armstrong and Kolesár (2018) propose using optimal confidence intervals obtained from the explicit bias-variance tradeoff, for bandwidths chosen ex-ante by the researcher.

with a margin closer than 10 percent. I therefore correct the critical values used for the calculation of p-values by the estimated effective degrees of freedom of weighted least square regressions employed with bandwidths identical to the RDDs. Degree of freedom corrections have first been suggested by Bell and McCaffrey (2002); for the computational implementation of the degree of freedom correction in this paper, I draw on Imbens and Kolesar (2016), and on Pustejovsky and Tipton (2018).[24]

5.2 State AG Investigations and Fines

I argued that political connections to state AGs in the US may benefit firms in that they lower the latent probability of being investigated and fined by a state AG. I attempt to measure this probability by looking at data on investigations and penalties imposed on firms by state AGs. Note, however, that my data only captures cases where companies were convicted, settled a case brought forward by a state AG, or where the state AG announced they would terminate an investigation without imposing penalties. While these cases represent a subset of all investigations, I - or for that matter the public at large - have no information about cases that were dropped by state AGs at an earlier point. Moreover, investigations are a relatively coarse metric of "latent investigation probability" as the baseline investigation probability is relatively low to begin with; within a bandwidth of 5 percent around the threshold, only 53 firms out of 662 firms in my sample were investigated by state AGs within the four years after they had contributed to either the losing or the winning candidate in a state AG race.[25]

Figure 1 graphically presents the results for an RDD results with investigations as the dependent variable. More specifically, a company investigated by a state AG during the term of the state AG (i.e. within the four years following an election) is assigned a 1, companies not investigated are assigned a 0. The dependent variable may therefore

[24]Plots and RD results throughout the paper are based on Calonico et. al's rdrobust R package (ADD CITATION).

[25]Within a bandwidth of 10 percent around the threshold, 107 out of 1381 firms were investigated by the state AG within four years after an election in which they made a contribution.

be interpreted as an investigation probability. The graph suggests that companies right above the threshold, i.e. companies that won a connection to a state AG, are somewhat less likely to become subject of a state AG investigation.[26] The RDD estimates in Table 3 provide another illustration of the fact that politically connected firms may face lower state AG investigation probabilities. The table includes specifications with year and industry fixed effects[27] as well as the covariates employees (logged), assets (logged), and market value (logged). Under the RD assumptions, the inclusion of such covariates should not affect bias but is expected to yield more precise estimates. Notice that the optimal estimation and bias correction bandwidths differ considerably depending on the specification. The third line - "RD Estimate Robust" - presents bias corrected estimates and cluster-robust standard errors where the p-values have been degree of freedom adjusted as described above. These estimates are therefore my preferred set of estimates. It can be seen that while my preferred specification in column 6 - it should yield the most precise estimate due to the inclusion of year and industry fixed effects as well as covariates - yields a statistically significant effects implying a decrease in investigation probabilities for politically connected firms by about 6 to 7 percentage points - given a baseline probability of being investigated of 8 to 9 percent for non-connected firms close to the threshold an effect of sizable magnitude -, the coefficients in columns 1, 3, and 4 fail to achieve statistical significance at conventional levels. As the point estimates for these specifications are still negative, I interpret the results taken together as providing suggestive evidence that political connections to state AGs may indeed lead to lowered latent investigation probabilities for firms.

Beyond the extensive margin of politically connected firms being investigated by state AGs at lower rates, connected companies may also benefit from paying lower penalties in

[26]For presentation purposes, the observations above and below the threshold were divided into five bins respectively for which means and confidence intervals were plotted.

[27]Since the electoral margin serves as a running variable, and since a considerable number of states only appear once in the sample close to the threshold, I cannot include state or election fixed effects due to collinearity issues.

the event of investigations. Note that the number of companies in my sample that were located around the threshold and settled on a penalty or were convicted is very small. 36 companies within the 5 percent bandwidth, and 77 companies within the 10 percent bandwidth in my sample ended up paying a penalty. Table 4 shows RDD results with penalties (logged) as dependent variable.[28][29] It appears to be the case that politically connected firms pay two to three times lower fines than non-connected firms. However, notice that these results are based on a small number of companies.

Overall, this subsection provides suggestive evidence that firms may benefit from political connections to state AGs as they are investigated at lower rates than non-connected firms, and pay lower fines in the case that they are convicted or settle.

5.3 Investment

To complement the investigation and penalty results, I now turn to the effects of firms' political connections to state AGs on firms' net investment. I argued that through the above presented channels, firms' political connections to state AGs may contribute to lowering uncertainty in the business environment for companies. Building on finance literature that suggests that lowered uncertainty should entail greater firm investment, I conjecture that firms' political connections to state AGs should lead to increased firm investment. Examining firm investment as an outcome allows me to study a larger sample as such firm financial information is available for most publicly listed companies. However, it comes at the price of capturing a more indirect form of benefit than the above discussed state AG investigations and penalties.

Figure 3 shows a plot of firm-level net investment rates (one year after a state AG election) as a function of the electoral margin of the connected candidate for state AG; it suggests visually that firms that gain a connection to a state AG increase their investment

[28] See Figure 2 for a visual representation of the results.

[29] In appendix section §A Table 4, I furthermore show results that take the logged penalties (+1) for the full sample as the dependent variable. While the signs of the coefficients are negative in all specifications, most of them fail to achieve statistical significance at conventional levels.

rates more in the subsequent year than firms that did not gain a connection to a state AG. Under the RD identification assumption discussed above, these result have a causal interpretation. Table 5 presents the RD results in more detail. As above, my preferred specifications are the ones in the third line that most closely follow the suggestions by Calonico, Cattaneo, and Titiunik (2014), with bias-corrected estimates and cluster-robust standard errors (on the election-level; p-values degree of freedom adjusted). As with the state AG investigation results, I present specifications that include year and industry fixed effects as well as the covariates employees (logged), assets (logged), and market value (logged).[30] Reassuringly, all specifications yield relatively similar estimates, indicating that firms that gain a political connection to a state AG increase their net investment rate by about 4 to 7 percentage points more than firms that did not gain an election to a state AG; the effects are statistically significant in all presented specifications. Given that the baseline net investment rate in my sample around the threshold lies at about 8 percent, this amounts to an increase of the net investment rate due to a political connection to a state AG by more than 50 percent.

5.4 RD Robustness and Sensitivity

5.4.1 McCrary Test for Sorting at the Threshold

In the appendix, I present tables and figures that support the plausibility of the RDD identification assumptions. First, the McCrary test (McCrary (2008)) in section §BFigure 4 indicates that there is no sorting at the threshold. This finding alleviates concerns that firms' campaign contributions affect the outcomes of state AG elections significantly. In fact, this is unlikely for two reasons: one, most states impose rather stringent limits on the amounts of campaign contributions to be given to state AG candidates by companies or PACs, making it rather unlikely that their contributions sway the electoral fortunes of a candidate. And two, donations by companies and/or their PACs are only a small

[30] As discussed above, the inclusion of covariates in an RDD is done in order to improve precision.

portions of total donations received by candidates; in fact, most candidates receive more contributions by individuals, trade associations, unions, and parties than by companies and their PACs, once again suggesting that one should not expect a direct effect of these contributions on the outcomes of state AG elections.

5.4.2 Placebo Checks

In the appendix in tables section §ATable 5, section §ATable 6, and section §ATable 8 I present placebo checks with the lagged outcome variables for the state AG investigations and penalties results as well as for the firm investment results. While it is reassuring that the placebo state AG investigation results in section §ATable 5 and the placebo investment results in section §ATable 8 yield insignificant results in all specifications, it appears to be the case that among the subset of firms that were convicted or reached a settlement, firms that went on to win a political connection to a state AG paid higher fines within the four years before the election. This fact and the low number of observations based on which the results in Table 4 are obtained therefore suggest that the penalty results at the intensive margin should be viewed as suggestive evidence at best.

Furthermore, section §ATable 10, section §ATable 11, section §ATable 12, and section §ATable 13 show RD results for firm-level placebo outcomes that I do not expect to be affected by political connections to state AGs, namely short-term labor growth, revenue growth, sales growth, and debt growth. Indeed, none of these firm financial variables appear to display a growth rate that resembles the net investment rate, i.e. the capital growth, analyzed in the results section, which should increase the confidence in the validity of the RD research design.

5.4.3 Concurrent Events

Lastly, one may be concerned that the RD estimates pick up the effect of concurrent events such as connections to governors that companies win in the same election. Naturally, this concern is stronger for the investment results than the investigation and penalty results as the latter clearly fall in the domain of the state AG, such that one would not expect connections to governors to interfere much with those effects. For the investment results, however, if companies donate to candidates of the same party, and the prevalence of ticket splitting is low, then the outlined concern would be particularly valid. In that case, one should only observe an effect of connections to state AGs if the governor is of the same party as the state AG. However, in section §ATable 14 and section §ATable 15, I show that the coefficients of connections to state AGs on investigation probabilities and firm investments, respectively, are not statistically significantly different for the cases where state AGs share the party of the governor and where they do not, thus dispelling concerns that the treatment effects pick up the effects of concurrent events such as firms gaining connections to governors.

5.4.4 Randomization Inference

As elaborated upon above, firms in my sample are clustered on the election level. In this robustness check, I treat election clusters as my unit of analysis and employ randomization inference (as suggested by Cattaneo et al. (2015)), where the difference in means of investigation probabilities and net investment rates, respectively, within each election cluster (e.g.the difference between the mean net investment rate of firms gaining a political connection to a state AG and the mean net investment rates of firms that did not gain a political connection to a state AG in a given election; analogously for investigation probabilities) is taken as the one observed effect out of two potential effects that could have been observed for each cluster if the treatment assignment, i.e. winning or not winning a political connection to a state AG, had no effect on investigation probabilities or net investment rates. There are therefore 2^n potential treatment effects that could have been observed under the sharp null, where n is the number of election clusters. For computational reasons, I calculate the distribution of potential treatment effects for the

22 elections in my sample that were decided by a margin of less than 3 percent.[31] section §BFigure 1 and section §BFigure 2 present the mean investigation probabilities and the mean net investment rates for firms connected to the winning and the losing candidate respectively for each of these elections. The section §BFigure 7 and section §BFigure 8 present the distribution of simulated potential treatment effects under the sharp null, where election clusters with closer margins were weighted more heavily using a triangular kernel. The dashed green line in both these figures represents the actually observed treatment effects in my data, while the dashed red line represents the 5th and the 95th percentile, respectively. While the randomization inference exercise yields a positive and significant investment result under the sharp null, the results for investigation probability appear to be null. Hence, I interpret the investigation results discussed in the results section as suggestive.

5.4.5 Bandwidth Sensitivity

While I used the CERRD bandwidth selection procedure suggested by Calonico, Cattaneo, and Farrell (2020) rather than present results for an arbitrary bandwidth choice, it is nonetheless informative to investigate how sensitive the presented results are to different bandwidth choices.section §BFigure 5 and section §BFigure 6 provide sensitivity checks for different bandwidth choices for the investigation probability RDD and the investment RDD. Reassuringly, it can be seen that the results are robust to a wide array of bandwidth choices.

[31]Increasing the margin would make this exercise computationally very demanding.

6 Heterogeneous Effects and Tentative Welfare Implications

6.1 Institutional and Political Moderators

Having shown that political connections to state AGs can lead to private benefits for companies, I now explore whether institutional and political features may moderate the effects firm-level political connections to state AGs have on investigation probabilities and net investment rates. More specifically, I elaborate on accountability, partisanship of the state AG, and partisanship of legislature and governor respectively. Towards this end, I present heterogeneous treatment effects with regards to these variables, calculated following Klašnja and Titiunik (2017).[32]

6.1.1 Accountability

Elected state AGs naturally are accountable first and foremost to the voter. However, state AGs may be impeached by the legislature (i.e. the House and the Senate) for grave misdemeanors (see Myers and Ross (2007)). The legislature therefore has a latent threat that should keep the state AG somewhat accountable to the legislature. Assuming that legislature majorities that share the party of the state AG are less likely to impeach a state AG, and thus to keep her/him accountable, I hence examine heterogeneous effects of firm-level connections to state AGs on investigation probabilities and net investment rates by whether the majority party in the legislature shared the state AG's party. I expect that firm-level benefits of political connections to state AGs should increase when the state AG is held accountable to a lesser extent, i.e. when the majority in the legislature is of the same party as the state AG. In order to mitigate post-treatment bias, I focus on the subset of firms that had donated to state AGs in elections before and after which the

[32]Note that there appear to be no statistically significant heterogeneous effects on investigation probabilities or investment by firm size, industry concentration, or contribution size, see section §ATable 20, section §ATable 21, and section §ATable 22.

majority party in the state legislature did not change. The results in the first two columns of section §ATable 16 and section §ATable 18 - while not statistically significant - suggest that firms that gained a political connection to a state AG that is less accountable to the legislature did in fact might benefit from a greater reduction in investigation probabilities and a greater investment rate increase. [33]

6.1.2 State AG Partisanship

Next, I investigate heterogeneous effects of political connections on firm-level investigation probabilities and net investment by the party of the state elected state AG. Generally, Republicans are perceived as more business-friendly, which is why one might expect political connections to Republican state AGs to be more valuable for companies; if a Republican state AG is less likely to initiate investigations into a company that had contributed to his/her campaign than a Democratic state AG, then one should expect the treatment effect to be of greater magnitudes for firms that gained a connection to a Republican state AG. Column 3 and Column 4 in section §ATable 16 and section §ATable 18 however suggest that the firm-level differences with respect to investigation probabilities and the net investment rate are not statistically significant between connections to Republican and Democratic state AGs; moreover, the differences are substantively very small. These results resonate with Thompson (2020) who shows that sheriffs', i.e. local law enforcement officials', partisanship does not appear to affect policy outcomes much.

