Elected Public Office and Private Benefit: Firm-level Returns from Businesspeople Becoming Politicians in Russia

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Abstract

Do firms benefit from political connections? If so, what are the underlying mechanisms behind the positive relationship? Using an original dataset of close to 3,000 politically connected firms in Russia, I examine the widespread but understudied strategy of businesspeople directly running for elected office. The paper employs a regression discontinuity design to identify the causal effect of gaining political ties, comparing outcomes of firms that are directed by candidates who either won or lost close elections to subnational legislatures. I first find that a connection to a winning politician can increase revenue by roughly 60% and profit margins by 15% over their time in office. I then test between different mechanisms potentially explaining the results, finding that connected firms improve their performance by gaining access to bureaucrats and reducing information costs, and not by signaling legitimacy to financiers. Finally, winning a parliamentary seat is more valuable for firms where democratization is greater, but less valuable when firms face acute sector-level competition. This finding suggests that the intensity of economic rivalry, rather than the quality of political institutions, best explains the decision to send a director into public office.

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1 Introduction

Businesspeople occupying higher political office are commonplace around the world. Though little systematic data exists, high levels of business penetration of national-level parliaments have been noted in the last decade in such places as Thailand, Benin, China, Kyrgyzstan, and Ukraine.[1] Work on so-called 'moonlighting politicians', members of parliament that continue to work in the private sector after election, has noted sizable shares of policymakers with outside employment in Italy, the United Kingdom and Canada, among others.[2] Private firm directors have even become national leaders of the executive branch in countries across Western Europe and Asia, such as Finland and Chile (DellaVigna et al., 2013).[3]

There are different theoretical explanations about why businessmen might run for office. One theory claims that weak democratic institutions may incentivize holding office, since voters are unable to punish businessperson politicians when they pursue private firm interests over the public good (Gehlbach, Sonin, and Zhuravskaya, 2010). Another holds that greater oligopolistic competition reduces the probability that lobbying or campaign contributions will result in policy dividends, making directly occupying political office the optimal non-market strategy (Szakonyi, 2015). Other work has shown that due to the high costs of winning office for businessmen, those that do run may be pursuing public service due to policy motivations or altruism (Fox and Lawless, 2005; Diermeier, Keane, and Merlo, 2005). To date, however, we have little evidence about the reasons behind and consequences of businesspeople simultaneously working in the public and private sectors (Geys and Mause, 2011).

In this paper, I examine whether businesspeople who become legislators are able to secure benefits for their firms while in elected office. I constructed an original dataset of 2,720 politically connected firms in Russia over the period of 2004-2012, matching over 12,000 regional-level candidates to any companies they directed at the time of their electoral campaign.[4] By exploiting close elections where the determination of the winner and runner-up is near randomly assigned (Lee, 2008; Eggers et al., 2014), I employ a regression discontinuity (RD) design to identify the causal effect of political connections on firm-level outcomes. Capitalizing on this discontinuity in the assignment to treatment, the RD design can causally attribute any differences in profitability, revenue, or other measures of the candidates' firm performance to the effect of winning elected office. In addition, I draw upon over 40 semi-structured interviews conducted with key actors at the regional level in Russia, including winning and losing businessperson candidates, to help elucidate the mechanisms behind any effect found. Results indicate that firms indeed derive significant benefits from having their director win political office at the regional level in Russia. Firms connected to winning candidates

[1] See Bunkanwanicha and Wiwattanakantang (2009), Koter (2014), Truex (2014), Wong (2010), Sjöberg (2011), and Semenova (2012)

[2] See Gagliarducci, Nannicini, and Naticchioni (2010) and Pan et al. (2014).

[3] Bershidsky, Leonid. April 20, 2015 "Another Wealthy Businessman Takes a Crack at Running a Country." *National Post* http://news.nationalpost.com/full-comment/leonid-bershidsky-another-wealthy-businessman-takes-a-crack-at-running-a-country (accessed March 17, 2016)

[4] I define a connected candidate if that individual served as director, deputy director or on the board of directors in the year he or she ran for office; in Russia, directors are equivalent to the Western titles of Corporate Executive Officer (CEO) or Director General.

increase revenue of 60% and profit margin by 15% in the final year these candidates spend in office. These results are statistically significant, pass a number of robustness checks that vary RD specifications, and reflect a local average treatment effect for firms located near the winning threshold.

Several underlying mechanisms are consistent with these findings. Direct ties to politicians may benefit firms by improving their reputation among financiers and investors or, alternately, by opening doors to bureaucrats and favorable state treatment. The distinction between the channels is important for informing how policymakers should develop regulations that curb rent-seeking. If weak legal institutions induce companies to build political ties in an attempt to secure financing, then strengthening rule of law and contract enforcement should be paramount. On the other hand, if connected politicians are abusing access to regulators and state agencies, public service reform should come first. To test between the two mechanisms, I collected empirical data on ways firm directors could convert their legislative power into performance improvements. I find that serving in office helps businessperson politicians win additional state contracts for their companies, but not increase their financial leverage. These findings suggest that connections are not alleviating credit constraints, but instead reducing information and transaction costs in dealing with government officials. Though direct data on regulatory benefits is unavailable, firms possessing immobile assets also enjoy greater revenue and profits. I interpret this result as supporting the wider claim that having a firm director win office helps relieve bureaucratic pressure and protect property rights.

I then exploit a rich national laboratory to examine how structural and institutional characteristics affect the payoffs of cultivating political relationships. First, somewhat counterintuitively, revenue and profits are higher for winning firms located in regions with more democratic institutions. I argue that when legislatures are able to exert policymaking authority and serve as a check on the executive, the opportunities to redirect budgetary resources to private interests are greater. Firms that are allied with the ruling party also see somewhat higher returns from winning office compared to the firms of those members of the party who lose office. Winning opposition-oriented candidates however gain large dividends for their firms, suggesting that regimes use political institutions to distribute rents to both supporters and potential opponents. More intense political battles within parliament may require more government resources to buy off all connected firms. This paper thus offers an important example of a situation where political competition does not check rent-seeking. Strong economic competition, however, reduces of the value of winning office for firms. When winning firms encounter sectoral rivals who have also secured a seat in a parliament, they find it more difficult to carve out private benefits. Deliberation within the parliament is most akin to a marketplace, with profit margins dropping with the appearance of new entrants. Lastly, political ties are more valuable in regions with natural resources, since the overall economic pie and relevant budgetary resources are larger for firms to take advantage of.

Russia emerges as a particularly interesting case for such a study, given the substantial presence of businesspeople on candidate slates and extensive firm-level data to uniquely identify the directors of all firms across the country. Like many developing and middle-income countries, Russia has weak enforcements of laws, creating dependence on polit-

ical ties as an important non-market strategy (Puffer and McCarthy, 2011; Slinko, Yakovlev, and Zhuravskaya, 2005). Though national-level democratic institutions were weakened during the period of study, writing off Russia as authoritarian masks important regional variation in democratic development, resource wealth, and economic concentration (Bruno, Bytchkova, and Estrin, 2013). Elections for regional legislative office are also an important target of corporate political activity in Russia (Mironov and Zhuravskaya, 2015; Reuter and Turovsky, 2014). This subnational variation in Russia improves our ability to generalize findings to other settings where conflicts of interest have also been found between politicians and bureaucrats (Acemoglu et al., 2013), while allowing us to hold constant macroeconomic factors that might imperil a cross-national study. Lastly, in Russia, no law prevents businesspeople at the regional level from holding office, leading to fewer instances of the asset-hiding common at the national level and the cause of measurement error. Comprehensive balance sheet information available in Russia provides the unique opportunity to study political connections on a scale unavailable to researchers using surveys or data from publicly traded companies.

This work contributes most directly to the literature on the determinants of and returns to nonmarket strategy, in particular political connections. Businesses have numerous avenues to enter the political arena and must navigate a series of trade-offs between allocating resources to lobbying or campaign contributions or to developing direct political connections (Lux, Crook, and Woehr, 2011). But even with an abundance of scholarship estimating the benefits of relational ties for firms (Khwaja and Mian, 2005; Boubakri et al., 2012; Hillman, Keim, and Schuler, 2004; Goldman, Rocholl, and So, 2013), we know comparatively less about which firms expend resources to develop them and how they manage to do so. My paper explores an oft-ignored but widespread type of corporate political activity based on relational ties, businessperson candidacy to elected office, whereby firm directors become legislators to help their firms achieve political influence. Running candidates for political office serves as a powerful mechanism to building insider political capital and is potentially available to all firms in a polity where elections are held. Political ties are allocated not solely through bribes or backdoor dealings, but out in the open as determined by a voting body. Businessperson candidacy in many respects democratizes how firms acquire political connections. However, with the notable exception of Gehlbach, Sonin, and Zhuravskaya (2010), little attention has been paid to this alternate approach of influencing politics. This paper offers the first empirical analysis of the value of businessperson candidacy.

To do so, I adopt an identification strategy that goes beyond matching and simple regression analysis to estimate the causal effect of these connections. This research design is closest to Boas, Hidalgo, and Richardson (2014), who also employ close elections to study the returns to campaign contributions. But businessperson candidacy may lead to more serious distortions for overall economic development than simply donating to campaigns or using public office to increase personal wealth (Querubin and Snyder Jr, 2011; Fisman, Schulz, and Vig, 2012; Eggers and Hainmueller, 2009). The takeover of legislatures by powerful firms (i.e., 'state capture') can have enormous social costs, as winning firms reap rewards not based on their market success or productivity but on their ability to win elections (Hellman, Jones, and Kaufmann, 2003). I show that elections function less as an opportunity for citizens to express their voice than

as a way to determine specific economic winners and losers by allocating insider benefits from the state to connected firms. In that regard, it answers a call in several fields to use natural experiments to dig into the mechanisms by which political connections actually change economic outcomes, instead of simple presenting cross-sectional comparisons (Acemoglu et al., 2013; Fisman, 2001; Hillman and Hitt, 1999; Feinberg, Hill, and Darendeli, 2015). In addition, I present new evidence about how structural and institutional factors impact the value of corporate political activity, building our understanding of the relationship between democratization and corruption (Treisman, 2007; Faccio, 2006; Li, Poppo, and Zhou, 2008). My work shows that stronger economic rivalry, rather than political competition, results in less rent-seeking. Strengthening state institutions to prevent excessive industry concentration could reduce the appeal of directly seeking office for firms, as would public service reform to enforce transparency in public procurement and regulation.

The paper finally makes several contributions to literature on the use and consequences of political institutions in developing democracies and autocracies. To date, much work on hybrid and non-democratic regimes has focused on why regimes adopt nominally democratic institutions to their own benefit (Brancati, 2014), with comparatively less done on why elites join up and legitimate these institutions as viable actors within society. My research provides some of the first causal evidence for the claim that elections to parliamentary seats are used to distribute rents among elites (Blaydes, 2011; Gandhi and Lust-Okar, 2009). This improves our understanding of how dominant parties both retain the loyalty of their members as well as co-opt other parties within society to increase their hold on power (Reuter and Turovsky, 2014; Reuter and Robertson, 2015). By utilizing the natural experiment of close elections, I show that institutions in competitive authoritarian regimes are not epiphenomenal to larger societal dynamics (Pepinsky, 2014), but instead can have independent effects on the behavior of elites and interest groups.

2 Businessperson Candidacy as Non-Market Strategy

Firms are believed to make investments in the political marketplace with the expectation of financial returns, such as increasing shareholder value or improving their firm's revenue. The type of benefit pursued can depend on a variety of factors, including sector, size, and the business environment where a firm operates. For example, in weakly institutionalized regimes, businesspeople enter politics in order to secure stronger property rights (Hellman, Jones, and Kaufmann, 2003; Markus, 2012). In more developed business environments where property is less easily expropriated, firms may look to politicians for assistance in maximizing rents in a firm's value chain or lowering their taxes, though these objectives can be present among firms anywhere (Barnett, 2006).

Multiple strategies are available to firms looking to enter politics. However, the extant literature on corporate political strategies has tended to focused on the two types viewed as the most dominant in the 'political investment portfolio' (Schneider, 2012): lobbying and making campaign contributions (Coen, Grant, and Wilson, 2012). Both

lobbying and contributing to campaigns are examples of what I term *indirect* corporate political strategies. Firms contribute information, money, and/or votes to politicians in exchange for access and influence (Hillman and Hitt, 1999). Politicians then become intermediaries and advocate on the firm's behalf to achieve its policy goals. Though larger contributions are presumed to increase the probability that a politician will implement the 'bought' policy, indirect strategies provide no formal guarantee that the exchange of policy will take place.

Yet analyses of the benefits of indirect political strategies have not reached definitive conclusions about whether a firm's expectation of a positive return is warranted. Evidence of positive political outcomes is ample (Bonardi, Holburn, and Bergh, 2006; Hillman, 2005), but well-founded identification strategies are much rarer (a recent exception is Boas, Hidalgo, and Richardson (2014)). Some studies have also found no effect of soft money and other political activities on specific firm-level outcomes (Ansolabehere, Snyder Jr, and Ueda, 2004), or that the effect of ties is continent on factors such as the structure of government and partisan competition (Choi, Jia, and Lu, 2014). Engaging in corporate political activity may even produce negative returns for some firms (Aggarwal, Meschke, and Wang, 2012). For example, lobbying in the U.S. in the financial sector often exposed firms to worse than normal stock returns once the financial crisis hit in 2008 (Igan, Mishra, and Tressel, 2011). Similarly, Hadani and Schuler (2013) look at the most popular corporate political activities in the United States (campaign contributions, lobbying and hiring public officials) and find a negative effect of all three on firm returns, except in the case those firms operating in highly regulated sectors.

Besides lobbying and making campaign contributions, firms also have a range of options that forgo the use of political intermediaries. These more direct political strategies essentially blur the line between politicians and firm executives. Aligned incentives (the politician now benefits monetarily from improvements in firm performance) may help deflect other actors competing for the politician's attention. Direct strategies more closely bind the politician to a firm and provider a stronger guarantee that an individual firm's interest will be represented. A common way of nurturing direct political connections is the placement of current or former politicians on the board of directors.

In this paper, I focus on a much less studied variant of a *direct* political strategy: a firm director himself or herself running for political office. Besides inviting former and current politicians to join the firm, firms can also send their own representatives into political office. Though appointments to executive positions indeed happen, a more widely available avenue for securing influence in government is for the firm director or trusted manager to seek a legislative seat. Part of the reason firm directors run themselves is that they don't trust politicians to represent their interests. At heart is a commitment problem: firm directors have no guarantee that the money they give will be returned in-kind with policy after the election. Sending friends and trusted relatives as firm proxies may help mitigate this problem, but such an approach can turn away voters for whom the proxy is an unknown political entity. A firm director, as employer and benefactor, commands superior name recognition and respect within the community, two vital characteristics for winning personalized elections. Therefore, he or she must personally represent the firm's political interests to capitalize on these electoral advantages. I define this overall approach to politics as 'businessperson candidacy,' a widely used,

but less understood corporate political strategy.

In related work, I investigate the exact conditions under which firm directors will opt for businessperson candidacy (Szakonyi, 2015). I claim that all firms would be better off if none of their directors personally ran for public office; they would save on the costs of the campaign and delegate politics to the politicians. Certain factors exacerbate this coordination dilemma. Acute economic competition increases the likelihood that competitors will seek political influence. The absence of strong political parties prevents firms from aggregating their interests and cooperating to check the reneging impulses of designated political representatives. Lastly, I find that larger firms are more likely possess the resources to afford campaign costs and any other political commitments derived from holding office.

Overall, the choice to send representatives from the firm directly into political office differs markedly from the other indirect and direct strategies discussed above. First, when businessmen personally occupy political positions, they enjoy unparalleled access to policy decisions. In Russia, businessmen legislators gain direct access to the executive branch by virtue of their political status and weight in opening doors to bureaucrats (Sakaeva, 2012). A current businessperson deputy from Tomsk claimed that bureaucrats are required to meet with deputies if they ask; if the event of non-compliance, these politicians can submit 'deputy requests' (*deputatskiye zaprosi*) that can force bureaucratic action in favor of their businesses.[5] Certain well-positioned parliamentarians can even draft laws to benefit their businesses.

Another notable difference between businessman candidacy and other corporate political strategies is the former's substantial cost. In fact, running for office may be the most resource-intensive and costly of all the strategies outlined. While lobbying and making campaign contributions can also run up huge tabs, becoming a politician requires a massive amount of time and resources. In the single-member districts elections I analyze below, businesspeople must finance electoral campaigns entirely on their own, without party support. One deputy put total cost of the campaign, the majority of which went to paying for ads and mobilizing voters, at up to 5-7 million rubles ($160,000-$200,000).[6] Even more importantly, interviews with several deputies uncovered that spending money is no guarantee of victory, and those expenditures are non-refundable.[7] Electoral politics can be contentious and vicious. Losing at the polls could cause a hit to the reputation of the firms, especially if it tied itself to divisive or controversial stances in order to get elected.

Once in office the designated representative must allocate some portion of their time to political duties instead of those related to firm operations (Geys and Mause, 2011). During re-election campaigns, voters will evaluate politicians not according to firm performance (like shareholders would, for example), but on their ability to deliver public goods

[5] Interview with businessman and deputy of Tomsk regional parliament, June 11, 2014. Tomsk, Tomsk Oblast, Russia.

[6] Exact estimates of the cost of winning election are hard to come by. Mironov and Zhuravskaya (2015) examine shadow transfers around regional gubernatorial elections and find that firms transferred on average a total of $2.5 million to electoral campaigns. Another estimate put the cost of winning a seat in the Omsk regional parliament at $80,000-150,000 in 2002 (Barsukova and Zvyagintsev, 2006).

[7] Interview with deputy of Tomsk regional parliament, June 6, 2014. Tomsk, Tomsk Oblast, Russia. Interview with businessman and deputy of Tomsk regional parliament, June 11, 2014. Tomsk, Tomsk Oblast, Russia.

and direct political attention to their constituencies. One deputy admitted that "being a deputy and a businessman at the same time is not easy"; the amount of constituent requests for help, especially financial assistance, was a significant burden on his ability to run his firm.[8] Firms may also need to satisfy social obligations to their constituents, often times mandated by the government in exchange for preferential treatment in other areas. This diversion of time and resources from pure economic activities can easily surpass financial expenditures on lobbying or campaign contributions, making businessperson candidacy a especially resource-intensive strategy.

Evidence of the benefits of direct political connections has been abundant, but true causal effects have been much harder to identify. Moreover, political connections may also undermine a firm's competitiveness, investment behavior, and ability to innovate (Desai and Olofsgard, 2008). Successful politicians may also not be effective firm managers, as government intervention into company management may lead to weak incentive systems and inadequate monitoring (Okhmatovskiy, 2010). In addition, the effectiveness of political ties may be undermined by the institutional environment. In countries with strong rule of law, direct political connections can actually hurt stock performance (Brockman, Rui, and Zou, 2013). If political circumstances change, a tie to the 'wrong' type of politician can even impose a range of negative consequence on a firm (Siegel, 2007). The direct strategy of cultivating ties can incur sizable risks for a firm with only contingent benefits, muddying the picture of the overall effectiveness of these types of political activities.

To briefly summarize, there is no consensus over whether corporate political activity as a whole is a profitable strategy for firms. The wide variation in empirical results strongly suggests the need for a more refined approach to analyzing the return on political investments. Although firm directors may be rational actors intent on improving market-based performance, their understanding of the political environment they are engaging in may be limited or flawed. Thus, the aim of this paper will be to examine if, when and how the strategy of firm directors seeking elected political office pays off for their companies' bottom line.

3 Data Description

To test the effect of having an affiliated person (director or member of the board of directors) hold political office on firm performance, I adopt a regression discontinuity (RD) design that exploits 'close' elections. On average across a large sample of elections, winning and losing candidates located near the cutoff score (the threshold required to win the election) should be plausibly comparable, as if victory in the elections was randomly assigned. Therefore, close elections become akin to a coin flip, dependent on such circumstantial exogenous factors as the weather on election day (Lee, 2008). RD designs using close elections have grown increasingly popular in studies across social sciences disciplines due to the clear assumptions required and their ability to identify a causal effect (Eggers et al., 2014). In this case, I employ the RD design to compare firm-level outcomes for those companies that are connected to candidates

[8] Interview with businessman and deputy of Tomsk regional parliament, June 11, 2014. Tomsk, Tomsk Oblast, Russia.

whose vote share falls close to the threshold required to win office. That is, I compare firms connected to narrowly winning candidates to firms connected to narrowly losing candidates. If the assumptions of the RD design are met, this empirical strategy excludes the influence of unobserved differences between both candidates and firms and allows us to measure the true economic effect of a firm having a connection to a legislator (Boas, Hidalgo, and Richardson, 2014; Meyersson, 2014). Below I present the data from the Russian case, the model specifications, and balance tests to satisfy the underlying assumptions of the RD design.

3.1 Data Description

I study the effect of political connections on firm performance using data on regional legislative elections held in Russia between 2004 and 2011 which comes from the Central Election Commission of the Russian Federation (CEC) and collected by the Center in Support of Democracy and Human Rights Helix.[9] The sample consists of 116 elections to regional convocations in 73 regions from January 1, 2004 until March 3, 2011.[10] Operating at the highest subnational level, regional legislatures are critical actors in Russian politics, holding responsibility for passing budgets, developing programs for social and economic development, confirming the appointment of officials, and setting land and transportation tax rates, among other activities. Organized interest groups view these legislatures as key sites of contestation over policy and spoils, where laws with long-term impacts on regional concerns are drafted (Reuter and Turovsky, 2014; Remington, 2008). Legislative committees are convened on a variety of issue areas from agriculture to health and education, helping drafting laws that affect funding such as subsidies, state guarantees, contracts, and transfers from the regional budget. These powers make them attractive for companies looking to get involved politically through a variety of means, as evidenced by multiple firm surveys (Marques, Govorun, and Pyle, 2014; Reuter and Turovsky, 2014).

The choice of the sample's starting point is due to a federal law that took effect in December 2003,[11] which required that no less than half of the total number of deputies in each regional legislature must be elected according to party lists. Golosov (2011) observes that the passage of this law fundamentally changed the structure of regional political competition, and ushered in a new era of political party development in the Russian Federation. Each region determines the exact allocation of seats between election through party list (PR) and plurality electoral districts; approximately 41% of all legislative seats from 2004-2011 were chosen using plurality rules. Regional legislative elections are set every four or five years by each regional legislature in Russia. Fixed beforehand and exogenous to any political or

[9]The Helix Center has systematized all election results from the CEC and uploaded the data to a centralized database found at http://db.geliks.org/. I collected the election data used in the paper from the Helix site, filling in any missing data from the primary CEC website (http://www.vybory.izbirkom.ru/region/izbirkom).

[10]The Russian Federation is technically composed of different types of federal subjects, including republics, oblasts, krais, autonomous krugs, and federal cities, even though each is governed by the same federal legislation. For the purposes of this paper, I refer to all of these entities through the single word: 'region.' Similarly, each of these regions has their own name for their lawmaking body. I use the term regional legislature to refer to them as a whole and 'Regional Duma' when using them as proper nouns.

[11]Federal Law No. 67-FZ of June 12, 2002. 'On the basic guarantees of citizens' electoral rights and the right to vote in referenda' (http://www.cikrf.ru/law/federal_law/zakon_02_67fz_n.html). Accessed March 8th, 2014.

socioeconomic factors, the Russian electoral calendar is such that roughly 10% of the total number of regions held a legislative election every six months during the time period from 2004 to 2011. Lastly, regional level deputies do not enjoy parliamentary immunity, unlike their counterparts at the national level and in countries like Ukraine.[12]

Because of difficulties analyzing close races with close margins between three or more candidates, I exclude all races where the difference between the winning candidate and the third place candidate is less than 5%. I also omit 45 multi-member districts, as the probability of being above the cutoff score is no longer 50%. I also drop all firms connected to candidates from the sample that lost their single-member district race, but gained a seat in the legislature on the party list. The analysis will then compare only the winners and losers in single-member district races, or 12,113 candidates running for office in 2,798 elections. The treatment is assigned at the level of the candidate, while the unit of analysis is the politically-connected firm. I define a political connection as having the firm's director, deputy director, board chair, or board member run for legislative office.[13] The treatment variable is winning office to a regional legislature and takes a value of 1 if a firm is connected to a winning candidate and 0 otherwise. The forcing variable used is the margin of victory between the winning candidate and the first runner-up in each single-member district. This gives us a cutoff point at zero, with all firms connected to candidates with a positive margin of victory entering the treatment group and all firms connected to the first runner-up joining the comparison group.[14] This variable, *Vote Margin*, takes values from -1 to 1.