6.1.3 Legislature's and Governor's Partisanship

While the partisanship of state AGs does not appear to have a differential effects, one might still hypothesize that the partisanship of lawmakers and governors could matter. Specifically, if Republican legislatures and governors are implementing more economically

[33]Note that this operationalization of state AG accountability is coarse; interacting the effects with whether a state AG was term limited or not would arguably have been preferable, however there were only 3 close elections after which the state AG found himself/herself in her last term. That is most likely due to the fact that the majority of states do not impose term limits on state AGs to begin with.

conservative policies, then one could hypothesize that a political connection to a state AG should be more valuable whenever the majority in the legislature and/or the governor are Democratic. Political connections to the state AG then may lead to weaker enforcement stringency whenever regulations are violated, thus making connections to a state AG a substitute to a Republican-controlled legislature, or a Republican governor. The results in Column 1 and Column 2 of Table 15section §A and section §ATable 17 provide suggestive support this argument: political connections to the state AG may be more valuable whenever the majority in the legislature is controlled by the Democratic party. Firms that win political connections to state AGs are investigated at lower rates and invest more whenever Democrats hold the majority in the legislature, compared with companies that win political connections to state AGs whenever Republicans hold the majority in the state legislature. Here again, I have subset the data to firms that had contributed to state AG elections, where the majority in the legislature did not change at the time of the state AG election, thus ameliorating concerns about post-treatment bias. Column 3 and Column 4 of Table 15section §A and section §ATable 17 suggest that analogous effects do not appear to hold for governor partisanship.

6.2 Tentative Welfare Implications

I show that firms increase their short-term net investments in response to reduced firm-level uncertainty due to a political connection to a state AG, where the uncertainty reduction in turn may be driven by lowered investigation probabilities due to political connections. In much of the finance literature, investment increases due to reduced uncertainty in the business environment are seen as potentially efficiency-enhancing. However, given limited resources such as capital in an economy, one could hypothesize that the reduction of firm-level uncertainty by means of political connections to state AGs could lead to disproportionally high investment by the "wrong" firms, which would be inefficient from an allocative point of view. I build on the observation that in optimum in a competitive market, the marginal product of capital should be equalized across firms; I argue that this implies capital growth, i.e. net investment rates, to be paralleled by productivity growth in optimum.[34] Table 6 shows RD results that indicate that firm productivity (as measured by the method proposed by Ackerberg et al. (2015)) in fact does not appear to grow after companies win a political connection to a state AG (see Figure 4 for a graphical representation). Hence, I conclude that political connections to state AGs might induce a economic inefficiency through capital misallocation.

7 Conclusion

In this paper, I explore the effects of firm-level political connections to state AGs in the US. Using regression discontinuity designs, I show that firms that gain political connections to state AGs are less likely to be investigated, pay lower penalties, and increase their net investment rates. Although anecdotal evidence on the importance of political connections to state AGs exists - as discussed in the introduction -, to my knowledge, this paper is the first that examines firm-level effects of political connections to law enforcement officials such as state AGs quantitatively. While firms' campaign contributions in the literature have often been viewed as a consumption expenditure, I argue that, at least in the case of state AGs, contributions might enable the establishment of political connections and in turn confer quid-pro-quo benefits such as lowered investigation probabilities and penalties.

[34]See section §F for a formalization.

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8 Tables

Table 1: Connection and Investigations Correlation

	Investigate	Investigate	Investigate	Investigate	Investigate	Investigate	Investigate
Intercept	0.07***	−0.01	−0.00	0.04	−0.13***	−0.10*	−0.22**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)
Connection	0.01	0.01	0.00	0.01	−0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Employment					0.02***		0.03***
					(0.00)		(0.00)
Assets					0.01*		0.02***
					(0.00)		(0.00)
Market Value					0.01		−0.01
					(0.00)		(0.01)
Industry FEs	NO	NO	YES	NO	NO	YES	YES
Year FEs	NO	YES	NO	NO	NO	YES	YES
State FEs	NO	NO	NO	YES	NO	YES	YES
R^2	0.00	0.01	0.05	0.01	0.06	0.08	0.12
Adj. R^2	−0.00	0.00	0.05	0.00	0.05	0.06	0.10
Num. obs.	4440	4440	4440	4440	4126	4440	4126

Dependent variable is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t+2, t+3, or t+4, and 0 otherwise. Cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 2: Connection and Investment Correlation

	Invest	Invest	Invest	Invest	Invest	Invest	Invest
Intercept	0.07^{***}	0.03	0.06^{***}	0.06^{*}	0.07^{***}	0.00	-0.04
	(0.01)	(0.04)	(0.02)	(0.03)	(0.02)	(0.05)	(0.05)
Connection	-0.00	0.00	-0.00	0.01	-0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Employment					-0.02^{***}		-0.02^{***}
					(0.00)		(0.00)
Assets					-0.03^{***}		-0.04^{***}
					(0.00)		(0.00)
Market Value					0.04^{***}		0.05^{***}
					(0.00)		(0.00)
Industry FEs	NO	NO	YES	NO	NO	YES	YES
Year FEs	NO	YES	NO	NO	NO	YES	YES
State FEs	NO	NO	NO	YES	NO	YES	YES
R^2	0.00	0.06	0.03	0.02	0.04	0.09	0.15
Adj. R^2	-0.00	0.05	0.02	0.01	0.04	0.07	0.13
Num. obs.	4099	4099	4099	4099	3841	4099	3841

Dependent variable is net investment rate in t+1. Included covariates (in specifications 5 and 7) are employees, assets, and market value. Cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. ***p<0.01, **p<0.05,*p<0.1.

Table 3: State AG Investigations RD

	Investigation	Investigation	Investigation	Investigation	Investigation	Investigation
RD Estimate Conventional	−0.0269	−0.0601**	−0.0295	−0.0380	−0.0589**	−0.0637***
	(0.0351)	(0.0235)	(0.0354)	(0.0364)	(0.0230)	(0.0218)
RD Estimate Bias Corrected	−0.0286	−0.0614**	−0.0320	−0.0423	−0.0624**	−0.0680***
	(0.0351)	(0.0235)	(0.0354)	(0.0364)	(0.0230)	(0.0218)
RD Estimate Robust	−0.0286	−0.0614**	−0.0320	−0.0423	−0.0624**	−0.0680**
	(0.0386)	(0.0260)	(0.0385)	(0.0390)	(0.0265)	(0.0264)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1653	592	1381	1212	807	822
Bandwidth	11.2163	3.7368	10.0023	9.5927	5.9773	6.3266
Bias Correction Bandwidth	18.2350	7.6931	16.2820	15.9263	13.6232	15.1490

Dependent variable is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t+2, t+3, or t+4, and 0 otherwise. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 4: State AG Fines RD (Intensive Margin)

	Penalties	Penalties	Penalties	Penalties	Penalties	Penalties
RD Estimate Conventional	-2.1667^{***}	-0.5370	-1.6755^{***}	-1.8779^{**}	-3.4584^{***}	-3.4434^{***}
	(0.6253)	(0.3115)	(0.1922)	(0.5815)	(0.0758)	(0.1951)
RD Estimate Bias Corrected	-2.3708^{***}	-19.0972^{***}	-1.8096^{***}	-2.0712^{***}	-3.2795^{***}	-3.3243^{***}
	(0.6253)	(0.3115)	(0.1922)	(0.5815)	(0.0758)	(0.1951)
RD Estimate Robust	-2.3708^{**}	-19.0972	-1.8096^{***}	-2.0712^{**}	-3.2795^{***}	-3.3243^{***}
	(0.7409)	(12.3727)	(0.3891)	(0.6486)	(0.2373)	(0.2416)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	34	19	34	31	33	28
Bandwidth	4.6047	1.1512	4.4095	3.7241	3.5199	3.1778
Bias Correction Bandwidth	8.1629	3.0468	8.1245	7.0431	6.5200	5.1868

Dependent variable is the penalty amount (logged) imposed on the firm by a state AG in period t+1, t+2, t+3, or t+4, for the subset of companies that were convicted or settled a case in that period. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***p<0.01, **p<0.05,*p<0.1.

Table 5: Net Investment RD Table

	Investment	Investment	Investment	Investment	Investment	Investment
RD Estimate Conventional	0.0473*	0.0502***	0.0504**	0.0492**	0.0684***	0.0511***
	(0.0239)	(0.0126)	(0.0238)	(0.0238)	(0.0128)	(0.0122)
RD Estimate Bias Corrected	0.0491**	0.0523***	0.0522**	0.0500**	0.0706***	0.0541***
	(0.0239)	(0.0126)	(0.0238)	(0.0238)	(0.0128)	(0.0122)
RD Estimate Robust	0.0491*	0.0523***	0.0522**	0.0500*	0.0706***	0.0541***
	(0.0251)	(0.0142)	(0.0248)	(0.0252)	(0.0142)	(0.0130)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1064	487	1057	1069	608	694
Bandwidth	8.5887	3.4536	8.3613	9.1809	5.0767	5.9742
Bias Correction Bandwidth	14.6248	7.0052	14.3522	15.5099	11.4014	13.2297

Dependent variable is net investment rate in t+1. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 6: Productivity Growth RD

	Prod	Prod	Prod	Prod	Prod	Prod
RD Estimate Conventional	0.0012	-0.0120^{**}	0.0003	0.0008	-0.0076	-0.0090^{*}
	(0.0071)	(0.0045)	(0.0076)	(0.0072)	(0.0051)	(0.0051)
RD Estimate Bias Corrected	0.0009	-0.0113^{**}	0.0004	0.0005	-0.0074	-0.0091^{*}
	(0.0071)	(0.0045)	(0.0076)	(0.0072)	(0.0051)	(0.0051)
RD Estimate Robust	0.0009	-0.0113^{**}	0.0004	0.0005	-0.0074	-0.0091
	(0.0077)	(0.0047)	(0.0083)	(0.0079)	(0.0054)	(0.0055)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	930	508	874	832	783	744
Bandwidth	9.2664	4.9993	8.6084	9.0989	6.9517	6.8770
Bias Correction Bandwidth	16.0513	9.5274	14.8902	16.0822	13.5393	13.5480

Dependent variable is productivity growth (measured by Ackerberg et al. (2015)'s method) in t+1. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***p<0.01, **p<0.05,*p<0.1.

9 Figures

Figure 1: Investigations RD Plot

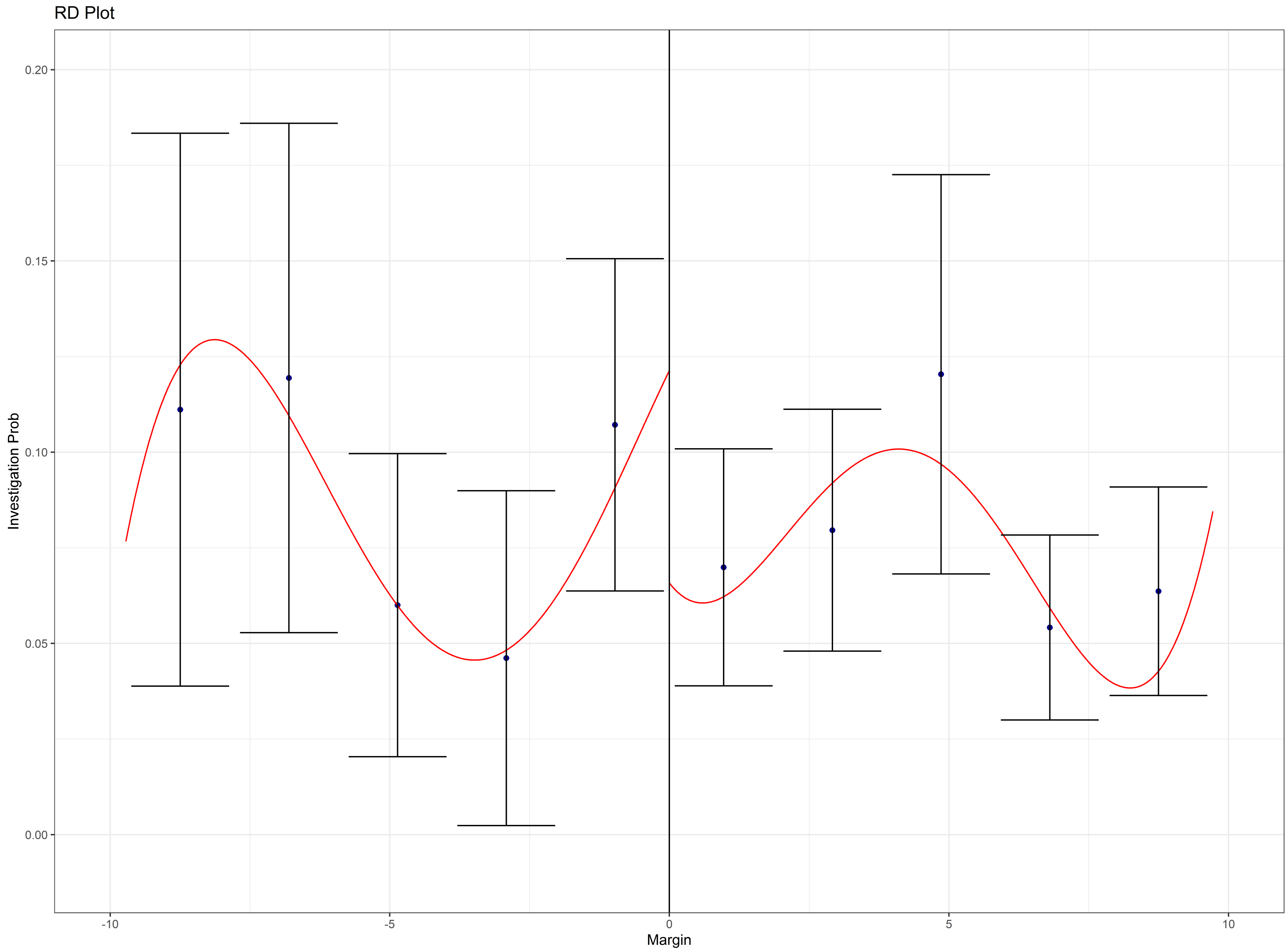


Figure shows firms' state AG investigation probabilities in years t+1, t+2, t+3, and t+4 as a function of the electoral margin of a politically connected candidate for state AG.