Both media and scholarly accounts of political developments in Russia raise concerns that elections to Russian regional legislatures may not be sufficiently competitive to allow for an RD design to be implemented. Although some degree of falsification does occur at the regional level, there are several reasons to believe that elites are truly competing for votes and not all electoral outcomes are not pre-ordained. First, the average margin of victory is 30.1% with a median of 25.7% (see Appendix for a graphical depiction). In terms of close elections, 634 elections were decided by less than 10 percentage points, or roughly 23% of the total sample. This considerable number of competitive elections and continuous nature of the forcing variable will allow us to isolate the RD treatment effect right around the electoral threshold. Competitive elections are also distributed proportionately across Russia. Figure 1 presents the regional breakdown of election decided by less than a 10% vote margin, as calculated as a proportion of the total number of SMD elections per region.[15] Next, to preview further discussion below, I examine balance along a range of co-variates between winning and losing candidates in close elections and find no evidence that electoral manipulation favors a

[12]"Judge Decided that Deputies Possess Enough Immunity". MediaKorSet, February 10, 2009. Accessed on February 2, 2015 at "http://www.mkset.ru/news/chronograph/11273/"

[13]The appendix presents robustness checks for defining a businessperson as only a firm director or deputy director.

[14]Firms connected to losing candidates do not constitute a true control group under a causal framework, since these firms are to a degree politically connected by virtue of their directors running for political office. Instead, the comparison analyzed here is between those firms connected to winning candidates and those connected to losing ones. Below I present an extension where a true control group of unconnected firms is approximated through matching, but not causally identified. Under the mixed-member electoral system, candidates are also allowed to run on both the PR and the SMD lists. In the Appendix, I show results from models that look only at candidates that ran in single-member districts.

[15]In Appendix Table A4, I run a series of models that regress the incidence of close elections (as defined as being decided by 5%, 10%, 20% or 30% of the vote) on a battery of possible determinants. Competitive elections are on the whole not significantly different from their non-counterparts, except for two critical factors: they involve a significantly larger number of candidates and the ruling United Russia party candidate is much less likely to win.

specific type of candidate or firm. Lastly, if authorities are indeed faking electoral competition to build legitimacy among the population, then we should not expect any financial benefit to accrue to the winners (or for that matter punishment inflicted on the losers). Any coordination between candidates would result in the rent-sharing between complicit parties (in this specific case, firms), and not significant advantages bestowed on the anointed victor. I do exclude the December 2011 election from the sample due to persistent concerns over vote fraud (Enikolopov et al., 2013).

The main outcome variables for this study are changes in reported revenue (logged and measured in the millions of rubles) and profit margin (net profits over total revenue) for each firm over the term in office.[16] I calculate the differences by subtracting each of the values for each connected firm in year prior to the election from their values in the final year of electoral term. Therefore, I include only firms that reported balance sheet data beginning the year prior to the election and spanning the entirety of the term in office. In the Russian case, all firms are required to submit their balance sheets and income statements to the official state statistics agency Rosstat every year. The majority of companies comply in order to maintain good relations with the authorities (Mironov and Zhuravskaya, 2015). The SPARK Professional Market and Company Analysis System aggregates this official data, including registration information and financial statements, for nearly 3 million firms in Russia (as of March 1st, 2014) over the last 15 years. This official financial data from SPARK has been used widely by academics and journalists alike studying firm-level performance, as well as malfeasance, in Russia.[17] Using reported financial data to analyze organizational performance may introduce some biases. For example, companies may avoid submitting accurate information about their profitability for fear of exposing themselves to greater tax liabilities or unwanted attention from hostile takeovers. However, given the sensitivity of politicians to unwanted public scrutiny of their financial dealings while in office, we might expect that politically connected firms would be more likely to hide their above-normal profits. This downward bias would make any identification of an effect of political ties on firm financial outcomes a lower bound.

In addition to the main financial outcomes of change in revenue and profit margin, I also collected an array of firm-level covariates from the main SPARK database. Below I show regression analysis with and without these controls, but given the myriad factors affecting firm performance beyond political connections, the most refined models are those that employ these covariates. The firm-level control variables used include a binary indicator marking the presence of any foreign ownership stake, a binary indicator marking the presence of any government ownership stake, and the natural log of total fixed assets (measured in millions of rubles) in the year prior to the election taking place. In addition, I employ sector fixed effects by coding the firms into two-digit categories according to the All-Russian Classification of

[16] Over the period under study, one Russian ruble equalled approximately $0.03.

[17] See, for example, Mironov and Zhuravskaya (2015) who use data from SPARK in their investigation of shadow election campaign financings, Mironov (2013) who looks at firm-level tax evasion, and several journalistic accounts of firms exerting influence on politicians (Beshley, Olga. 'Hunters of Oxotniy Ryad', *The New Times*, November 15, 2011.; Buribayev, Aidar. 'How Russian Elections Are Financed' *Forbes Russia*, October 11, 2012

Kinds of Economic Activity (or OKVED).[18] I exclude all firms working in the financial intermediaries and insurance sectors, including banks. Unfortunately the Russian government only collects minimal data on firms' lobbying or campaign contributions activity and only at the federal level. Therefore, this study is a strict comparison of firms with elected representation and those without. Lastly, I show models containing region fixed effects based on the 73 regions where the elections took place and year fixed effects taken from the year the outcome variables were measured. Candidate-level controls are measured using the Helix Center database and include the age of the candidate at the time of election, gender, a binary indicatory for membership in the United Russia ruling party, and a binary indicator if the candidate is an incumbent from the previous convocation of the regional legislature.

Along with the rest of the covariates at the firm-level, financial data was collected by matching each individual candidate with any firm-level positions stakes they held in the year prior to running for political office. Data on the affiliation of individuals with firms comes from the SPARK database, which collects registration data on almost 12 million 'individual entrepreneurs' who are uniquely identified through a tax identification number. Data on entrepreneurs includes personal demographic information (age, registration date, etc.) as well as entries for every legal entity that they had ever had an official affiliation with (firm director, member of the board of directors, etc.). The two datasets, firms and entrepreneurs, are housed together, making it easy for researchers to connect observations across them.

Using a programming script, I matched each candidate to his or her corresponding entry in the SPARK 'individual entrepreneur' database if one existed, using their first name, last name, middle name, region, and birthdate as identifying information. Roughly 73% of these candidates had unique entries in the entrepreneur database.[19] Next, I manually matched firms to all candidates who listed a company as their place of work on their ballot registration form but who were not located in the SPARK database; these manual matches accounted for 201 candidates from 241 firms, or roughly 9% of the sample. The final dataset includes firms that candidates directed at the time of their electoral campaign or sat on the board of directors. Due to data constraints, I am unable to identify whether other candidates not listed as directors ran for office on behalf of the firm (such as friends or relatives of the firm director). Similarly, I cannot measure lobbying, campaign contributions, or bribes made to politicians. Thus, the analysis presented below strictly compares firms whose director ran and won political office with those whose director ran and lost.

In all, I identified 2,720 firms connected to 1,976 candidates in Russia from 2004-2011. Put differently, these figures suggest that at least 16% of all candidates to regional legislatures during this period were drawn from the local community of firm directors and business executives.[20] Each of these candidates is connected to on average 1.5 firms at the time of their election campaign; I include all connected firms in the analysis. Roughly 17% of the companies work in wholesale or retail trade, the largest sector for those running for office, with the agricultural and food processing

[18] OKVED is the internationally recognized, industry standard of classification used by the Russian State Statistics service during this period.

[19] This however does not mean that 73% of all candidates are entrepreneurs: SPARK includes considerable information on a number of occupational characteristics of individuals in Russia, including time worked in public institutions, such as hospitals, schools, and political parties.

[20] I interpret this number as a lower bound because of the constraint that firms submit balance sheet information beginning in the year prior to the election and up until the end of the term.

sectors having the second and third largest number of firms with 12% and 10% respectively. During the year prior to the contested election of its director to regional office, the median firm has roughly 67 million rubles in fixed assets ($2 million), revenue of 80 million rubles ($2.7 million), and net profit of 925,000 rubles ($31,000). In fact, 28% of companies were in the red during that year. Companies with some degree of government ownership make up 6% of the sample, while those with a minority foreign ownership share constitute 3% of the total.

To supplement the quantitative data analyzed, I also conducted over 40 semi-structured interviews with a range of actors on the subnational level across Russia. Throughout 2013 and 2014, I spoke with businessperson candidates (winning and losing), deputies without direct business interests, journalists, academics, and civil society representatives from three regions in Russia: Tomsk, Ryazan, and Perm. The discussions below will draw upon these wide-ranging conversations, helping fill in key details about why businesspeople decide to run for office and how they potentially extract private benefits for their firms.

3.2 Regression Discontinuity Design

All analysis is done at the firm level, while the treatment is applied to candidates during the year of the election. Standard errors then should be clustered at the candidate level. I also collapse the panel data into a cross-section, as the two main outcome variables are differences between the values from the year prior to the election and the final year that a candidate served or would have served in office. I include the pre-election value of each outcome in every regression to account for differences in starting level. Because of midterm entries and exits, the average length of time a candidate spends in office is 4 years. For firms connected to losing candidates, the exit year is the final year of the parliamentary session to which the candidates ran for office.

I follow Lee (2008) in adopting a regression discontinuity approach that maximizes my ability to control for any differences in any observed and unobserved heterogeneity among firms. First, I show the effects from a simple OLS regression using the global (full) sample of firms connected to candidates. This model estimates a correlation between a politically-connected firm winning an election and performance outcomes. However, because of the potential biases discussed earlier, we cannot interpret the point estimates as reflecting a causal effect. The following specifications are used in these first OLS regressions (with and without controls):

$$Y_i = \alpha_i + \beta * z_i + Controls_{t-1} + \epsilon_i \tag{1}$$

where Y_i is the outcome variable for firm i (changes in revenue and profit margin over the term), z_i is a binary treatment indicator for whether a candidate won or lost the election, *Controls* is the set of firm covariates from the pre-election year and various fixed effects, and ϵ_i is a normally distributed error term.

Next, I use the regression discontinuity design to estimate a causal effect. The first approach sharply narrows the

estimation window and excludes the use of any control function. This design employs a simple OLS model, but more closely compares observations located right at the threshold and weights observations equally within this sample. I present results using both 2% and 3% windows around the threshold.

The second approach also narrows the estimation window, but includes a local-linear control function to control for any correlation between the vote margin (the forcing variable) and the outcomes of interest. I use windows of 5% and 10% in order to more closely hone in around the threshold. Below is the local-linear specification estimated, with and without controls:

$$Y_i = \alpha_i + \beta * z_i + \gamma * Margin_i + \eta * z_i * Margin_i + FirmControls_{t-1} + \epsilon_i \tag{2}$$

where Y_i is the outcome variable for firm i (changes in revenue and profit margin over the term), z_i is a binary treatment indicator for whether a candidate won or lost the election, *Margin* is the forcing variable (which is also interacted with the treatment variable under the local linear design), *Controls* is the set of firm covariates from the pre-election year and various fixed effects, and ϵ_i is a normally distributed error term.

The final specification uses a cubic control function on a wider sample, with the model estimated separately on both sides of the threshold. This allows us to fit smoothed curves that more heavily weight observations closer to the threshold, which helps control for problems such as endogeneity and omitted variable bias. In order not to overfit the regressions by including outliers at the tails, I restrict the sample to a bandwidth of 20%.[21] The specification for this approach is the following:

$$Y_i = \alpha_i + \beta * z_i + \gamma * f(Margin_i) + \eta * z_i * f(Margin_i) + \epsilon_i \tag{3}$$

where Y_i is the outcome variable for firm i (changes in revenue and profit margin over the term), z_i is a binary treatment indicator for whether a candidate won or lost the election, $f(Margin_i)$ is a cubic control function that is interacted with the treatment variable to fit above and below the threshold, *Controls* is the set of firm covariates from the pre-election year and various fixed effects, and ϵ_i is a normally distributed error term. All together, these approaches help illustrate the effects of various trade-offs made over the size of the window around the threshold and the type of control function adopted.

[21] The optimal bandwidth $\hat{h}$ as determined by the Imbens and Kalyanaraman (2012) algorithm returns values of 37% and 40% for the outcome variables respectively, which is far too large of a margin for an election to be considered close. Therefore I use a margin of 20%, or roughly half of the optimal bandwidth for the polynomial specifications.

4 Balance Checks

Before moving on to the results, I first run a series of standard validity checks to determine if any sorting is occurring around the cutoff point. Though regression discontinuity studies using close elections are becoming more and more common in the literature, concerns have been raised about their validity as a quasi-random design. If imbalances occur between winners and losers near the winning threshold, then the assumption that elections are decided randomly is violated. For example, incumbents running from the party in control of the electoral infrastructure (i.e. the incumbent party) may enjoy persistent advantages in close elections (Caughey and Sekhon, 2011; Grimmer et al., 2012) (though recent work has shown that this "strategic sorting" effect may be limited to elections in the postwar U.S. House of Representatives (Eggers et al., 2014)).

In the case of Russia in the 2000s, the main cleavages around which sorting would most likely occur also relate to the incumbent status and party affiliation of candidates. Incumbents representing Putin's United Russia (UR) party may benefit from compatriot election officials and the use of administrative resources to sway close electoral outcomes in their favor, such as clientelist machines to mobilize voters. First, I run McCrary (2008) density tests to more formally assess the validity of the assumption of continuity around the threshold. Figure 2 shows the graph of these tests for all winning candidates, Panel (a), and just UR incumbents, Panel (b). In both cases, the estimated difference is small and the p-value returned is considerably above standard levels of statistical significance. Therefore, we cannot reject the null hypothesis of no sorting around the cutoff point of 0 for these two samples.

Next, I investigate whether any sorting occurs in both the types of candidates located around the winning threshold as well as the specific firms that these individuals are connected to. For example, recent research has shown that large, state-owned firms with highly immobile workers are more likely to mobilize their workers to vote during elections in Russia (Frye, Reuter, and Szakonyi, 2014). Similarly, candidates running on behalf of these firms may be able to marshal company resources and budgets to spend on last minute campaigning or to influence officials in what are perceived to be close elections. In order to capture the causal effect of winning office, the data must satisfy the assumption that both candidates and the firms they are connected to are roughly similar across a set of baseline covariates.

To assess covariate balance among candidates and firms, I use two specifications: close margin and local linear regression. Since the treatment effect we are interested in is the effect of winning office, the forcing variable in these specifications is the overall vote margin. I estimate the difference between winners and closers using two sample sizes for the close margin (bandwidths of 2% and 3%) and two sample sizes for the local linear (bandwidths of 5% and 10%). Robust standard errors are clustered on the candidate level.

Figure 3 presents the t-statistics from a two-tailed test of the hypothesis that the difference between the comparison groups (winning versus losing candidates) for each of the 21 covariates is zero. For both specifications across the

sample sizes, we see little evidence of imbalance between winners and losers and their affiliated firms. In none of the five specifications run do any t-statistics top a value of 2 (with the exception of one model on the presence of a systemic opposition), the conventional level of statistical significance for rejecting the null hypothesis. Winning candidates are not more likely to run the type of firms most likely to participate in election campaigns nor do they have greater company resources to take advantage of to further their electoral campaigns. The 21 sets of regressions used to generate these t-tests are included in the Robustness Appendix.

5 RDD Results

First, I present the graphic illustrations of the RD treatment effect in Figure 4. I plot logged revenue (Panel A) and profit margin (Panel B) in the final year of the term against vote margin in bins of 1 percent, while limiting the interval of vote margin to elections decided by less than 10% to ease interpretation around the threshold. A LOESS regression line using the unbinned data is included, with the gray area indicating confidence intervals of 95%. The graphs are centered at the discontinuity cutoff point: a vote margin value of zero. The graphs show a positive jump for both revenue and profits around the threshold for winning elections. To calculate the size of this jump more precisely, I next turn to regression analysis.

Results from regressions on change in revenue on victory in single-member district elections, as indicated by the binary variable *District Win*, are presented in Table 1. As described above, Columns 1-2 present the results from basic OLS on the full sample of firms. The first model, run without any control variables, indicates that politically connected firms earn substantially higher revenue over the term than their firms without connections. Next I add the battery of firm-level controls (logged total assets, a dummy for state ownership, and a dummy for foreign ownership), and candidate-level controls (age, gender, ruling party membership, and incumbency) as well as year, sector, and region fixed effects to the full OLS specification. The addition of these predictors substantially reduces the effect of winning office, but the result is still statistically significant at the 0.001% level. Although we cannot claim that the point estimates from Models 1 and 2 present causal evidence, the correlation between political connections and firm performance is clearly positive in the Russian case.

Moving onto the RDD models, we see a consistent, positive, and statistically significant effect of directors winning election on firm revenue, as shown in Columns 3-9. In Columns 3 and 4, the bandwidth is narrowed to 2% and 3% respectively without covariates being included, and the point estimate on District Win indicates that firms connected to winning office enjoy an increase of revenue of between 40% and 50% as compared to firms whose candidate narrowly lost. Alternately including local-linear and cubic control functions, widening the bandwidth used, and adding the full set of firm and candidate covariates and year, sector, and region fixed effects, returns substantively similar and consistently statistically significant point estimates on the treatment variable (as shown in Columns 5-9). In all, the

coefficients on *District Win* from the varied set of RDD models range from roughly 40% to 70%, translating into a substantial effect of winning office on revenue. The range of specifications run strongly suggests that the results shown reflect a causal effect of winning office. There appear to be large revenue advantages for a firm from having its director win elected office.

Similar results emerge from the regressions on change in profit margin shown in Table 2. The order of the model specifications is identical to that from Table 1, except here the outcome variable is different. First, as above, the results from the basic OLS models on the full sample (no bandwidth restriction) indicate that politically connected firms see a somewhat higher profit margin over the term their representative holds elected office. When the battery of firm-level and candidate-level controls and year, sector and region fixed effects is added, the result increases but is only significant at the 10% level. Again, given the nature of the simple OLS regression, we cannot interpret these strong and positive correlations as reflecting a causal effect.

However, the RDD results on change in profit margin present much more persuasive causal evidence that winning office leads to more profitable firms. Though some variation in the size of the point estimates exists, the coefficient on *District Win* is statistically significant across the different model specifications and windows used. Using both the close margin approach and local linear and cubic control functions, as well as varying the bandwidth used and covariates employed returns similar point estimates on the variable of interest - the treatment variable *District Win*. The difference in profit margin over the term that a winning firm director holds office ranges from 10% to 20%. The presence of a political connection can spell the difference between an impressively profitable firm and one that barely breaks into the black.

6 Causal Mechanisms

What then is potentially driving the results on increased revenue and profit margins for politically connected firms? I next investigate several channels by which firm directors in office can help their companies. First, one set of theories argues that political connections help firms by reducing uncertainty among financiers. When markets are underdeveloped and legal institutions weak, lenders have less information about potential clients and look towards other signals of the borrowing quality or the degree of property rights protection of firms (Richter, 2010). These personal relationships can substitute for weak legal institutions in enforcing contracts (Allen, Qian, and Qian, 2005). Similarly, in a study of firms connected to parliamentarians in China, Truex (2014) finds little evidence of formal policy influence. Instead market investors interpreted membership in the National People's Congress as a "reputation boost", and lifted their share price accordingly. In Russia, signaling legitimacy in the absence of other market mechanisms may be especially importance given how important private banks to lending operations. A survey of 1,047 firms with credit access across 37 Russian

regions in 2012 showed that roughly 70% received their most recent loan from a private financial institution.[22] Having a firm director serve as a legislator may work as a powerful tool to secure financing. Connected firms in Brazil and Pakistan have been shown to benefit from greater financing (Claessens, Feijen, and Laeven, 2008; Khwaja and Mian, 2005), while companies in the U.S. with political ties pay a lower cost of capital (Houston et al., 2014).

Another theory asserts that corporate political investment opens doors to state bureaucrats who hold sway over lucrative public procurement and regulatory and tax requirement. Under this logic, the value of political ties hinges more on access to key government insiders rather acting as a signal to the market about competitiveness and earnings potential (Ang and Jia, 2014; Amore and Bennedsen, 2013; Zheng, Singh, and Mitchell, 2015). Winning a seat in parliament helps reduce the costs of acquiring information about state contracts and can even help companies influence the way bureaucrats design and conduct tenders. In Novgorod Region in 2005, a regional deputy and local firm director openly stated that winning a seat in the regional legislature would help his business achieve a necessary 'understanding' with regional and local officials.[23] That year his company signed a memorandum of cooperation with the executive branch of his regional government worth 35 million rubles ($1 million). Similarly, a primary objective for Russian firms has been to score tax breaks and lax tax enforcement from regional governments (Slinko, Yakovlev, and Zhuravskaya, 2005; Yakovlev and Zhuravskaya, 2006).[24] In Perm' Region, a regional deputy and director of a large director of a large silicate panels factory came under criminal investigation for underpaying his tax bill by 31 million rubles ($1 million) in 2003 and 2004.[25]

Measuring all channels by which political connections function, whether it be through reputation or access, is impossible. For example, data on subsidies, a key indicator of state support, is not available to the public in Russia. Codifying influence over the regulatory process, such as by lobbying for weaker regulations or the selective enforcement of existing ones, would involve drawing generalizations over the key rules affecting each industry across Russia over time, potentially a never-ending enterprise. Therefore, I am constrained to narrow in on performance-improving activities that businessperson politicians in Russia might undertake where empirical data is more readily available: taking on additional debt (evidence of signaling to private entities) and receiving state contracts and lower taxes (evidence of achieving access). To measure financial leverage, I calculate a ratio of total liabilities (long-term and short-term liabilities) to total assets (*Leverage*) also from the same database. I used a ratio of the annual profit tax paid divided by total profit before tax for each firm-year called *TaxRate*, using data from the SPARK financial database. Lastly, I combine data on all signed contracts between government organizations and individual companies from the Federal Registry of State Contracts housed at the website of the Federal Treasury and the State Procurement Portal.[26] I create

[22] Enterprise Surveys (http://www.enterprisesurveys.org), The World Bank. Russian Federation 2012 Enterprise Survey.

[23] Romanova, Lyudmila. November 11, 2006 "Revolution of the Governing" *Vedomosti Smart Money* http://www.vedomosti.ru/smartmoney/article/2006/11/07/1652 (accessed February 3, 2015)

[24] I control for variation in official rates by including region fixed effects in the model specifications.

[25] Ura.ru News Agency. September 9, 2008 "Perm Deputy Suspected of Tax Evasion. Investigators Able to Press Charges." http://ura.ru/content/perm/09-09-2008/news/43641.html (accessed February 3, 2015)

[26] Federal Register, http://reestrgk.roskazna.ru/index.php (accessed February 21, 2015). Procurement Portal,

a variable *ContractsSum* that sums and logs the total amount of state contracts that firms connected to winning and losing candidates won during the full legislative term they sought office in as measured in millions of rubles.[27] The estimation strategy used to measure the effect of winning office on the three mechanisms is identical to that used above in the regressions on changes in revenue and profit margin.

I present results from the set of regressions on effective tax rates, leverage, and state contracts in Table 3. No clear relationship emerges between political connections and effective tax rate, no matter which bandwidth or model specification is used. Likewise, political connections are not being used to increase firms' leverage. The point estimates on *District Win* are slightly negative, but none are consistent across the various model specifications. That leaves state contracts, the last mechanism for which data on firms is available. Columns 7-9 in Table 3 presents evidence that firms connected to winning candidates indeed enjoy greater opportunities concluding procurement contracts from the government. This analysis compares contracts only among firms that participated in public procurement on both sides of the threshold. The estimates from the RDD specifications show that winning firms win between 3 and 5 times more state contracts than losing firms. Though the sizes of these coefficients does not account for the entire increase in revenue as measured in Table 1, they do suggest that one way politically connected firms are able to increase both their revenue and profits is to tap into the largess of public procurement.

7 Heterogeneous Treatment Effects

The value of political connections may depend on other institutional and contextual factors as well. First, political factors may enable some businesspeople to extract more rents from government institutions. The absence of civil society makes it much harder to hold politicians accountable for their actions by applying pressure through public campaigns (Faccio, 2006). Weaker market institutions also make informal access to political power even more advantageous, since avenues such as independent courts are unavailable for firms to protect their property rights (Li et al., 2008). Alternately, where democracy has taken stronger root, politicians may be wary of abusing their public office for personal financial gain, knowing that they might the voted out of office by voters unhappy with their record of providing public goods (Gehlbach, Sonin, and Zhuravskaya, 2010). Lastly, since the early 2000s, the ruling United Russia party has built a formidable monopoly on political power across Russia, winning a majority in 86% of regional legislatures. Accordingly, we might expect that firms connected to representatives of this one ruling party to fare better than their counterparts from other parties. Similar research has found that connections to be worth more when they tie firms to the political group in power (Khwaja and Mian, 2005; Zhu and Chung, 2014).

Secondly, industrial structure has been shown to have large impacts on both how firms develop their political strategies and the dividends from seeking access (Hillman, Keim, and Schuler, 2004). In other work, I show that

http://zakupki.gov.ru/epz/main/public/home.html (accessed February 21, 2015)

[27] Because data is not available prior to 2007, I restrict analysis to candidates that ran for office beginning in 2008.

greater oligopolistic competition within a sector spurs firm directors to run for office; firms worry that similarly-sized competitors will use seats in legislatures to restrict their own access to policymaking. If that hypothesis is correct, firms run by businessperson politicians should enjoy greater spoils under two conditions: (1) when the structure of their industry is dominated by a few large firms and (2) when there are fewer rival firms also present in regional legislatures. The logic here states that the division of market power among a small set of rivals increases the potential payoffs of winning elected office for those firms that can pull off the feat.