Figure 2: Penalties (Intensive Margin) RD Plot

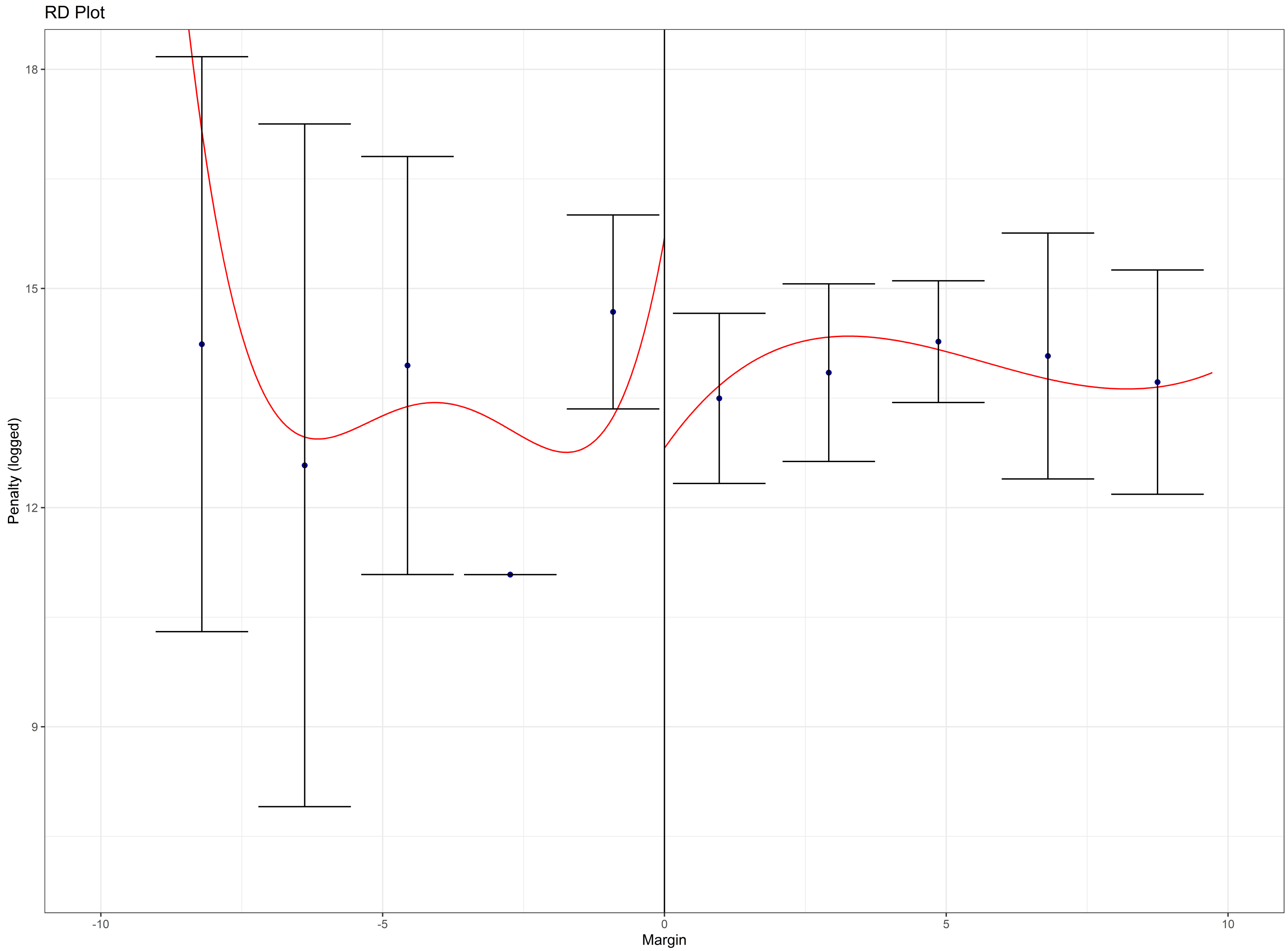


Figure shows firms' state AG penalties (logged) in years t+1, t+2, t+3, and t+4 as a function of the electoral margin of a politically connected candidate for state AG. Plot for the subset of firms that were convicted or settled during that four-year period.

Figure 3: Net Investment RD Plot

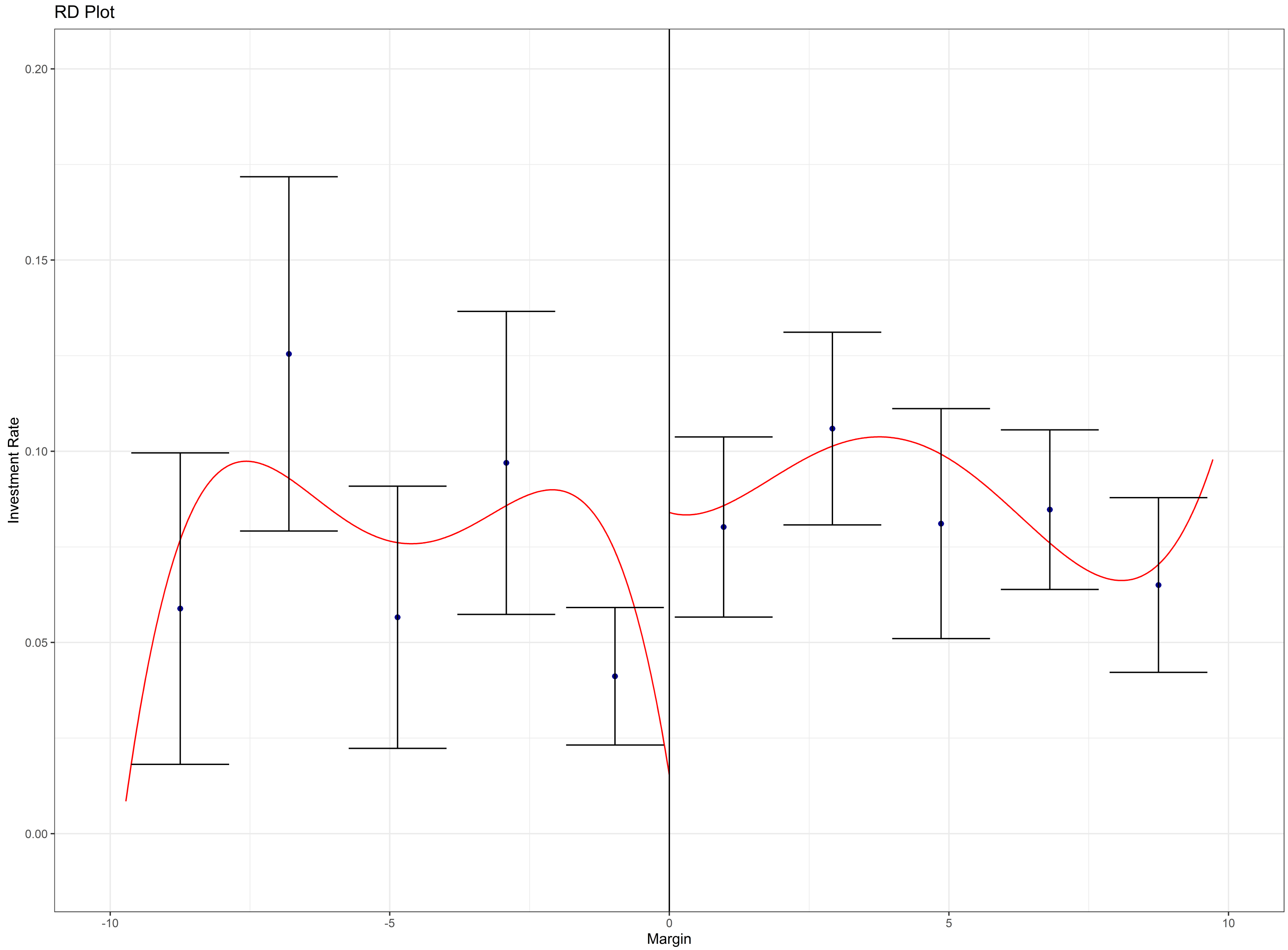


Figure shows firms' net investment rate in t+1 as a function of the electoral margin of a politically connected candidate for state AG.

Figure 4: Productivity Growth RD Plot

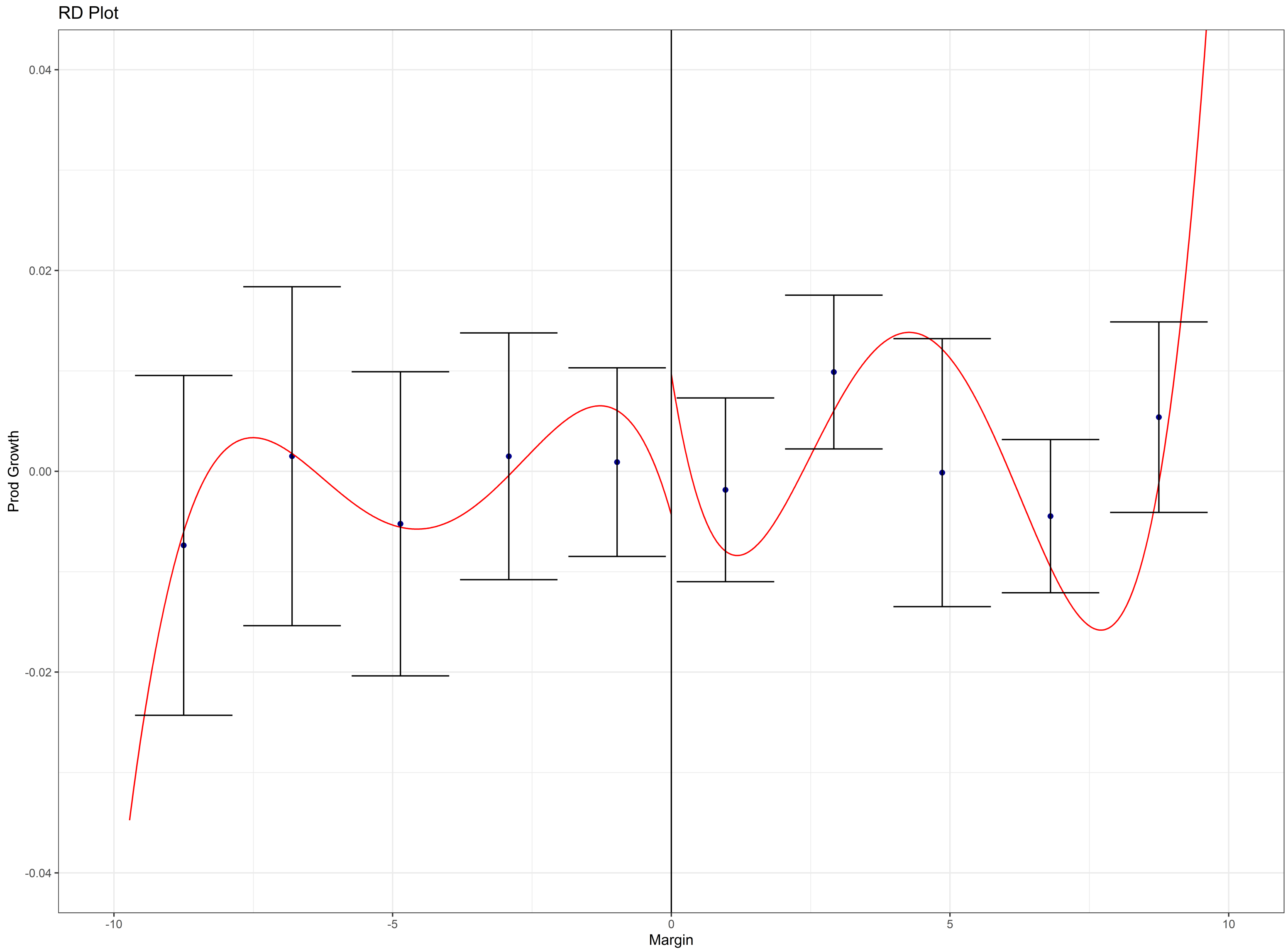


Figure shows firms' productivity growth (measured by Ackerberg et al. (2015)'s method) in t+1 as a function of the electoral margin of a politically connected candidate for state AG.