Next, the ability of firms to reap benefits from connections may also depend on the amount of government revenue that can be diverted. Governments vary in the size of budgets to be allocated, mainly based on the level of tax revenue. In resource-rich countries such as Russia, regions with resource endowments may disproportionately enjoy increased tax revenue from extractive firms and thus larger government coffers that sweeten the pie available to the politicians with access to them. Politically connected firms may see larger dividends when the overall pie to split is larger; the additional budget funds both attract greater attention from local firms and allow for more pork to be distributed among them.

Firms also vary across a number of important dimensions that may significantly influence how access to politicians is translated into real gains. For example, author interviews with businesspeople in Tomsk region in Russia in 2014 suggested that overall the firms in construction industry was most interested in seeking elected office.[28] Lucrative state contracts and building permits are allotted mainly at the discretion of regional bureaucrats, who are known to informally grant privileges and leak information to members of parliament. As such, we might expect that firms working in this sector would reap additional profits in the form of real estate deals brokered though official state channels. Work has also shown that firms that are more vulnerable to regulatory sanction or expropriation may value access to politicians more than companies working in sectors less subject to the whims of local bureaucrats (Hellman, Jones, and Kaufmann, 2003; Chen et al., 2011). The harder it is for a firm to redeploy its assets elsewhere (i.e. the level of asset specificity), the easier it is for government officials to engage in opportunistic behavior and extract excessive rents. Evidence from interviews supports this: although politicians do not enjoy immunity, regulators and tax authorities may be less likely to pursue even action against high-profile businesspeople in office for fear of the cases being construed as being politically motivated.[29] The value of political office should be greater for those firms for which regulation is a larger barrier to their economic activity.

To examine these possible heterogeneous treatment effects, I follow the literature in splitting the subpopulation of firms to candidates located close to the threshold along axes of theoretical interest. In all of the models presented below, this subset is limited to observations within a bandwidth of a 10% vote margin, helping to narrow in on the local treatment effect identified in the above regressions while retaining adequate sample size in each subset. Next, I choose

[28] Interview with Vasiliy Semkin, deputy of Tomsk Regional Duma. Tomsk, Russia. June 11, 2014.
[29] Interview with Valeriy Otsipov, deputy of Tomsk Regional Duma. Tomsk, Russia. June 9, 2014.

various cut points to subsample this subpopulation; the cutpoints usually are the median or tercile value of dimensions chosen, unless they are categorical in nature, upon which each categorical value is used to split the sample. Lastly, all models also employ firm-level covariates such as size, sector, and ownership to ensure that interpretations of any possible interactions with region-level indicators are potentially not being biased by omitted variables as well as year effects.

Data on each of dimensions outlined above comes from public sources at the regional level. With regards to institutional quality, I use two widely used measures scored at the subnational level in Russia. The first is the Carnegie Democracy Index, developed under the Moscow Carnegie Center's Regional Monitoring Project and updated three times from the period of 2000 to 2012. The measure indexes expert assessments of ten different measures of democracy for Russia's regions on a scale of 5 to 50, with higher scores indicative of stronger democratic institutions. I also measure the percentage of seats that United Russia controlled in each regional legislature, assuming that stronger ruling party control is indicative of less political competition. Measures of competition come from a panel dataset on the universe of registered firms in Russia from 2003-2014. For each OKVED two-digit category, I compute the number of firms from the same sector as that observed have firm directors serving in the regional legislature. Economic dimensions are measured using gross regional product and the presence of natural resources (oil, natural gas, and metals) from Rosstat and the Russian Federal Agency for Subsoil Use, respectively. I code firms with immobile assets as those working in heavy industry, light industry, mining, energy/natural resources, construction, or agriculture (OKVED codes 1-44, 70). The remaining firms, such as those in trade, communications and transportation, are coded as having immobile assets.

Table 4 presents the results from the regressions using the institutional variables to subset the sample. All of the models presented use a local linear control function and the full battery of controls, with change in revenue as the dependent variable in Columns 1-2 and change in profit margin the dependent variable in Columns 3-4. Panel A subsets the sample according to low and high levels of democracy as determined by the median of the Democracy Index. Panel B similarly subsets the sample according to the median value of the percentage of legislative seats held by United Russia. These results indicate a positive relationship between the level of democracy and firm returns from political connections. In more democratic regions as well as those where the ruling party faces more political rivals, connected firms see greater profit margins and revenue. These findings suggest that although democratic development may help curb excesses in bribery and increase accountability in some areas, firm directors that can breakthrough into legislative institutions may still be able to extract rents from the government. Nonetheless, aligning oneself with the ruling United Russia party can pay big dividends (Panel C). Perhaps more consequentially, director membership in parties outside of the ruling coalition does not doom the performance of affiliated firms. Although opposition candidates can expect somewhat smaller growth in revenue, their ability to bring home earnings and profits is not diminished compared to those members of opposition parties that lost election.

On the other hand, economic competition diminishes the return on running for office for companies. As shown in Table 5, connected firms earn both greater revenue and larger profit margins when they operate in more oligopolistic industries. A firm which loses a parliamentary election but sees a rival win can incur significant costs. Lobbying and making campaign contributions are simply less efficacious if the strategy of businessperson candidacy is adopted by the other members of a single industry. Similarly, the more firm directors from a given sector that win office to a single legislature, the smaller the payoff for their affiliated firms (results in Panel B). The marketplace for rents that emerges within parliament offers reduced profit margins for participants.

Next, subsetting along other economic dimensions, we also see that political connected firms derive greater revenue and profits in wealthier regions, especially where natural resources are fueling economic growth. The top two panels in Table 6 present models which are subset on the levels of gross regional product and the presence of natural resources. The results from Panel A indicate the economic development does increase the returns on holding office. We see similar differences when the sample is split according to resource wealth in Panel B. Controlling for individual firm sector, size, and ownership, firms in resource-rich regions make roughly several times more profits than their counterparts in resource-poor regions. Panel C splits the sample based on whether the firms mainly possess mobile or immobile assets. Firms with immobile assets grow at a similar rate but their increases in profit margins are somewhat larger over the term. Political access may be helping drive down the costs of business for firms with immobile assets. Previous outlays on regulation or dealing with bureaucratic arbitrariness are no longer mandated if political ties can help clear up ties with officials.

8 Out of Sample Performance Effects

The research design used does not employ a true control group; the firm performance outcomes are compared between so-called 'winning' and 'losing' firms. Firms that did not have a director run for political office are excluded from the sample and not analyzed. This leaves open the possibility that the difference in revenue and profits between winning and losing firms is not being caused the benefits of acquiring political ties, but instead by losing firms seeing a weakening of their potential performance due to the absence of political representation. To address this question, I match firms with a director who ran for office with those that chose not to send a representative to participate in this process. The logic behind this is to identify similar companies without political ambitions and test how they fared while a potential competitor gained direct access to the regional legislature. One challenge is that firms whose directors run for office are significantly different from those that do not. Analyses of the firm-level determinants of corporate political analysis worldwide have shown that attributes such as size, recent performance, dependence on government, and ownership structure are related to the choice to seek political influence (Hillman, Keim, and Schuler, 2004; Damania, 2002; Grier, Munger, and Roberts, 1994). Though matching does not generate identification as the RD design used above, I employ

this approach to better understand the mechanism of the identified improvements for firms with director winning office.

I use the Coarsened Exact Matching (CEM) technique developed in Iacus, King, and Porro (2011). My choice of CEM to achieve balance between treatment and control (matched) groups stems from the need to exactly pair firms that operated in the same region and during the same time period as those who put a director up as a candidate for legislative office. The dataset used for the common support includes all registered firms in the SPARK database in operation from 2004-2012. I run six matching procedures, first based on two treatment categories: 1) firms with directors that contested and *won* regional legislative elections and 2) firms with directors that contested and *lost* regional legislative elections. Within each treatment category, three bandwidths are used to subset firms: 10%, 20%, and 100% (margin of victory/loss). I use a simple OLS model with CEM sample weights to return the estimated SATT, presenting results using the three bandwidths, as well as models with and without the covariates used to match the observations (the presence of state ownership, open joint-stock company status, closed joint-stock company status, and the availability of balance sheets in years corresponding to the first and last year a treated firm would have had political representation in a regional legislature). All models employ year, region, and sector fixed effects. For a detailed explanation of the method as well as the specific coarsening procedures utilized, please refer to the Robustness Appendix.

The results on revenue and profit from the specifications using the winning firms are presented in Tables 7 and 8. When compared to a matched sample of similar firms that did not have a director run for political office, those firms that did win representation see much higher revenue and profits over their term in office. The results from Table 7 indicate that firms with directors winning elections can grow by 20%-30% compared with those who didn't. Similarly, profit margins are higher for winning firms, in the range of 7%-16%.[30] On the other hand, firms with directors who lost election to regional legislatures appear to enjoy slightly larger revenue and profit margins than firms with directors that did not opt to run. In Tables 9 and 10, I present the results from specifications that use as the treatment whether a firm contested and lost an election. Such losing firms on the whole do better than their unconnected counterparts; these point estimates are only statistically significant in several of the models.

Overall, we see that a substantial portion of the effect of having a political connection on firm performance is derived from benefits accruing from winning representation to regional legislatures, and not from those contesting but losing elections being punished by the market. This exercise provides additional evidence to the claim that acquiring a political connection not only allows a firm to achieve greater revenue than its competitors, but also achieve a larger profit margin. Firms losing elections do better than their competitors down the road, but the revenue and profits they receive from such activity are markedly lower.

[30]The estimates from the matching regressions are slightly smaller than those from the RDD design. There are many large, profitable firms that never contest office at the regional level, instead relying on national-level lobbying. These firms have subsidiaries across regions, reducing the importance of focusing on one or another regional legislature. Since national-level representation is unobserved, I cannot control for these firms in the matched sample. The estimates from the RDD and matching designs that include all covariates and fixed effects are much more comparable.

9 Discussion and Concluding Remarks

To briefly summarize, using an RD design to estimate the causal effect of having an affiliated person win office, I find that politically connected firms indeed enjoy greater profit margin while an affiliate holds office. Politically connected firms enjoy increased profit margins of roughly 15% more than similar firms without a director having won political office. Similarly, these connected firms also see an increase in annual revenues of approximately 60% more that those without an affiliated person in power. Such evidence suggests powerful incentives for firms to adopt the strategy of running their director or owner in elections. Because the winning elections differs notably from making campaign contributions or lobbying, benchmarking across these strategies is difficult.[31] Cingano and Pinotti (2013) shows that firms that employ at least one official at the local level can see increases of roughly 6% in revenue and profitability in Italy. Alternately, Amore and Bennedsen (2013) report that companies with family ties to politicians can increase their profits by 100% in 'lowly corrupt' Denmark, similar to work on Thailand showing abnormal returns for connected companies of upwards of 200% (Bunkanwanicha and Wiwattanakantang, 2009).

Overall in Russia, gaining direct access to regional legislatures can make the difference between profitable and unprofitable firms. I demonstrate that the benefits of connections derive from lowered informational and regulatory costs for firms in their dealings with bureaucrats, and not from greater access to finance (Braggion and Moore, 2013). Interviews with businesspeople deputies attested to this view of political ties: companies whose directors were not able to win a seat were vulnerable to harassment from officials and the loss of market share.[32] Furthermore, deputies noted that corrupt state officials only wanted to work illegally with people they already knew from being in office; a lost election meant a closed door to key policymakers and regulators.[33] Moreover, the additional state contracts enjoyed by politically connected firms suggest that public tax dollars are being allotted not on a competitive and transparent basis, but rather according to crony and insider ties.

The finding that connected firms draw greater profits in more democratic regions challenges previous work that argues that the value of political connections is attenuated by stronger political institutions (Faccio, 2006). For example, Gehlbach, Sonin, and Zhuravskaya (2010) argue that rent-seeking businesspeople should be less likely to seek elected office when institutions are more democratic, since they fear being voted out by the median voter. Instead, the positive relationship between democracy and the value of ties shown here reveals that businesspeople may value more democratic and competitive parliaments that are able to pass real legislation. When parliaments are uncompetitive, businesspeople prefer to lobby the executive branch, which dictates the distribution of rents. When parliaments can exert influence on regulations and budgets, businesspeople instead view access to them as key, possibly by occupying the

[31] Complicating matters further is the fact that research on the value of political connections that employs accounting-based measures of organizational performance is rare. The vast majority analyzes the stock market returns for publicly traded companies.

[32] Interview with deputy of Perm' Regional Duma, October 8, 2013, Perm, Perm Krai, Russia.

[33] Interview with businessmen and former deputy of Perm' Regional Duma, October 2, 2013, Perm, Perm Krai, Russia. Interview with deputy of Tomsk Regional Duma, June 9, 2014, Tomsk, Tomsk Oblast, Russia.

seats themselves. Parliaments become forums in which business interests are negotiated and private favors exchanged, with public goods and rents accrued to the special interests represented inside. Any firms and other groups left outside these networks lose their ability to influence policy and overall societal representation is further distorted.

Often maligned in countries such as Russia as toothless and submissive, I also demonstrate that parliaments cannot be simply dismissed as institutional window-dressing: economic elites make serious investments to gain access to them and earn large payoffs as a result. In Russia, the fierce competition created by direct parliamentary elections results in candidates committing substantial resources in win. While in office, businessperson deputies wield significant formal policy influence in directing state contracts to their own firms, in addition to enjoying real information asymmetries. Parliaments allow power and resources to be shared with key actors (Boix and Svolik, 2013), even those outside of the ruling party. I thus present new evidence of how institutions can be used to monetarily co-opt opposition leaders (Reuter and Robertson, 2015), and show how greater political competition within these bodes can lead to increased rent-seeking.

The results from the paper have potential implications for policy prescriptions to reduce corruption and strengthen democratic representation. Though some features of this case study are specific to Russia, we have little reason to believe that the findings are not generalizable to other settings, especially more democratic countries. Roughly 10-15% of the parliaments studied here are located in regions classified as electorally democratic as the Philippines, for example, with genuine competition taking place (Panov and Ross, 2013; Saikkonen, 2015).[34] Businesspeople also run for political office in countries around the world, with wide cross-national differences in policies banning politicians from simultaneously working in the public and private sectors.[35] Strengthening political institutions without paying due attention to the elites that inhabit and influence them will not curb the problem of firms abusing privileged ties and access to political power. One solution would be to enforce strict regulations on the time politicians can devote to outside activities. Moreover, countries may need to require that elected officials disclose not only their personal financial assets, but submit financial documentation on the firm affiliations they and their relatives possess. In 2012, the World Bank calculated that 91% of the 176 countries it examined required members of the national parliament to disclose their earnings and assets (Rossi et al., 2012). Collecting the same information on connected firms' performance could be critical to reducing firm-level rents being misappropriated from public office. Lastly, as economic competition intensifies, businesspeople appear to view running for office as a less lucrative corporate political strategy. Policies that prevent oligopolies from dominating industry could return the focus of businesspeople to their companies, while delegating political representation to the politicians.

[34] Parliaments in regions considered more democratic are even slightly more likely to be populated by businessmen, at a rate of about 40% compared to that of 36% in the non-democratic sample.

[35] Comprehensive data on this is unavailable. A recent study on restrictions on the entry of public servants into parliaments suggested that cases were few where laws similar were passed to prevent businesspeople from running (Braendle and Stutzer, 2013). For example, in France and Italy, only managers of former state or of firms that sell to the state are not allowed to contest elections. Georgia and Serbia both require MPs to give up business managerial duties while in office. However, members of parliament can continue to serve on the board of directors in Germany, Switzerland, and the United Kingdom, among others (Gagliarducci, Nannicini, and Naticchioni, 2010)

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FIGURE 1: PERCENTAGE OF TOTAL SMD ELECTIONS DECIDED BY LESS THAN 10%, BY REGION

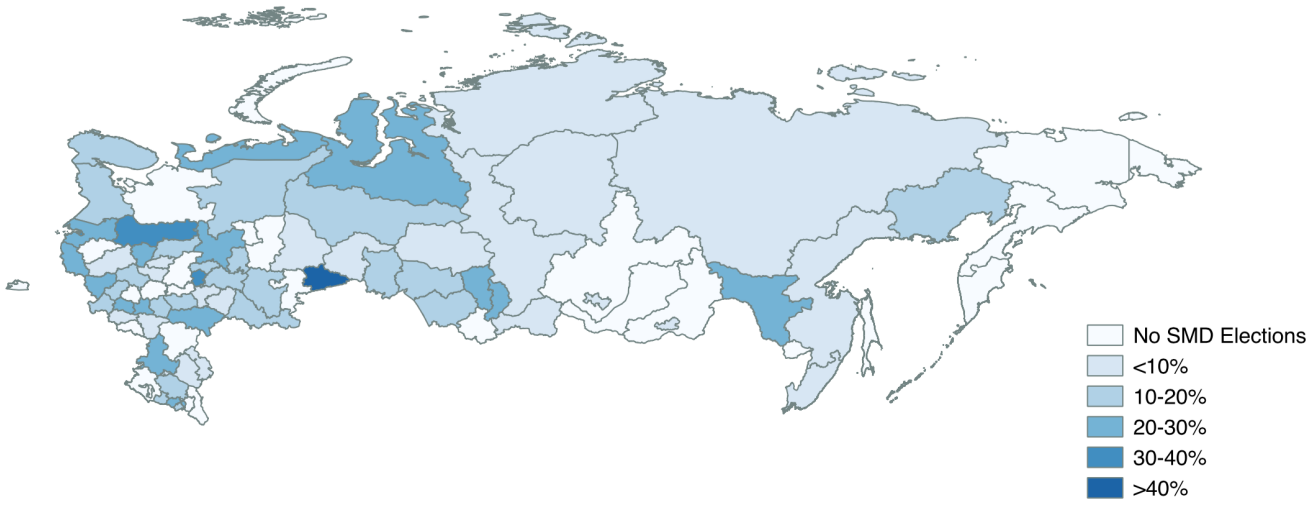
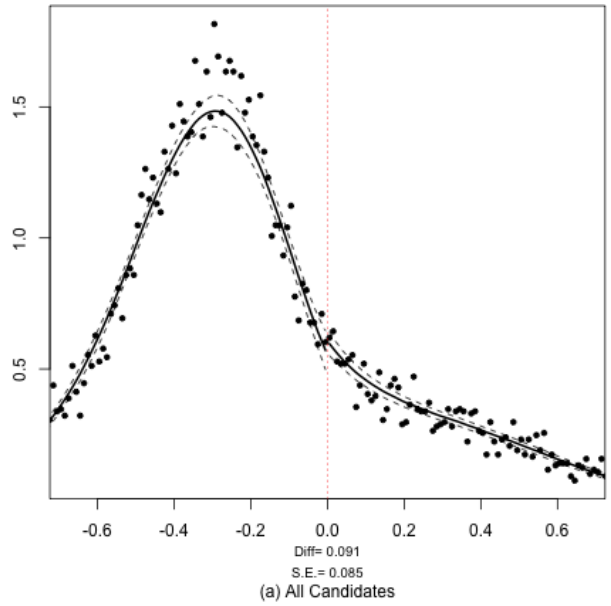
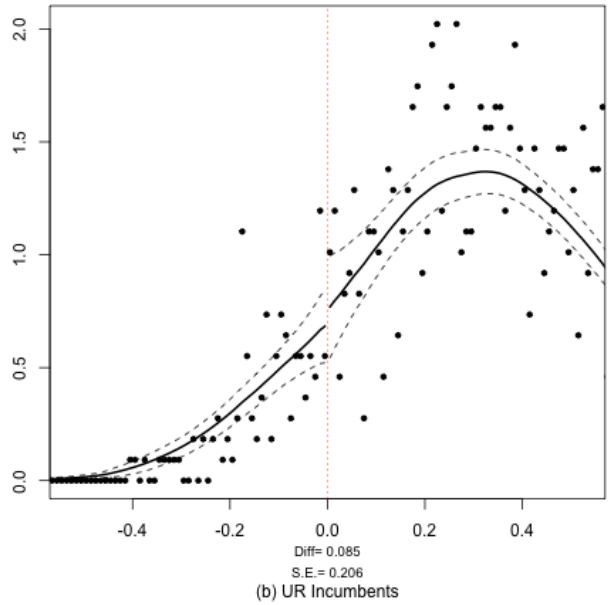


FIGURE 2: MCCRARY (2008) DENSITY TESTS - WINNING MARGIN



(a) All Candidates



(b) UR Incumbents

FIGURE 3: BALANCE STATISTICS

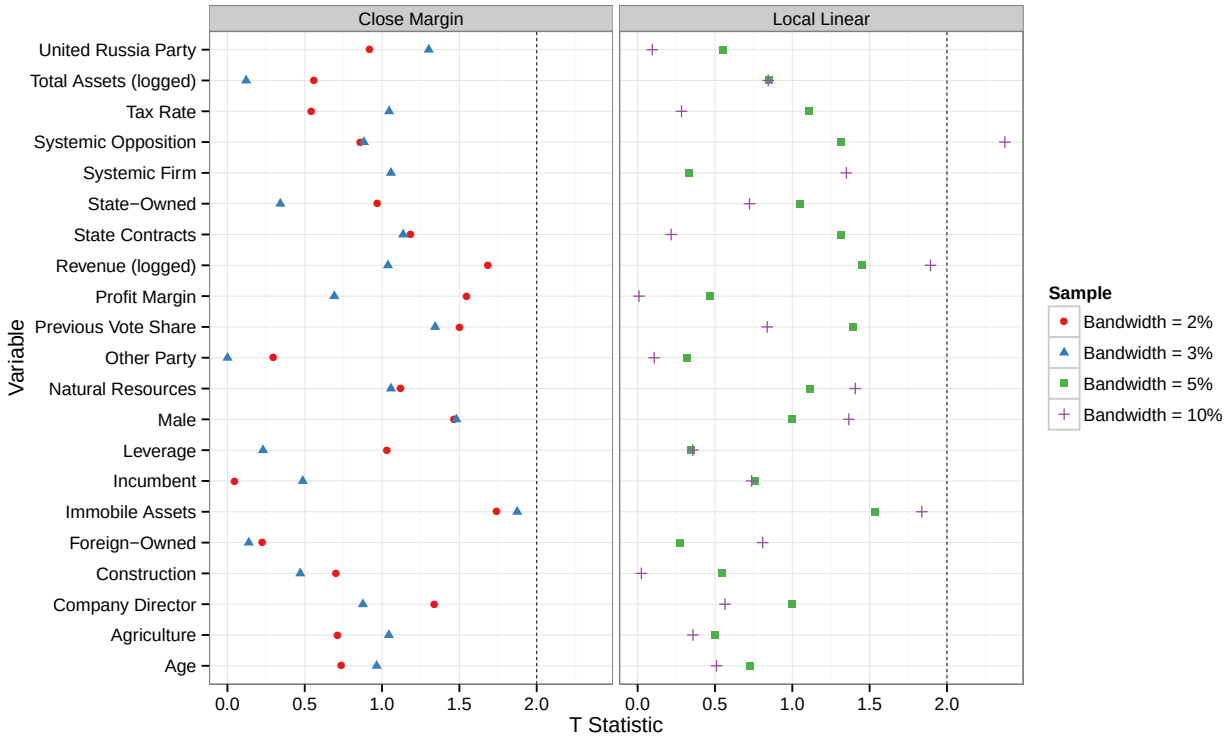
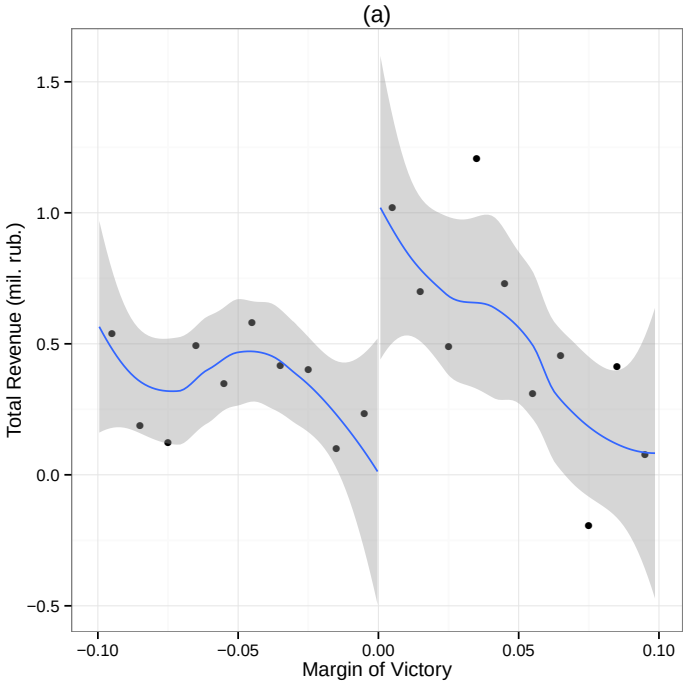