A Appendix Tables

A.1 Summary Statistics

Table 1: Summary Statistics I

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Margin	4,440	17.070	23.576	−49.830	3.670	22.290	100.000
Prop. Connection	4,440	0.837	0.369	0	1	1	1
Amount	4,440	3,004.328	5,583.865	0	500	3,000	100,500
Prop Investigated	4,440	0.078	0.269	0	0	0	1
Penalty (log)	4,440	0.800	3.287	0	0	0	20
Net Investment Rate	4,099	0.073	0.185	−0.251	−0.024	0.122	0.726
Employment (log)	4,328	3.326	1.584	0.000	2.197	4.527	7.741
Assets (log)	4,416	9.844	2.242	0.068	8.536	11.386	14.659
Market Value (log)	4,159	9.468	2.263	0.138	8.039	11.195	13.567
Sales (log)	4,415	9.260	2.009	0.000	8.056	10.779	13.055
Revenue (log)	4,415	9.185	2.036	0.000	7.996	10.698	13.146
Productivity Growth	3,431	0.003	0.068	−0.217	−0.014	0.021	0.210

Margin stands for the electoral margin with which a connected candidate won or lost the race for state AG. Prop. Connection captures the proportion of firms that were connected to a state AG. Amount is the amount donated by the firm to the state AG candidate. The investigation probability is a dummy of whether a firm was investigated by a state AG in the four years after an election, and fine is the size of the penalty that convicted companies had to pay. Net investment rate is the growth rate of PPENT (net property, plants, and equipment) in Compustat, assets are AT (assets total) in Compustat, employment is EMP in Compustat, market value is share prices times shares (PRCC*CSHO) in Compustat, Sales is SALE in Compustat, revenue is REVT (revenues total) in Compustat, productivity growth is the growth rate of TFP (following Ackerberg et al. 2015).

Table 2: Summary Statistics II

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Margin	662	0.856	2.274	−4.810	−0.150	2.600	4.810
Prop. Connection	662	0.662	0.474	0	0	1	1
Amount	662	2,997.486	4,572.528	1	750	3,000	50,000
Prop Investigated	662	0.080	0.272	0	0	0	1
Penalty (log)	662	0.762	3.223	0	0	0	19
Net Investment Rate	605	0.081	0.179	−0.251	−0.009	0.125	0.726
Employment (log)	649	3.344	1.496	0.000	2.313	4.416	7.697
Assets (log)	657	9.890	2.154	0.068	8.650	11.311	14.659
Market Value (log)	621	9.432	2.192	0.238	7.988	11.092	13.567
Sales (log)	657	9.267	1.948	0.011	8.136	10.649	13.053
Revenue (log)	657	9.209	1.972	0.012	8.095	10.641	13.146
Productivity Growth	508	0.002	0.059	−0.217	−0.015	0.022	0.210

Summary statistics narrowed to 5 percent bandwidth. Margin stands for the electoral margin with which a connected candidate won or lost the race for state AG. Prop. Connection captures the proportion of firms that were connected to a state AG. Amount is the amount donated by the firm to the state AG candidate. The investigation probability is a dummy of whether a firm was investigated by a state AG in the four years after an election, and fine is the size of the penalty that convicted companies had to pay. Net investment rate is the growth rate of PPENT (net property, plants, and equipment) in Compustat, assets are AT (assets total) in Compustat, employment is EMP in Compustat, market value is share prices times shares (PRCC*CSHO) in Compustat, Sales is SALE in Compustat, revenue is REVT (revenues total) in Compustat, productivity growth is the growth rate of TFP (following Ackerberg et al. 2015).

Table 3: Summary Statistics by Industry

	Industry	N Won	N Lost	Invest Won	Invest Lost	Investigate Won	Investigate Lost
1	Agriculture	14.00	5.00	0.05	0.27	0.00	0.00
2	Mining,Oil,Utilities	134.00	68.00	0.06	0.10	0.04	0.04
3	Manufacturing	263.00	124.00	0.05	0.05	0.08	0.08
4	Trade	97.00	52.00	0.09	0.07	0.08	0.10
5	Finance,Insurance	381.00	151.00	0.11	0.07	0.08	0.13
6	Education,Health	26.00	11.00	0.18	0.10	0.04	0.09
7	Arts,Entertainment	25.00	9.00	0.12	0.03	0.00	0.00
8	Other Services	5.00	2.00	0.04	-0.01	0.00	0.00
9	Administration	10.00	4.00	-0.04	-0.12	0.10	0.00

Summary table shows number of firms that had donated to the winning and losing candidates in state AG races respectively, as well as mean investment rate outcomes and mean investigation outcomes for both these groups of firms by industry.

A.2 Additional Results

Table 4: State AG Fines (All Firms)

	Penalties	Penalties	Penalties	Penalties	Penalties	Penalties
RD Estimate Conventional	−0.2306	−0.6152**	−0.3137	−0.3265	−0.4127	−0.4596
	(0.6157)	(0.2707)	(0.5946)	(0.6643)	(0.2750)	(0.2691)
RD Estimate Bias Corrected	−0.2607	−0.4651	−0.3646	−0.3944	−0.4452	−0.5714**
	(0.6157)	(0.2707)	(0.5946)	(0.6643)	(0.2750)	(0.2691)
RD Estimate Robust	−0.2607	−0.4651	−0.3646	−0.3944	−0.4452	−0.5714*
	(0.6722)	(0.4128)	(0.6422)	(0.7109)	(0.3213)	(0.2915)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1612	488	1381	1395	843	787
Bandwidth	11.0066	3.0548	9.8436	10.5321	6.1463	6.0924
Bias Correction Bandwidth	16.9030	5.4577	15.2304	16.1999	13.4986	12.2276

Dependent variable is the penalty amount +1 (logged) imposed on the firm by a state AG in period t, t-1, t-2, or t-3. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

A.3 Placebo Checks

Table 5: State AG Investigations RD Placebo

	InvestigPlac	InvestigPlac	InvestigPlac	InvestigPlac	InvestigPlac	InvestigPlac
RD Estimate Conventional	−0.0280	0.0070	−0.0333	−0.0303	0.0001	0.0048
	(0.0480)	(0.0203)	(0.0473)	(0.0453)	(0.0207)	(0.0194)
RD Estimate Bias Corrected	−0.0293	0.0055	−0.0348	−0.0311	−0.0053	0.0004
	(0.0480)	(0.0203)	(0.0473)	(0.0453)	(0.0207)	(0.0194)
RD Estimate Robust	−0.0293	0.0055	−0.0348	−0.0311	−0.0053	0.0004
	(0.0519)	(0.0224)	(0.0512)	(0.0489)	(0.0209)	(0.0197)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1496	592	1338	1395	807	826
Bandwidth	10.3844	3.6912	9.7167	10.5560	5.9998	6.3842
Bias Correction Bandwidth	15.5047	6.4070	14.3223	15.8398	9.9561	10.6155

Dependent variable is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t, t-1, t-2, or t-3, and 0 otherwise. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 6: State AG Fines RD Placebo (Intensive Margin)

	PenaltiesPlac	PenaltiesPlac	PenaltiesPlac	PenaltiesPlac	PenaltiesPlac	PenaltiesPlac
RD Estimate Conventional	2.0407	11.8903***	8.7619***	3.1064	3.4336**	20.1563***
	(2.6836)	(0.4771)	(1.7106)	(3.5546)	(1.1716)	(0.0729)
RD Estimate Bias Corrected	2.5572	12.1097***	9.4730***	3.2857	4.2414**	20.3957***
	(2.6836)	(0.4771)	(1.7106)	(3.5546)	(1.1716)	(0.0729)
RD Estimate Robust	2.5572	12.1097***	9.4730***	3.2857	4.2414**	20.3957***
	(2.8139)	(1.0718)	(1.7794)	(3.7214)	(1.2685)	(0.2451)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	51	29	33	32	25	31
Bandwidth	6.2302	2.9013	3.3684	4.2506	2.1982	3.8109
Bias Correction Bandwidth	10.9218	5.3202	6.9939	8.2151	4.6971	6.3368

Dependent variable is the penalty amount (logged) imposed on the firm by a state AG in period t, t-1, t-2, or t-3, for the subset of companies that were convicted or settled a case in that period. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***p<0.01, **p<0.05,*p<0.1.

Table 7: State AG Fines RD Placebo (All Firms)

	PenaltiesPlac	PenaltiesPlac	PenaltiesPlac	PenaltiesPlac	PenaltiesPlac	PenaltiesPlac
RD Estimate Conventional	−0.3922	−0.2415	−0.4940	−0.4357	−0.2368	−0.2247
	(0.4832)	(0.2806)	(0.4707)	(0.4563)	(0.2615)	(0.2483)
RD Estimate Bias Corrected	−0.4231	−0.2589	−0.5249	−0.4634	−0.3009	−0.2866
	(0.4832)	(0.2806)	(0.4707)	(0.4563)	(0.2615)	(0.2483)
RD Estimate Robust	−0.4231	−0.2589	−0.5249	−0.4634	−0.3009	−0.2866
	(0.5208)	(0.3107)	(0.5074)	(0.4896)	(0.2726)	(0.2593)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1381	592	1256	1344	843	787
Bandwidth	9.9533	3.9802	9.2902	10.0906	6.0897	6.1183
Bias Correction Bandwidth	15.7073	6.6663	14.4861	16.0512	10.0295	10.7493

Dependent variable is the penalty amount +1 (logged) imposed on the firm by a state AG in period t, t-1, t-2, or t-3. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 8: Net Investment RD Table Placebo

	InvestmentPlac	InvestmentPlac	InvestmentPlac	InvestmentPlac	InvestmentPlac	InvestmentPlac
RD Estimate Conventional	0.0047	−0.0136	0.0065	0.0056	−0.0123	−0.0126
	(0.0167)	(0.0113)	(0.0182)	(0.0169)	(0.0125)	(0.0123)
RD Estimate Bias Corrected	0.0040	−0.0151	0.0063	0.0049	−0.0131	−0.0121
	(0.0167)	(0.0113)	(0.0182)	(0.0169)	(0.0125)	(0.0123)
RD Estimate Robust	0.0040	−0.0151	0.0063	0.0049	−0.0131	−0.0121
	(0.0181)	(0.0122)	(0.0197)	(0.0184)	(0.0136)	(0.0133)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1040	562	1040	978	1107	1513
Bandwidth	7.5316	4.1798	7.4804	7.6242	8.8783	11.4655
Bias Correction Bandwidth	13.4020	8.9073	12.8352	13.4531	16.7073	22.9366

Dependent variable is net investment rate in t. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 9: Productivity Growth RD Placebo

	ProdPlac	ProdPlac	PenaltiesPlac	ProdPlac	ProdPlac	ProdPlac
RD Estimate Conventional	0.0063	0.0179***	0.0069	0.0074	0.0182***	0.0202***
	(0.0075)	(0.0064)	(0.0074)	(0.0077)	(0.0060)	(0.0064)
RD Estimate Bias Corrected	0.0064	0.0181***	0.0069	0.0083	0.0190***	0.0214***
	(0.0075)	(0.0064)	(0.0074)	(0.0077)	(0.0060)	(0.0064)
RD Estimate Robust	0.0064	0.0181**	0.0069	0.0083	0.0190***	0.0214***
	(0.0082)	(0.0076)	(0.0080)	(0.0086)	(0.0065)	(0.0068)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1309	846	1165	1454	898	856
Bandwidth	11.8224	7.3279	10.7321	13.8131	8.8491	8.8376
Bias Correction Bandwidth	19.4876	12.8180	18.3463	26.5294	16.6459	16.9488

Dependent variable is productivity growth (measured by Ackerberg et al. (2015)'s method) in t. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 10: Employment Growth

	Empl	Empl	Empl	Empl	Empl	Empl
RD Estimate Conventional	0.0085	0.0167*	0.0112	0.0100	0.0143**	0.0115*
	(0.0162)	(0.0076)	(0.0162)	(0.0157)	(0.0061)	(0.0059)
RD Estimate Bias Corrected	0.0087	0.0174**	0.0116	0.0097	0.0141**	0.0112*
	(0.0162)	(0.0076)	(0.0162)	(0.0157)	(0.0061)	(0.0059)
RD Estimate Robust	0.0087	0.0174*	0.0116	0.0097	0.0141**	0.0112*
	(0.0176)	(0.0081)	(0.0175)	(0.0170)	(0.0063)	(0.0062)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1387	398	1190	1230	1023	859
Bandwidth	10.5849	2.7511	9.5377	9.7330	7.4628	6.7721
Bias Correction Bandwidth	18.0447	5.1737	16.4253	16.7017	16.4542	15.1422

Dependent variable is labor growth in t+1. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***p<0.01, **p<0.05,*p<0.1.

Table 11: Revenue Growth

	Rev	Rev	Rev	Rev	Rev	Rev
RD Estimate Conventional	0.0170	0.0001	0.0168	0.0173	0.0082	0.0018
	(0.0200)	(0.0079)	(0.0198)	(0.0204)	(0.0094)	(0.0101)
RD Estimate Bias Corrected	0.0180	−0.0023	0.0184	0.0182	0.0084	0.0012
	(0.0200)	(0.0079)	(0.0198)	(0.0204)	(0.0094)	(0.0101)
RD Estimate Robust	0.0180	−0.0023	0.0184	0.0182	0.0084	0.0012
	(0.0220)	(0.0087)	(0.0218)	(0.0223)	(0.0102)	(0.0108)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1104	566	1096	1042	942	989
Bandwidth	8.7104	4.2251	8.1287	9.1019	6.8031	7.8504
Bias Correction Bandwidth	15.5799	8.2331	15.0077	16.2129	14.2987	15.2706

Dependent variable is revenue growth in t+1. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***p<0.01, **p<0.05,*p<0.1.

Table 12: Sales Growth

	Sale	Sale	Sale	Sale	Sale	Sale
RD Estimate Conventional	0.0154	−0.0000	0.0155	0.0157	0.0081	0.0018
	(0.0183)	(0.0079)	(0.0182)	(0.0184)	(0.0093)	(0.0099)
RD Estimate Bias Corrected	0.0169	0.0028	0.0174	0.0168	0.0082	0.0013
	(0.0183)	(0.0079)	(0.0182)	(0.0184)	(0.0093)	(0.0099)
RD Estimate Robust	0.0169	0.0028	0.0174	0.0168	0.0082	0.0013
	(0.0201)	(0.0128)	(0.0200)	(0.0202)	(0.0099)	(0.0107)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1096	591	1049	1042	990	989
Bandwidth	8.3173	4.5232	7.8714	8.9030	6.8746	7.7399
Bias Correction Bandwidth	14.9637	8.9182	14.5446	15.7803	14.2181	14.9122

Dependent variable is sales growth in t+1. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 13: Debt Growth

	Debt	Debt	Debt	Debt	Debt	Debt
RD Estimate Conventional	0.0253	0.0449	0.0160	0.0234	0.0327	0.0034
	(0.0628)	(0.0611)	(0.0673)	(0.0669)	(0.0657)	(0.0588)
RD Estimate Bias Corrected	0.0249	0.0523	0.0160	0.0189	0.0435	−0.0033
	(0.0628)	(0.0611)	(0.0673)	(0.0669)	(0.0657)	(0.0588)
RD Estimate Robust	0.0249	0.0523	0.0160	0.0189	0.0435	−0.0033
	(0.0676)	(0.0644)	(0.0719)	(0.0721)	(0.0690)	(0.0647)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	801	543	769	784	581	778
Bandwidth	8.3659	5.9027	7.8414	9.0462	6.1574	8.2864
Bias Correction Bandwidth	14.6286	11.0336	13.4337	16.1214	11.4883	15.5490

Dependent variable is debt growth in t+1. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***p<0.01, **p<0.05,*p<0.1.

A.4 Heterogeneous Effects

Table 14: Governor-State AG Co-Partisanship: Investigations

	Investig	Investig	Investig	Investig
Difference	0.0825	−0.0042	−0.0190	0.0049
	(0.0703)	(0.0674)	(0.0777)	(0.0749)
Coef Group 1	−0.0271	−0.0845	−0.0627	−0.0592
Coef Group 0	−0.1096	−0.0803	−0.0436	−0.0641

Dependent variable is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t-+2, t+3, or t+4, and 0 otherwise. Heterogeneous treatment effects by whether the state AG shared the party of the governor (Column 1 and Column 2 only or the subset of cases where the governor party did not change before and after the election; Column 3 and Column 4 for the whole dataset). The coefficient for group 1 is the RD coefficient for investigations of firms that won a connection to a state AG that is not a copartisan of the governor, the coefficient for group 0 is the RD coefficient for investigation of firms that won a connection to state AG that shares party of the governor. In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***p<0.01, **p<0.05, *p<0.1.

Table 15: Governor-State AG Co-Partisanship: Investment

	Investig	Investig	Investig	Investig
Difference	0.0825	−0.0042	−0.0190	0.0049
	(0.0703)	(0.0674)	(0.0777)	(0.0749)
Coef Group 1	−0.0271	−0.0845	−0.0627	−0.0592
Coef Group 0	−0.1096	−0.0803	−0.0436	−0.0641

Dependent variable is net investment rate in t+1. Heterogeneous treatment effects by whether the state AG shared the party of the governor (Column 1 and Column 2 only or the subset of cases where the governor party did not change before and after the election; Column 3 and Column 4 for the whole dataset). The coefficient for group 1 is the RD coefficient for investigations of firms that won a connection to a state AG that is not a copartisan of the governor, the coefficient for group 0 is the RD coefficient for investigation of firms that won a connection to state AG that shares party of the governor. In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***p<0.01, **p<0.05, *p<0.1.

Table 16: Accountability and Partisanship: Investigations

	Investig	Investig	Investig	Investig
Difference	0.0825	−0.0042	−0.0190	0.0049
	(0.0703)	(0.0674)	(0.0777)	(0.0749)
Coef Group 1	−0.0271	−0.0845	−0.0627	−0.0592
Coef Group 0	−0.1096	−0.0803	−0.0436	−0.0641

Dependent variable is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t-+2, t+3, or t+4, and 0 otherwise. Heterogeneous treatment effects by whether the state AG shared the party of the majority in the legislature (Column 1 and Column 2), and heterogeneous effects by whether the state AG is Republican or Democrat (Column 3 and Column 4). In Column 1 and Column 2, the coefficient for group 1 is the RD coefficient for investigations of firms that won a connection to a state AG that is not a copartisan of the majority in the legislature, the coefficient for group 0 is the RD coefficient for investigation of firms that won a connection to state AG that shares party with the legislature's majority. Column 3 presents heterogeneous treatment effects by state AG partisanship. Group 1 captures firm-level investigation effects if the state AG was Republican, group 0 if the state AG was a Democrat. In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***p<0.01, **p<0.05, *p<0.1.

Table 17: Legislature and Governor Partisanship: Investigations

	Investig	Investig	Investig	Investig
Difference	0.1688***	0.0575	0.0056	−0.0818
	(0.0522)	(0.0605)	(0.1579)	(0.1574)
Coef Group 1	0.0085	−0.0539	0.0274	−0.0281
Coef Group 0	−0.1603	−0.1114	0.0217	0.0537

Dependent variable is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t-+2, t+3, or t+4, and 0 otherwise. Heterogeneous treatment effects by whether the majority in the state legislature was Republican or Democratic (Column 1 and Column 2), and heterogeneous effects by whether the governor was Republican or Democratic (Column 3 and Column 4). In Column 1 and Column 2, the coefficient for group 1 is the RD coefficient for investigations of firms that won a connection to a state AG whenever the majority in the legislature was Republican, the coefficient for group 0 is the RD coefficient for investigations of firms that won a connection to state AG whenever the majority in the legislature was Democratic. In Column 3, the coefficient for group 1 is the RD coefficient for investigations of firms that won a connection to a state AG whenever governor was Republican, the coefficient for group 0 is the RD coefficient for investigations of firms that won a connection to state AG whenever the governor was Democratic. In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Table 18: Accountability and Partisanship: Investment

	Invest	Invest	Invest	Invest
Difference	−0.0804	−0.0580	−0.0052	0.0049
	(0.0515)	(0.0478)	(0.0577)	(0.0567)
Coef Group 1	0.0144	0.0287	0.0458	0.0483
Coef Group 0	0.0948	0.0867	0.0510	0.0434

Dependent variable is net investment rate in t+1. Heterogeneous treatment effects by whether the state AG shared the party of the majority in the legislature (Column 1 and Column 2), and heterogeneous effects by whether the state AG is Republican or Democrat (Column 3 and Column 4). In Column 1 and Column 2, the coefficient for group 1 is the RD coefficient for the net investment rate of firms that won a connection to a state AG that is not a copartisan of the majority in the legislature, the coefficient for group 0 is the RD coefficient for the net investment rate of firms that won a connection to state AG that shares party with the legislature's majority. Column 3 presents heterogeneous treatment effects by state AG partisanship. Group 1 captures firm-level net investment rate effects if the state AG was Republican, group 0 if the state AG was a Democrat. In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***p<0.01, **p<0.05, *p<0.1.

Table 19: Legislature and Governor Partisanship: Investment

	Invest	Invest	Invest	Invest
Difference	−0.0620	−0.0464	−0.0100	−0.0407
	(0.0493)	(0.0470)	(0.1044)	(0.1013)
Coef Group 1	0.0415	0.0514	0.0403	0.0188
Coef Group 0	0.1035	0.0978	0.0504	0.0595

Dependent variable is net investment rate in t+1. Heterogeneous treatment effects by whether the majority in the state legislature was Republican or Democratic (Column 1 and Column 2), and heterogeneous effects by whether the governor was Republican or Democratic (Column 3 and Column 4). In Column 1 and Column 2, the coefficient for group 1 is the RD coefficient for investigations of firms that won a connection to a state AG whenever the majority in the legislature was Republican, the coefficient for group 0 is the RD coefficient for the net investment rate of firms that won a connection to state AG whenever the majority in the legislature was Democratic. In Column 3, the coefficient for group 1 is the RD coefficient for the net investment rate of firms that won a connection to a state AG whenever governor was Republican, the coefficient for group 0 is the RD coefficient for the net investment rate of firms that won a connection to state AG whenever the governor was Democratic. In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***p<0.01, **p<0.05, *p<0.1.

Table 20: High vs Low Contributions

	Invest	Invest	Investig	Investig
Difference	−0.0116	−0.0282	0.0483	0.0267
	(0.0470)	(0.0402)	(0.0734)	(0.0658)
Coef Group 1	0.0374	0.0310	−0.0388	−0.0456
Coef Group 0	0.0490	0.0592	−0.0871	−0.0723

Dependent variable in columns 1 and 2 is net investment rate in t+1;dependent variable in columns 3 and 4 is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t-+2, t+3, or t+4, and 0 otherwise. Heterogeneous treatment effects by whether a company gave a greater or smaller than the median contribution in my sample. In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***p<0.01, **p<0.05, *p<0.1.

Table 21: Larger vs Smaller Firms

	Invest	Invest	Investig	Investig
Difference	-0.0654	-0.0482	-0.1876^{*}	0.0409
	(0.0465)	(0.0421)	(0.0995)	(0.0670)
Coef Group 1	0.0094	0.0206	-0.1743	-0.0370
Coef Group 0	0.0748	0.0688	0.0133	-0.0779

Dependent variable in columns 1 and 2 is net investment rate in t+1;dependent variable in columns 3 and 4 is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t-+2, t+3, or t+4, and 0 otherwise. Heterogeneous treatment effects by whether a company was greater or smaller than the median firm in my sample (size measured by assets). In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***p<0.01, **p<0.05, *p<0.1.

Table 22: Firms in More and Less Concentrated Industries

	Invest	Invest	Investig	Investig
Difference	0.0387	0.0390	0.0434	0.0129
	(0.0576)	(0.0447)	(0.0836)	(0.0672)
Coef Group 1	0.0659	0.0634	−0.0223	−0.0583
Coef Group 0	0.0271	0.0244	−0.0657	−0.0712

Dependent variable in columns 1 and 2 is net investment rate in t+1;dependent variable in columns 3 and 4 is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t-+2, t+3, or t+4, and 0 otherwise. Heterogeneous treatment effects by whether a company's industry was more or less concentrated than the median Herfindahl concentration in my sample. In Column 2 and Column 4, estimates below the thresholds are calculated based on all firms that were politically connected to a losing candidate for state AG. Bandwidth set to 10. Cluster-robust standard errors by election. ***p<0.01, **p<0.05, *p<0.1.

A.5 Alternative Operationalization of Connections: Non-Individual + C-Suite Contributions

Table 23: State AG Investigations RD

	Investigation	Investigation	Investigation	Investigation	Investigation	Investigation
RD Estimate Conventional	-0.0275	-0.0543^{**}	-0.0274	-0.0286	-0.0588^{**}	-0.0583^{**}
	(0.0309)	(0.0231)	(0.0317)	(0.0335)	(0.0227)	(0.0213)
RD Estimate Bias Corrected	-0.0287	-0.0516^{**}	-0.0293	-0.0312	-0.0603^{**}	-0.0619^{***}
	(0.0309)	(0.0231)	(0.0317)	(0.0335)	(0.0227)	(0.0213)
RD Estimate Robust	-0.0287	-0.0516^{*}	-0.0293	-0.0312	-0.0603^{**}	-0.0619^{**}
	(0.0340)	(0.0264)	(0.0345)	(0.0361)	(0.0258)	(0.0243)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1852	592	1523	1476	899	837
Bandwidth	11.5088	3.4704	9.9421	10.1065	5.9849	6.0032
Bias Correction Bandwidth	18.6604	6.3247	16.2327	16.7359	12.4693	13.6449

Dependent variable is binary that takes on 1 if a firm was convicted by a state AG or settled a case in period t+1, t+2, t+3, or t+4, and 0 otherwise. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 24: State AG Fines RD (Intensive Margin)

	Penalties	Penalties	Penalties	Penalties	Penalties	Penalties
RD Estimate Conventional	-2.1612^{***}	-1.1039^{***}	-1.6733^{***}	-1.8454^{**}	-4.4075^{***}	-5.8495^{***}
	(0.6224)	(0.2238)	(0.1869)	(0.5830)	(0.0951)	(0.1885)
RD Estimate Bias Corrected	-2.3659^{***}	-1.4561^{***}	-1.8340^{***}	-2.0194^{***}	-4.6818^{***}	-5.6877^{***}
	(0.6224)	(0.2238)	(0.1869)	(0.5830)	(0.0951)	(0.1885)
RD Estimate Robust	-2.3659^{**}	-1.4561^{*}	-1.8340^{***}	-2.0194^{**}	-4.6818^{***}	-5.6877^{***}
	(0.7309)	(0.5673)	(0.2732)	(0.6569)	(0.3102)	(0.2719)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	35	25	35	32	34	29
Bandwidth	4.6308	2.1707	4.3702	4.0448	3.9855	3.1448
Bias Correction Bandwidth	8.3484	3.5116	8.0937	7.4503	7.1776	5.2133

Dependent variable is the penalty amount (logged) imposed on the firm by a state AG in period t+1, t+2, t+3, or t+4, for the subset of companies that were convicted or settled a case in that period. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***$p<0.01$, **$p<0.05$,*$p<0.1$.