FIGURE 4: RD - GRAPHICAL ILLUSTRATIONS



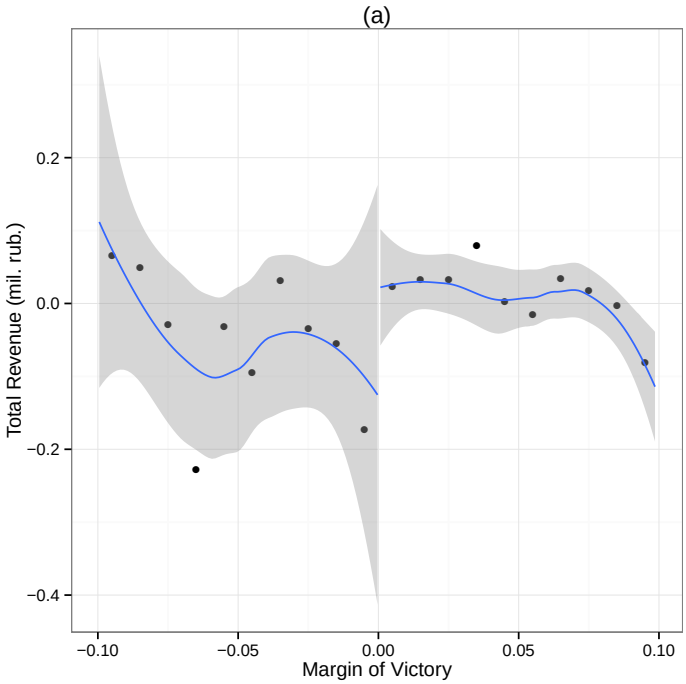


TABLE 1: POLITICAL CONNECTIONS AND FIRM REVENUE

Control Function:	None				Local Linear				Cubic
Bandwidth:	Global		2%	3%	5%		10%		20%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
District Win	0.336****	0.279****	0.533***	0.416***	0.653**	0.657**	0.586***	0.576**	0.772**
	(0.059)	(0.070)	(0.195)	(0.149)	(0.303)	(0.284)	(0.190)	(0.231)	(0.325)
Bandwidth:	0.8	0.8	0.02	0.03	0.05	0.05	0.1	0.1	0.2
Firm and cand. covariates:	No	Yes	No	No	No	Yes	No	Yes	Yes
Year, Region, Sector FE:	No	Yes	No	No	No	No	No	Yes	Yes
Observations	2,557	2,557	89	139	211	211	445	445	950

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level as well as the lagged value for the outcome as a covariate. Columns 1-2 present OLS results using the full sample, with and without firm and candidate controls and year, sector and region fixed effects. Columns 3-4 also use OLS specifications, but restrict the bandwidth to close winning vote margins of 2% and 3% respectively. Columns 5-8 are RD specifications with a local-linear control for candidate winning vote margin, with and without controls and fixed effects. The bandwidth used in Columns 5-7 is a 10% margin of victory, while Column 8 uses a 20% to expand the number of observations as to not introduce bias into the estimation with the cubic polynomial. Firm and candidate controls include age, gender, incumbent status, membership in the ruling United Russia party, a binary indicator for state ownership, a binary indicator for foreign ownership, and logged total assets in the year prior to the election. Year fixed effects capture the year the outcome variables are measured, region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category.

TABLE 2: POLITICAL CONNECTIONS AND FIRM PROFIT MARGIN

Control Function:	None				Local Linear				Cubic
Bandwidth:	Global		2%	3%	5%		10%		20%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
District Win	0.010	0.035*	0.145**	0.105**	0.173**	0.172**	0.132***	0.128*	0.201**
	(0.018)	(0.019)	(0.067)	(0.045)	(0.080)	(0.082)	(0.047)	(0.066)	(0.101)
Bandwidth:	0.8	0.8	0.02	0.03	0.05	0.05	0.1	0.1	0.2
Firm and cand. covariates:	No	Yes	No	No	No	Yes	No	Yes	Yes
Year, Region, Sector FE:	No	Yes	No	No	No	No	No	Yes	Yes
Observations	2,540	2,540	88	138	210	210	442	442	944

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level. Columns 1-2 present OLS results using the full sample, with and without firm and candidate controls and year, sector and region fixed effects. Columns 3-4 also use OLS specifications, but restrict the bandwidth to close winning vote margins of 2% and 3% respectively. Columns 5-8 are RD specifications with a local-linear control for candidate winning vote margin, with and without controls and fixed effects. The bandwidth used in Columns 5-7 is a 10% margin of victory, while Column 8 uses a 20% to expand the number of observations as to not introduce bias into the estimation with the cubic polynomial. Firm and candidate controls include age, gender, incumbent status, membership in the ruling United Russia party, a binary indicator for state ownership, a binary indicator for foreign ownership, and logged total assets in the year prior to the election. Year fixed effects capture the year the outcome variables are measured, region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category.

TABLE 3: POLITICAL CONNECTIONS AND UNDERLYING MECHANISMS

Dependent Variable:	Leverage			Tax Rate			State Contracts		
Control Function:	Local Linear			Local Linear			Local Linear		
Bandwidth:	5%	5%	10%	5%	5%	10%	5%	5%	10%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
District Win	−0.003	−0.040	0.018	0.121*	0.115	0.044	5.177**	4.709*	2.572
	(0.108)	(0.112)	(0.104)	(0.066)	(0.070)	(0.048)	(2.445)	(2.373)	(1.670)
Bandwidth:	0.05	0.05	0.1	0.05	0.05	0.1	0.05	0.05	0.1
Firm and cand. covariates:	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year, Region, Sector FE:	No	No	Yes	No	No	Yes	No	No	No
Observations	225	225	483	117	117	230	39	39	79

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level as well as the lagged value for the outcome as a covariate. Columns 1-3 use firm leverage as the outcome variable; Columns 4-6 use tax rate; Columns 7-10 use total state contracts (logged). All models include a local linear control function, and use bandwidths of either 5% or 10%. Firm and candidate controls include age, gender, incumbent status, membership in the ruling United Russia party, a binary indicator for state ownership, a binary indicator for foreign ownership, and logged total assets in the year of the election. Year fixed effects capture the year the outcome variables are measured, region fixed effects capture the region where the election was held, and sector fixed effects capture a firms two-digit OKVED economic category.

TABLE 4: HETEROGENEOUS TREATMENT EFFECTS - INSTITUTIONAL VARIABLES

Dependent Variable:	Change in Revenue		Change in Profit Margin	
Panel A: Sample Split at Median of Democracy Score				
Samples:	Low Dem.	High Dem.	Low Dem.	High Dem.
	(1)	(2)	(3)	(4)
District Win	0.535	0.715***	0.079*	0.218**
	(0.344)	(0.269)	(0.041)	(0.090)
Bandwidth:	0.1	0.1	0.1	0.1
Observations	199	246	198	244
Panel B: Sample Split at Median of UR Control of Parliament				
Samples:	Low UR Control	High UR Control	Low UR Control	High UR Control
District Win	0.584**	0.173	0.196***	−0.018
	(0.242)	(0.423)	(0.062)	(0.068)
Bandwidth:	0.1	0.1	0.1	0.1
Observations	322	123	320	122
Panel C: Sample Split at Membership in UR Party				
Samples:	Non-UR	UR	Non-UR	UR
District Win	0.561**	0.791	0.134**	0.180
	(0.250)	(0.480)	(0.067)	(0.122)
Bandwidth:	0.1	0.1	0.1	0.1
Observations	280	165	278	164

*p<0.1; **p<0.05; ***p<0.01

This table displays subgroup RD treatment effects of winning office using a bandwidth of 5% vote share and a local-linear control function. Panel A presents results from subsetting by the median democracy score in the region using the Carnegie Democracy Index. Panel B presents results from subsetting on the median of the number of seats the United Russia controlled in the parliament. Panel C subsets on whether a candidate was a member of the United Russia party. All models include firm-level and candidate-level covariates as well as sector and year fixed effects. Robust standard errors clustered on the candidate level.

TABLE 5: HETEROGENEOUS TREATMENT EFFECTS - COMPETITION-RELATED VARIABLES

Dependent Variable:	Change in Revenue		Change in Profit Margin	
Panel A: Sample Split at Terciles of Oligopolistic Concentration				
Samples:	Low Olig.	High Olig.	Low Olig.	High Olig.
	(1)	(2)	(3)	(4)
District Win	0.660**	0.376	0.064	0.172**
	(0.307)	(0.238)	(0.047)	(0.085)
Bandwidth:	0.1	0.1	0.1	0.1
Observations	215	230	214	228
Panel B: Sample Split at Terciles of Sectoral Representation in Parliament				
Samples:	Low Sec.	High Sec.	Low Sec.	High Sec.
District Win	0.624*	0.606**	0.220*	0.076*
	(0.316)	(0.290)	(0.118)	(0.044)
Bandwidth:	0.1	0.1	0.1	0.1
Observations	181	264	180	262

*p<0.1; **p<0.05; ***p<0.01

This table displays subgroup RD treatment effects of winning office using a bandwidth of 10% vote share and a local-linear control function. Panel A presents results from subsetting by terciles of the average percentage of total turnover that the top four firms comprise in the observed firm'sector. Panel B presents results from subsetting on terciles of the number of politicians elected to a regional parliament representing a observed firm's sector. All models include firm-level and candidate-level covariates as well as sector and year fixed effects. Robust standard errors clustered on the candidate level.

TABLE 6: HETEROGENEOUS TREATMENT EFFECTS - ECONOMIC AND SECTORAL VARIABLES

Dependent Variable:	Change in Revenue		Change in Profit Margin	
Panel A: Sample Split at Median of Regional GRP Score				
Samples:	Low GRP	High GRP	Low GRP	High GRP
	(1)	(2)	(3)	(4)
District Win	0.568**	0.688	0.070*	0.307*
	(0.271)	(0.425)	(0.038)	(0.162)
Bandwidth:	0.1	0.1	0.1	0.1
Observations	269	176	267	175
Panel B: Sample Split according to Presence of Natural Resources				
Samples:	No Resources	Resources	No Resources	Resources
District Win	0.587**	1.103**	0.094**	0.349*
	(0.253)	(0.469)	(0.045)	(0.206)
Bandwidth:	0.1	0.1	0.1	0.1
Observations	307	138	304	138
	(1)	(2)	(3)	(4)
Panel C: Sample Split at Firms with Immobile Assets				
Samples:	Mobile	Immobile	Mobile	Immobile
District Win	0.662*	0.526*	0.107	0.152**
	(0.373)	(0.317)	(0.098)	(0.068)
Bandwidth:	0.1	0.1	0.1	0.1
Observations	206	239	204	238

*p<0.1; **p<0.05; ***p<0.01

This table displays subgroup RD treatment effects of winning office using a bandwidth of 10% vote share and a local-linear control function. Panel A presents results from subsetting by the median of the level of Gross Regional Product in the region, while Panel B presents results from subsetting on whether the region possessed natural resources (oil, gas, metals, or diamonds). Panel C presents results from subsetting on whether a firm is coded to have immobile assets. All models include firm-level and candidate-level covariates. Panels A and B include sector and year fixed effects, while Panel C includes region and year fixed effects. Robust standard errors clustered on the candidate level.

TABLE 7: MATCHING: WINNING FIRMS AND FIRM TOTAL REVENUE

Bandwidth Cutoff:	0.1	0.1	0.2	0.2	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Won Election	0.23**	0.27***	0.35***	0.37***	0.25***	0.31***
	(0.09)	(0.10)	(0.07)	(0.07)	(0.04)	(0.04)
Matching Covariates:	No	Yes	No	Yes	No	Yes
Region, Sector FE:	Yes	Yes	Yes	Yes	Yes	Yes
Treated Observations	208	208	435	435	1419	1419
$\mathcal{L}1$	0.37	0.37	0.36	0.36	0.3	0.3
Observations	18,972	18,972	36,090	36,090	93,851	93,851
R^2	0.19	0.08	0.23	0.12	0.17	0.07

*p<0.1; **p<0.05; ***p<0.01

Results from dataset matched using Coarsened Exact Matching (CEM). Variables used to match include total assets (logged), state ownership, and legal status. Total assets is measured in the year prior to that when director of the treated firm ran for office. Revenue is measured in the final year that the director of the treated firm would have left office. Region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category. Columns 1-2 match only on firms that won by less than 10% margin; Columns 3-4 match only on firms that won by less than 20% margin; Columns 5-7 match on all firms that won.