Table 25: Net Investment RD Table

	Investment	Investment	Investment	Investment	Investment	Investment
RD Estimate Conventional	0.0450*	0.0431**	0.0480*	0.0463*	0.0661***	0.0524**
	(0.0238)	(0.0154)	(0.0239)	(0.0254)	(0.0176)	(0.0189)
RD Estimate Bias Corrected	0.0466*	0.0550***	0.0499**	0.0470*	0.0687***	0.0559***
	(0.0238)	(0.0154)	(0.0239)	(0.0254)	(0.0176)	(0.0189)
RD Estimate Robust	0.0466*	0.0550**	0.0499*	0.0470*	0.0687***	0.0559**
	(0.0246)	(0.0218)	(0.0247)	(0.0263)	(0.0192)	(0.0211)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1158	536	1158	1099	634	652
Bandwidth	8.1477	3.4642	8.0755	9.0296	4.5218	5.1949
Bias Correction Bandwidth	14.1900	6.0553	14.0971	15.3274	10.1093	10.8197

Dependent variable is net investment rate in t+1. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***p<0.01, **p<0.05,*p<0.1.

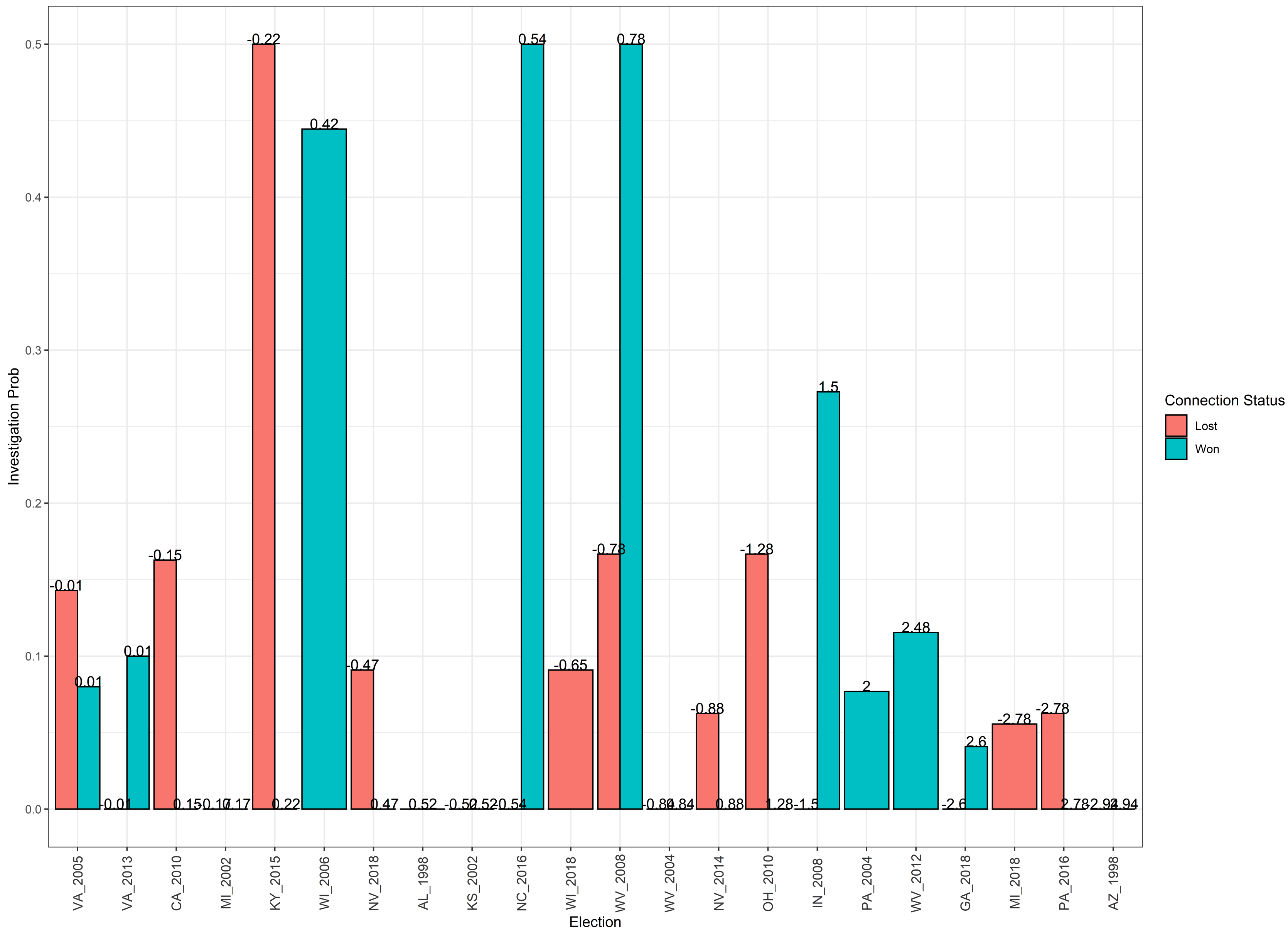
Table 26: Productivity Growth RD

	Prod	Prod	Prod	Prod	Prod	Prod
RD Estimate Conventional	0.0011	−0.0108**	−0.0002	0.0005	−0.0072	−0.0062
	(0.0060)	(0.0046)	(0.0061)	(0.0061)	(0.0046)	(0.0047)
RD Estimate Bias Corrected	0.0009	−0.0087*	0.0004	0.0001	−0.0070	−0.0057
	(0.0060)	(0.0046)	(0.0061)	(0.0061)	(0.0046)	(0.0047)
RD Estimate Robust	0.0009	−0.0087*	0.0004	0.0001	−0.0070	−0.0057
	(0.0065)	(0.0048)	(0.0066)	(0.0066)	(0.0048)	(0.0050)
Year FEs	NO	YES	NO	NO	YES	YES
Industry FEs	NO	NO	YES	NO	YES	YES
Controls	NO	NO	NO	YES	NO	YES
Num. obs.	1047	610	891	1058	725	838
Bandwidth	9.6153	5.4710	7.9112	9.9202	6.4791	7.1906
Bias Correction Bandwidth	17.6120	9.9537	14.6381	18.4645	13.5915	14.5563

Dependent variable is productivity growth (measured by Ackerberg et al. (2015)'s method) in t+1. Included covariates (in specifications 4 and 6) are employees, assets, and market value. The conventional RD estimates in the first row do not correct for bias, and do not contain cluster-robust standard errors. The bias corrected RD estimates in the second row are bias-corrected, but standard errors are not cluster-robust. The third row presents bias-corrected estimates, as well as cluster-robust standard errors (by election). P-values have been degree of freedom adjusted. Bandwidth has been chosen based on the CERRD optimal bandwidth selection procedure. ***p<0.01, **p<0.05,*p<0.1.

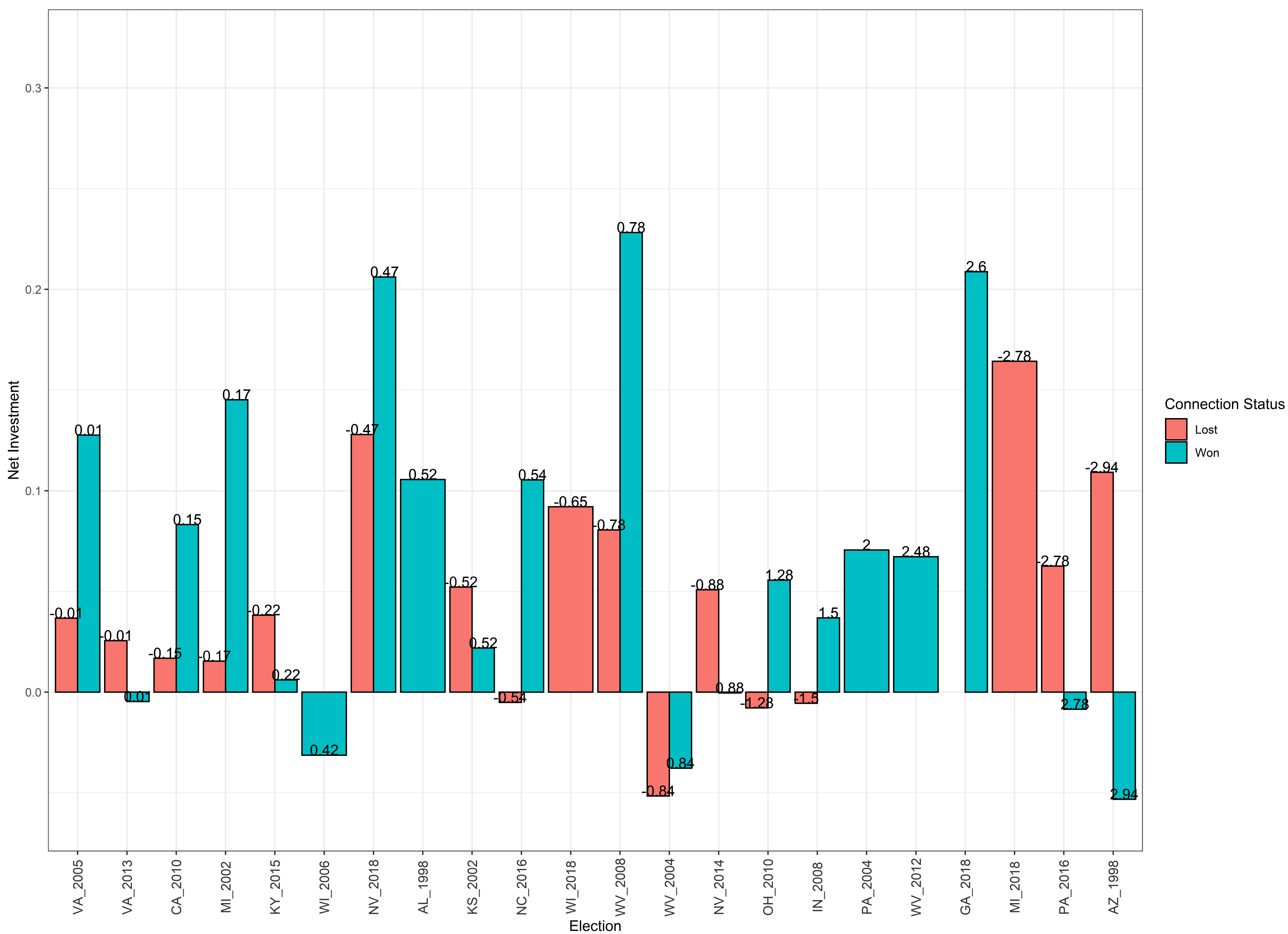
B Appendix Figures

Figure 1: Firm Investigations by Election and Connection Status



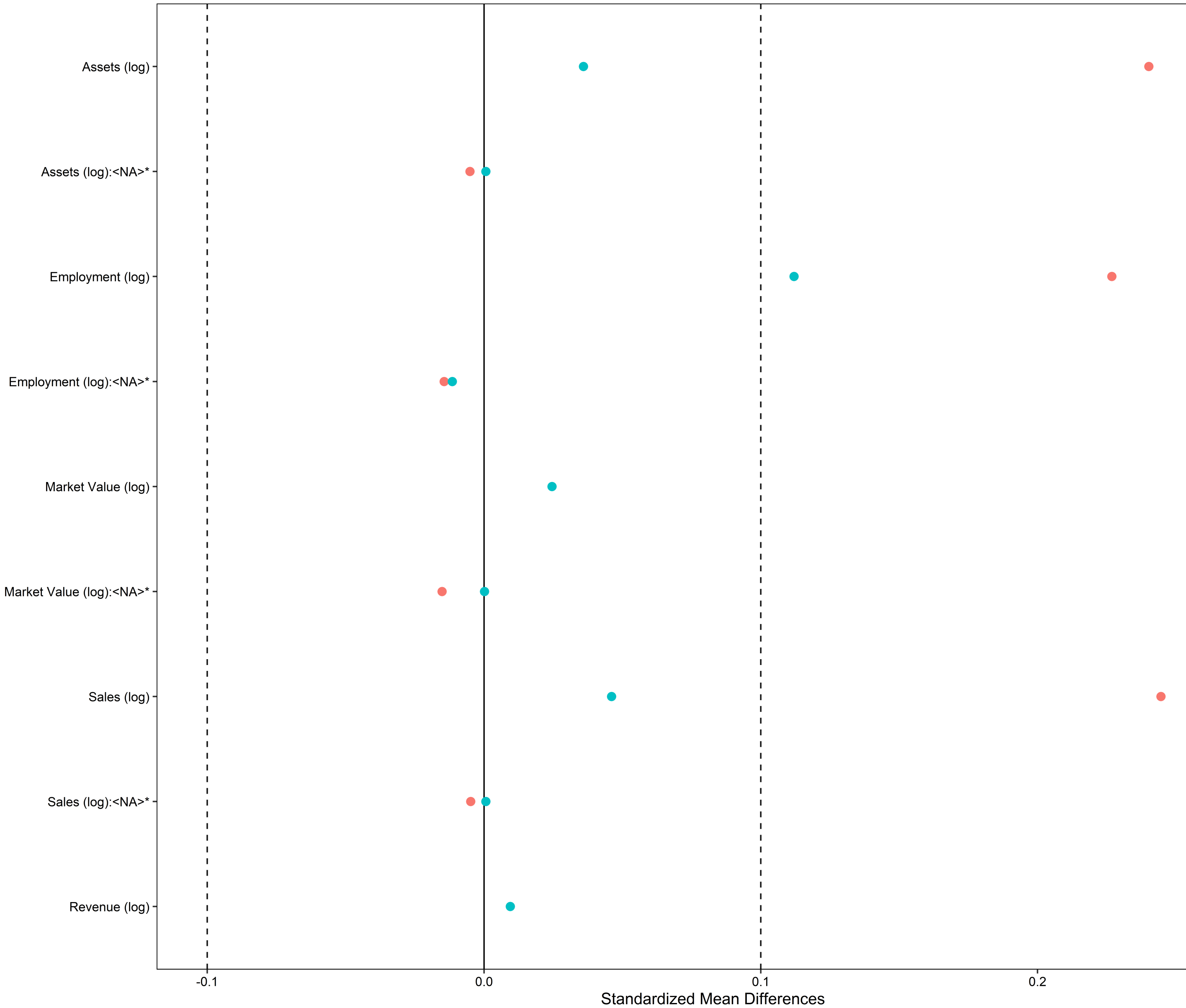
For all state AG elections that were decided by a margin of 3 percent or less, this figure shows the mean investigation rate that firms connected to the winning candidate, and firms connected to the losing candidate respectively faced.

Figure 2: Firm Investment by Election and Connection Status



For all state AG elections that were decided by a margin of 3 percent or less, this figure shows the mean net investment rates in t+1 that firms connected to the winning candidate, and firms connected to the losing candidate respectively displayed.

Figure 3: Balance Table



Standardized mean differences for selected covariates

Figure 4: McCrary Test for Sorting at the Threshold

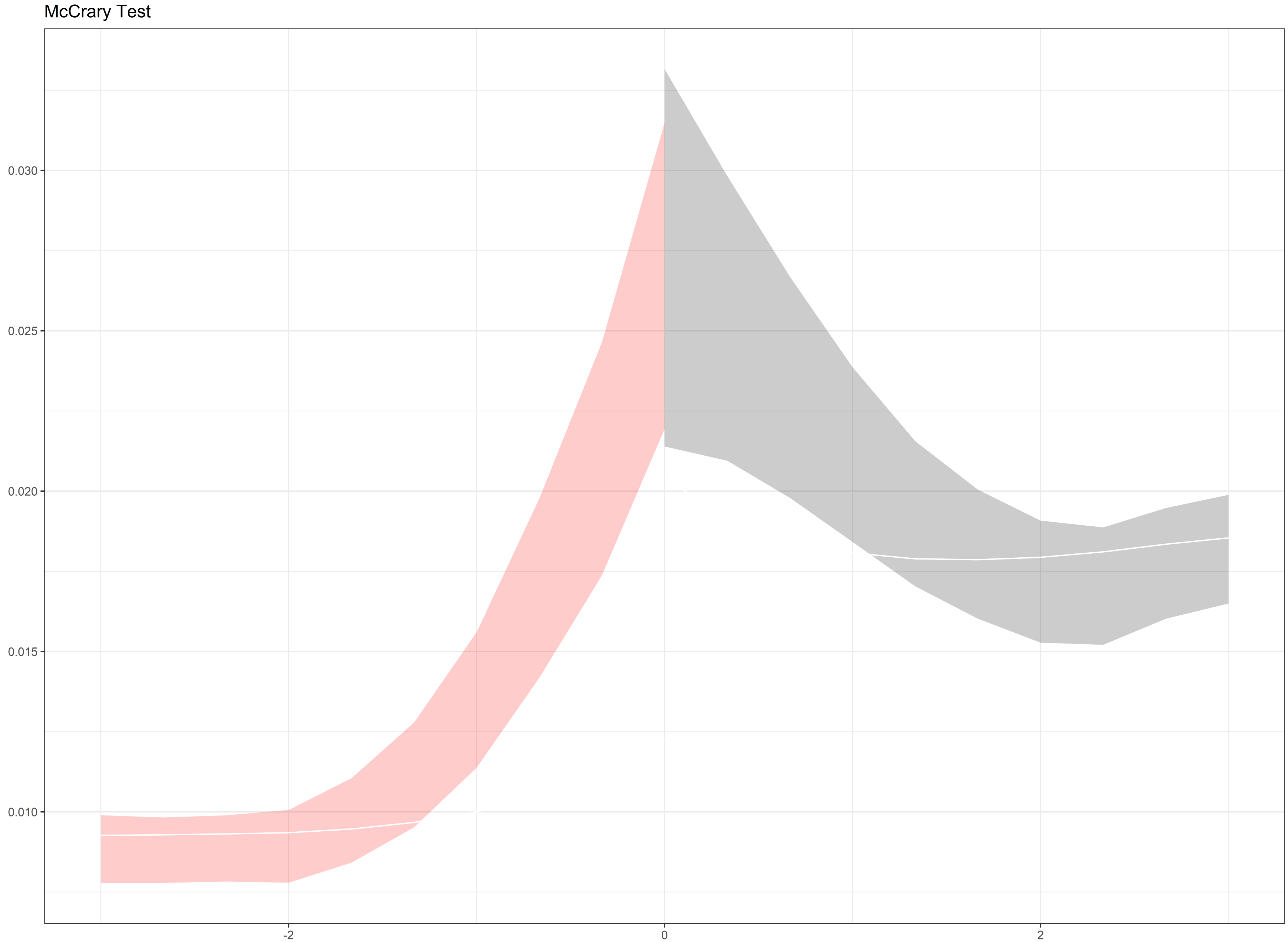


Figure shows a plot for the McCrary test for sorting at the threshold. Density of units right above and right below the threshold value is being depicted.

Figure 5: Investigations Bandwidth Sensitivity Analysis

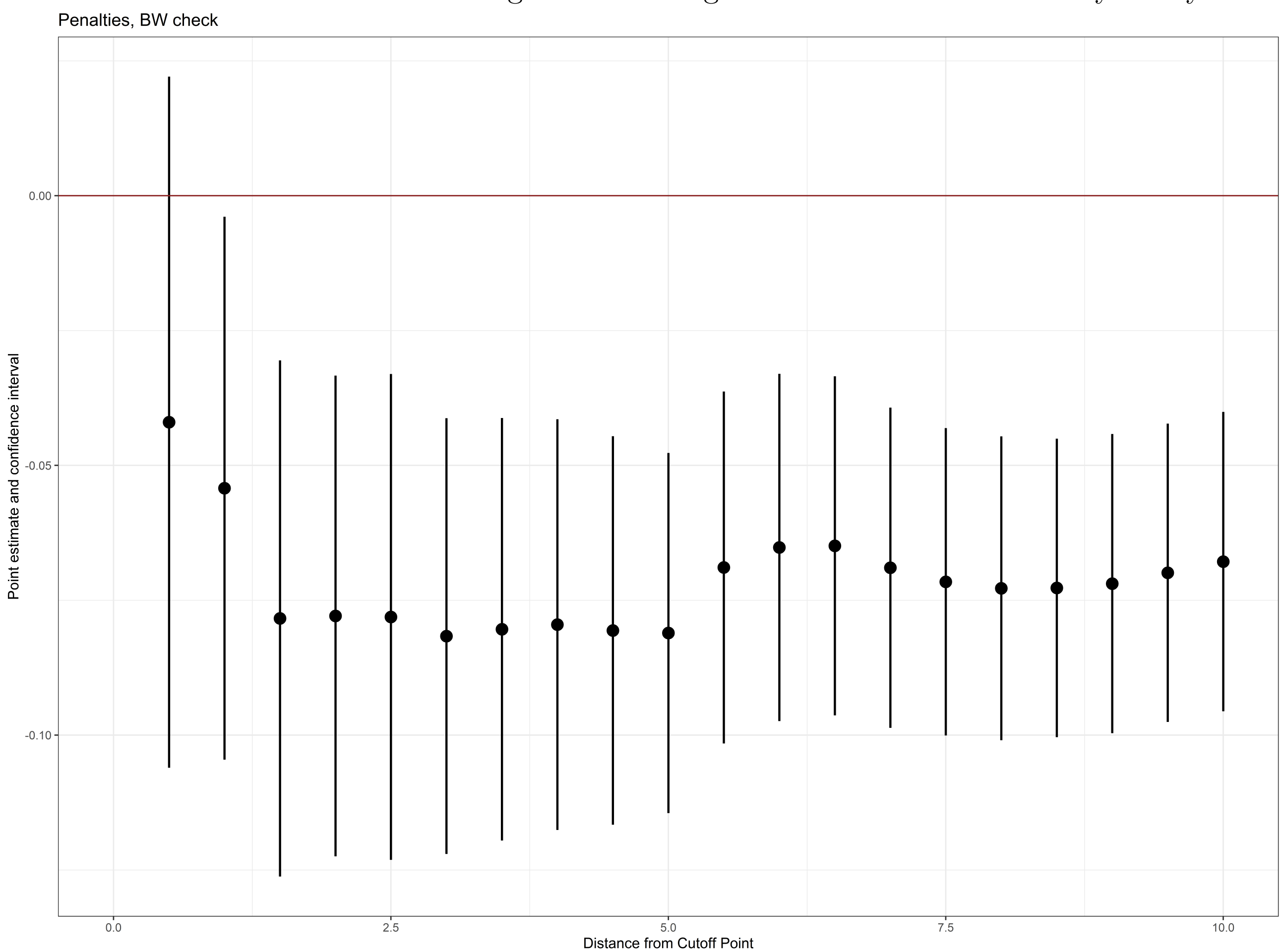


Figure shows invesigation RD point estimates and confidence intervals for varying bandwidths around the threshold.

Figure 6: Invesment Bandwidth Sensitivity Analysis

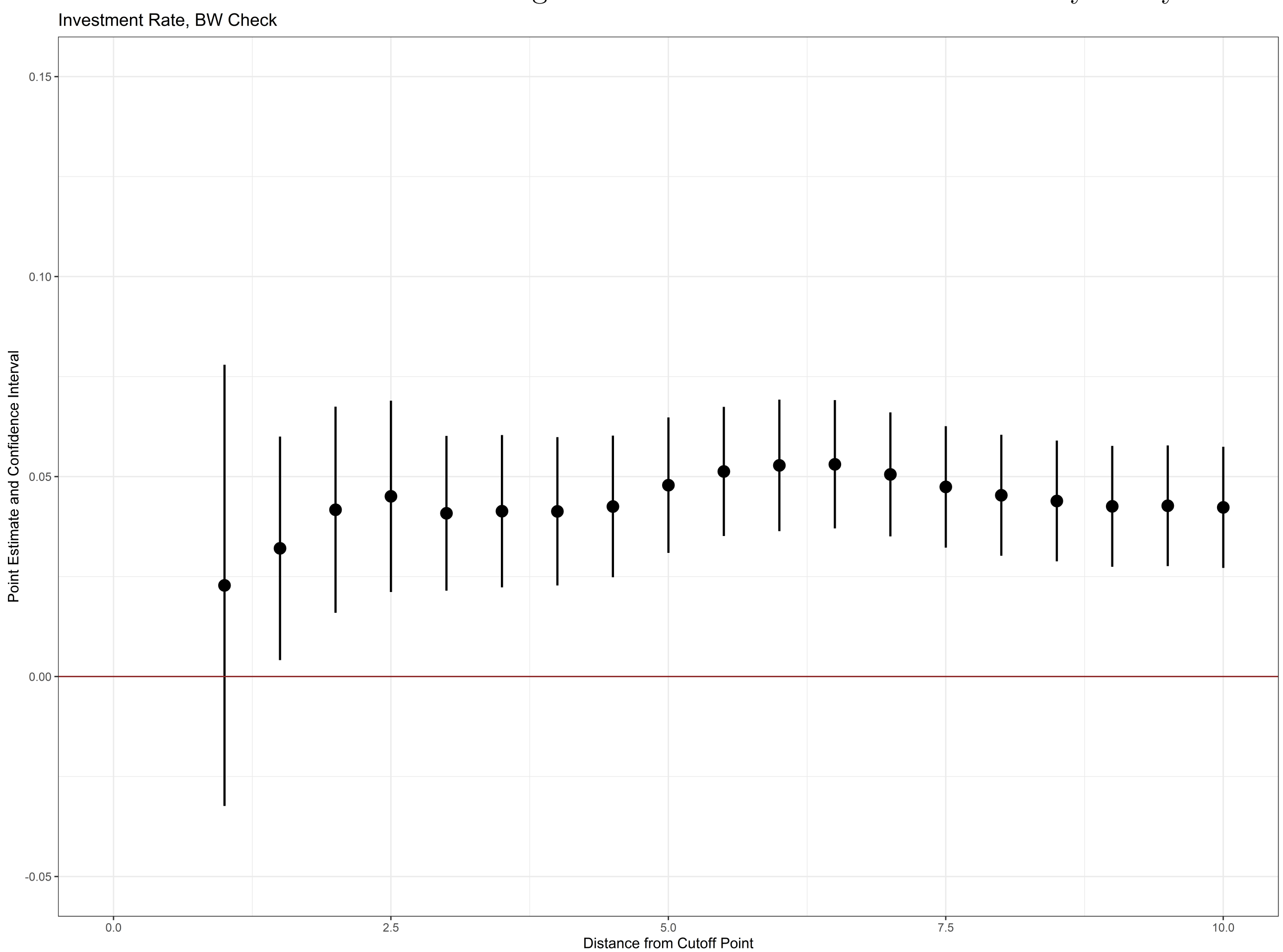
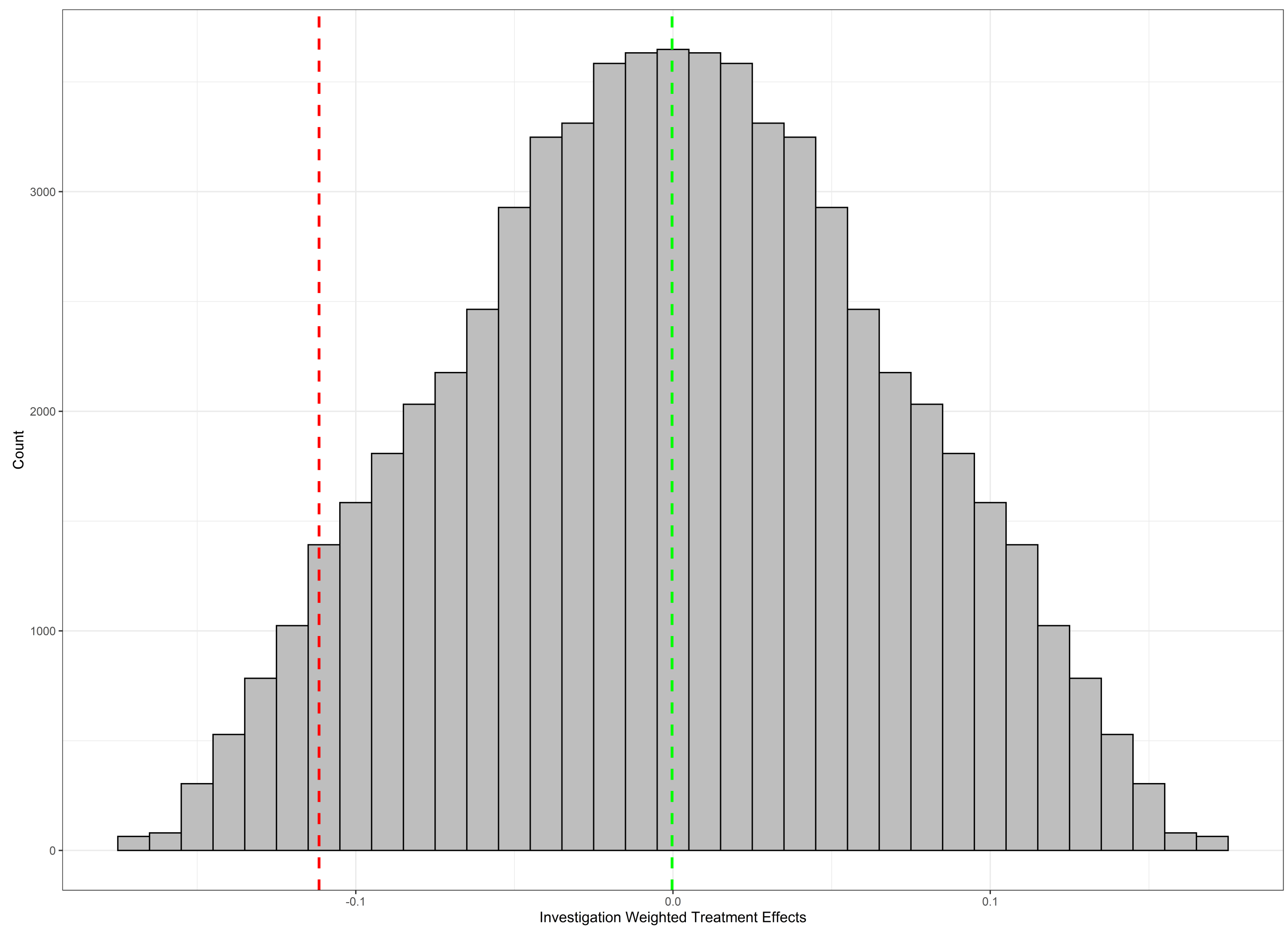


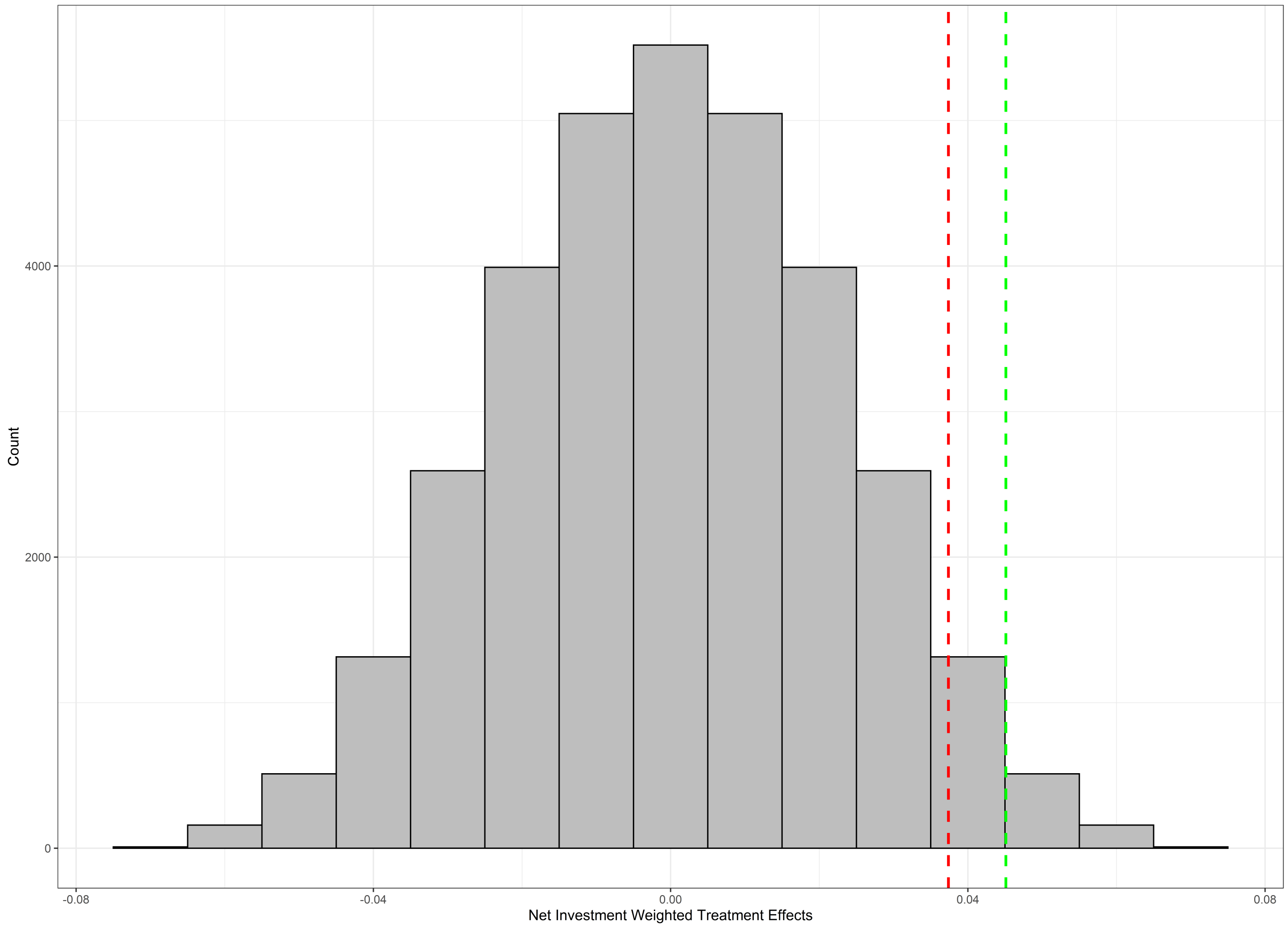
Figure shows investment rate RD point estimates and confidence intervals for varying bandwidths around the threshold.

Figure 7: Randomization Inference: Investigations



The figure shows the distribution of potential treatment effects under the sharp null, for elections decided by a margin of 3 percent or less. Every election is treated as one observation, and differences of means of net investment rates for firms that are connected to the winner and to the loser respectively in a given electoin were calculated. Treatment effects closer to the threshold were weighted more heavily, as a triangular kernel was being applied. The red line denotes the 5th percentile, while the green line denotes the treatment effect observed in my data.

Figure 8: Randomization Inference: Investment



The figure shows the distribution of potential treatment effects under the sharp null, for elections decided by a margin of 3 percent or less. Every election is treated as one observation, and differences of means of net investment rates for firms that are connected to the winner and to the loser respectively in a given electoin were calculated. Treatment effects closer to the threshold were weighted more heavily, as a triangular kernel was being applied. The red line denotes the 95th percentile, while the green line denotes the treatment effect observed in my data.

C Model

In this section, I present a short model loosely based on Gordon and Hafer (2005) in order to fix ideas. Consider a model with two players: a state AG AG, and a firm F. The state AG can decide to spend resources on a prosecution P of the firm, which is either politically connected to the state AG or not. The firm on the other hand makes investment decisions, given the likelihood of being prosecuted in the future.

The utility functions then look as follows:

$$U_{AG}(P;c) = -P + f(c)R(P) \tag{2}$$

, where P is the intensity with which a prosecution is conducted into the firm, which causes costs $-P$ to the state AG. The state AG reaps a reward R from a prosecution, where $R'(P) > 0, R''(P) < 0$. Lastly, the reward decreases when the firm is politically connected to the state AG as future contributions would decrease, such that $f'(c) < 0$. c is a binary variable that captures whether a firm is politically connected to the state AG or not.

The firm's utility function is

$$U_F(I;c) = -I + g(P(c))F(K+I) \tag{3}$$

, where I is the cost of investment in the current period, and the investment will then be added to the capital stock for the next period production function F, which is assumed to look as follows: $F(K) = K^{\alpha}$. Production in the future might be hampered by state AG prosecution efforts $P(c)$, which will be a function of whether firm and state AG are politically connected. Prosecution efforts translate into a more adversarial business environment, which is captured by the function g, where $g'(P(c)) < 0$.

Taking first order conditions of the state AG's and the firm's utility functions with respect to their choice variables respectively yields the following:

$$R'(P) = \frac{1}{f(c)} \implies P'(c) < 0 \tag{4}$$

$$I = (\frac{1}{g(P(c)\alpha})^{\frac{1}{\alpha-1}} - K \tag{5}$$

Taking the results combined, the model predicts that in equilibrium $I'(c) > 0$; i.e., a firm invests more when it is connected to a state AG.[35]

[35]Note that the model could be extended to endogenize political connections, and to include the severity of firm-level non-compliance as well as firm-level productivity, and the effects of accountability.

D Dataset Construction

E Degree of Freedom Adjustment to RD Estimates

F Resource Misallocation

To fix ideas, consider a sector s and firms i. Assume that capital and labor go into the production function, and let the rental rate of capital in sector s be denoted by r_s, while the wage rate in sector s is w_s. Given a Cobb-Douglas production function with only capital and labor as input, firms maximize their profits over the choice of capital and labor. This choice can be expressed as follows:

$$max_{K_{si},L_{si}}\Pi_{si} = P_s A_{si} K_{si}^{\alpha_s} L_{si}^{1-\alpha_s} - r_s K_{si} - w_s L_{si} \tag{6}$$

Note that A_{si} captures total factor productivity of company i in sector s, in quantity terms; moreover,

$$P_s A_{si} K_{si}^{\alpha_s} L_{si}^{1-\alpha_s} = P_s Y_{si} \tag{7}$$

, where the right hand side captures sales. The first order conditions of the firm's profit maximization problem with respect to the firm's choice of capital then imply

$$P_s Y_{si} A_{si} \frac{1}{K_{si}} = \frac{r_s}{\alpha_s} \tag{8}$$

Notice that the quantity on the right hand side is a constant, and assume that sales are constant in the short run. Then, in optimum, A_{si} and K_{si} need to move in the same direction; these two quantities - capital and total factor productivity - are therefore sufficient statistics that capture efficiency costs due to capital misallocation stemming from political connections.