TABLE 8: MATCHING: WINNING FIRMS AND FIRM NET PROFIT

Bandwidth Cutoff:	0.1	0.1	0.2	0.2	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Won Election	0.16***	0.16***	0.16***	0.16***	0.08***	0.07***
	(0.06)	(0.06)	(0.04)	(0.04)	(0.03)	(0.03)
Matching Covariates:	No	Yes	No	Yes	No	Yes
Region, Sector FE:	Yes	Yes	Yes	Yes	Yes	Yes
Treated Observations	208	208	435	435	1419	1419
$\mathcal{L}1$	0.37	0.37	0.36	0.36	0.3	0.3
Observations	18,972	18,972	36,090	36,090	93,851	93,851
R^2	0.14	0.14	0.08	0.07	0.07	0.07

*p<0.1; **p<0.05; ***p<0.01

Results from dataset matched using Coarsened Exact Matching (CEM). Variables used to match include total assets (logged), state ownership, and legal status. Total assets is measured in the year prior to that when director of the treated firm ran for office. Revenue is measured in the final year that the director of the treated firm would have left office. Region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category. Columns 1-2 match only on firms that won by less than 10% margin; Columns 3-4 match only on firms that won by less than 20% margin; Columns 5-7 match on all firms that won.

TABLE 9: MATCHING: LOSING FIRMS AND FIRM TOTAL REVENUE

Bandwidth Cutoff:	0.1	0.1	0.2	0.2	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Lost Election	0.21**	0.21**	0.15**	0.17***	0.04	0.06
	(0.09)	(0.09)	(0.06)	(0.07)	(0.04)	(0.04)
Matching Covariates:	No	Yes	No	Yes	No	Yes
Region, Sector FE:	Yes	Yes	Yes	Yes	Yes	Yes
Treated Observations	205	205	463	463	1107	1107
$\mathcal{L}1$	0.39	0.39	0.37	0.37	0.33	0.33
Observations	16,469	16,469	38,226	38,226	93,184	93,184
R^2	0.16	0.13	0.16	0.12	0.15	0.11

*p<0.1; **p<0.05; ***p<0.01

Results from dataset matched using Coarsened Exact Matching (CEM). Variables used to match include total assets (logged), state ownership, and legal status. Total assets is measured in the year prior to that when director of the treated firm ran for office. Revenue is measured in the final year that the director of the treated firm would have left office. Region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category. Columns 1-2 match only on firms that lost by less than 10% margin; Columns 3-4 match only on firms that lost by less than 20% margin; Columns 5-7 match on all firms that lost.

TABLE 10: MATCHING: LOSING FIRMS AND FIRM NET PROFIT

Bandwidth Cutoff:	0.1	0.1	0.2	0.2	1	1
	(1)	(2)	(3)	(4)	(5)	(6)
Firm Lost Election	0.10	0.10	0.14**	0.14**	0.09***	0.09***
	(0.07)	(0.07)	(0.04)	(0.04)	(0.03)	(0.03)
Matching Covariates:	No	Yes	No	Yes	No	Yes
Region, Sector FE:	Yes	Yes	Yes	Yes	Yes	Yes
Treated Observations	205	205	463	463	1107	1107
$\mathcal{L}1$	0.39	0.39	0.37	0.37	0.33	0.33
Observations	16,469	16,469	38,226	38,226	93,184	93,184
R^2	0.09	0.08	0.12	0.11	0.06	0.06

*p<0.1; **p<0.05; ***p<0.01

Results from dataset matched using Coarsened Exact Matching (CEM). Variables used to match include total assets (logged), state ownership, and legal status. Total assets is measured in the year prior to that when director of the treated firm ran for office. Revenue is measured in the final year that the director of the treated firm would have left office. Region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category. Columns 1-2 match only on firms that lost by less than 10% margin; Columns 3-4 match only on firms that lost by less than 20% margin; Columns 5-7 match on all firms that lost

A Appendix

A.1 Placebo Checks

- Tables A1, A2, and A3 present the results of placebo regressions on the baseline covariates used to assess balance between the treatment and the control group in the regression discontinuity design used in this paper. The aim here is determine whether there is balance between observations located near the threshold needed to win an election. By running placebo models on other variables measured at the time of assignment to treatment, we can check that treatment status is being more or less randomly assigned. The t-statistics derived from these models (as well as from other specifications) are those used to generate Figure 3 (Balance Statistics).

- The regressions exclude other covariates, including year and region fixed effects, and two specifications and sample sizes are presented. In Panel A, the sample is restricted to elections within a 2% bandwidth, that is, to elections that were decided by a winning margin of less than 2% and no control function is included. In Panel B, the sample is restricted to elections within a 5% bandwidth, or to elections that were decided by a winning margin of less than 5%, and a local linear control function is included.

- The results show that the treatment of winning a close election is not correlated with any of the other baseline covariates (measured during the year prior to the election). We do not observe any sorting either at the candidate level (using various characteristics of the candidates vying for elections) nor at the firm level (using various firm-level financial and descriptive indicators). We can thus be confident that using the Regression Discontinuity Design based on close elections is appropriate for the Russian case, as elections are truly competitive and victory appears to be as-if randomly assigned among a large sample of close races.

TABLE A1: PLACEBO CHECKS - CANDIDATE COVARIATES

Outcome:	Age	Male	Incumbent (Any)	United Russia Party	Systemic Opposition	Other Party	Company Director	Previous Vote Share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Close Margin RD with bandwidth of 2%								
District Win	0.016	−0.056	−0.003	0.050	0.038	0.009	0.066	0.057
	(0.022)	(0.038)	(0.053)	(0.054)	(0.045)	(0.030)	(0.049)	(0.039)
Constant	3.860***	0.899***	0.323***	0.316***	0.171***	0.070***	0.222***	0.340***
	(0.016)	(0.024)	(0.037)	(0.037)	(0.030)	(0.020)	(0.033)	(0.024)
Observations	311	311	311	311	311	311	311	84
Panel B: Local linear RD with bandwidth of 5%								
District Win	0.020	−0.048	−0.050	0.037	0.073	−0.013	0.062	0.068
	(0.028)	(0.048)	(0.067)	(0.068)	(0.055)	(0.038)	(0.061)	(0.047)
Constant	3.858***	0.899***	0.354***	0.310***	0.136***	0.085***	0.210***	0.337***
	(0.021)	(0.032)	(0.046)	(0.047)	(0.036)	(0.026)	(0.040)	(0.031)
Observations	736	736	736	736	736	736	736	190

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level. Panel A restricts the sample to observations within a 2% bandwidth and does not use a control function. Panel B restricts to 5%.

TABLE A2: PLACEBO CHECKS - FIRM COVARIATES (1)

Outcome:	Foreign-Owned	State-Owned	Systemic Firm	Agriculture	Construction	Natural Resources	Immobile Assets
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Close Margin RD with bandwidth of 2%							
District Win	−0.010	0.065	0.000	−0.050	0.045	−0.025	−0.180
	(0.045)	(0.066)	(0.000)	(0.070)	(0.063)	(0.025)	(0.109)
Constant	0.050	0.075*	0.000	0.150***	0.075*	0.025	0.700***
	(0.035)	(0.043)	(0.000)	(0.055)	(0.041)	(0.025)	(0.075)
Observations	90	90	90	90	90	90	90
Panel B: Local linear RD with bandwidth of 5%							
District Win	−0.016	0.080	0.009	−0.042	0.043	−0.020	−0.211
	(0.056)	(0.087)	(0.020)	(0.095)	(0.085)	(0.021)	(0.145)
Constant	0.035	0.092*	−0.009	0.190***	0.038	0.020	0.708***
	(0.047)	(0.054)	(0.020)	(0.073)	(0.067)	(0.021)	(0.104)
Observations	232	232	232	232	232	232	232

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level. Panel A restricts the sample to observations within a 2% bandwidth and does not use a control function. Panel B restricts to 5%.

TABLE A3: PLACEBO CHECKS - FIRM COVARIATES (2)

Outcome:	Total Assets (logged)	Revenue (logged)	Profit Margin	Leverage	Tax Rate	State Contracts
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Close Margin RD with bandwidth of 2%						
District Win	−0.270	−0.753	−0.051	0.117	−0.021	−5,722,726.000
	(0.514)	(0.466)	(0.033)	(0.104)	(0.039)	(5,424,817.000)
Constant	11.000***	11.604***	0.036	0.553***	0.264***	6,147,074.000
	(0.371)	(0.318)	(0.025)	(0.048)	(0.034)	(5,418,501.000)
Observations	90	89	89	89	62	90
Panel B: Local linear RD with bandwidth of 5%						
District Win	−0.617	−0.915	−0.032	0.071	−0.068	−14,719,396.000
	(0.681)	(0.632)	(0.054)	(0.145)	(0.055)	(10,167,475.000)
Constant	11.361***	11.690***	0.038	0.477***	0.264***	5,276,528.000
	(0.475)	(0.445)	(0.038)	(0.077)	(0.046)	(4,616,992.000)
Observations	232	217	218	228	151	232

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level. Panel A restricts the sample to observations within a 2% bandwidth and does not use a control function. Panel B restricts to 5%.

A.2 Determinants of Close Elections

- Table A4 presents the results from a series of models investigating possible differences between so-called 'close' (or competitive) elections and other elections determined by a much larger margin of votes. Key to this discussion is that close elections may not be representative of the full sample of elections in the Russian context in meaningful ways. Therefore the local average treatment effect identified through the RD design may be credible for the subpopulation of firms located near the threshold, but it may not reflect the overall advantages accrued to firms that are located farther from or at the extremes on the scale of vote margin.

- To examine this possibility, I ran models that used varying definitions of 'close' elections as a binary dependent variable. In Model 1, an election was determined close (coded as 1) if the winner won by less than 5% of the total vote, whereas in Models 2, 3, and 4, the dependent variables are coded as 1 if the margin was less than 10%, 20%, and 35% respectively. Several explanatory variables are used. First, the total number of candidates is calculated in Number of Candidates. Next, the binary variable UR Victory takes a 1 if a candidate affiliated with the ruling United Russia party won; this indicator reflects the possibility that these elections were not truly competitive if United Russia candidates were more likely to win them. Next, the percentage of male candidates running and average age are captured with the Male Candidate and Average Candidate Age variables. The binary variable Incumbent Ran takes a 1 if any incumbent from the previous parliamentary convocation ran in the election. Lastly, the number of voters on the voter list is logged and measured in Number of Voters.

- Because of the binary dependent variables, I use logit models with robust standard errors clustered on the regional level in all specifications. Several interesting results emerge. First, as expected, a greater number of candidates running is associated with a greater likelihood of an election being competitive. This is intuitively plausible, seeing that the presence of multiple candidates can eat into the vote share of the potential winner and spread votes between more viable politicians. Secondly, politicians from the ruling United Russia party are less likely to win in competitive elections. The fact that close elections are not UR strongholds, and UR politicians do not have any disproportionate advantage in winning these races, provides additional support to the validity of using the close elections RD design in the Russian context. However, besides the results for these two variables, no other point estimates are statistically significant. Close elections look remarkably similar to non-competitive ones along a number of important dimensions, which should increase our ability to make generalizations about the local average treatment effect.

TABLE A4: DETERMINANTS OF COMPETITIVE ELECTIONS

	Close 5%	Close 10%	Close 20%	Close 35%
	(1)	(2)	(3)	(4)
Number of Candidates	0.243***	0.281***	0.357***	0.462***
	(0.061)	(0.061)	(0.061)	(0.074)
UR Victory	−1.685***	−1.629***	−1.795***	−1.672***
	(0.143)	(0.133)	(0.102)	(0.109)
Male Candidate %	−0.419	−0.253	−0.304	−0.475*
	(0.331)	(0.284)	(0.227)	(0.266)
Average Candidate Age	0.108	0.078	0.157	0.230
	(0.452)	(0.386)	(0.391)	(0.358)
Incumbent Ran	−0.047	−0.075	−0.108	0.028
	(0.126)	(0.103)	(0.101)	(0.112)
Midterm Election	−0.034	−0.094	−0.125	−0.158
	(0.270)	(0.249)	(0.207)	(0.189)
Number of Voters (logged)	−0.130	−0.079	−0.076	−0.128
	(0.097)	(0.116)	(0.109)	(0.124)
Constant	−0.487	−0.361	0.167	1.105
	(1.915)	(1.940)	(2.003)	(2.090)

*p<0.1; **p<0.05; ***p<0.01

Logit models used for binary outcomes. All models use robust standard errors clustered on the region level. Dependent variables reflect different cutoffs for defining competitive elections.

A.3 Multiple Thresholds

- An additional robustness check is to test how the main specifications perform using multiple values of bandwidths. This approach helps identify any dependence on a specific sample or threshold that could be driving the results. Figures A5 and A6 show the estimates for two specifications, the local-linear model and the close margin model, with the solid line depicting the treatment effect and 95% confidence interval shown in the shaded area. The effects are estimated at thresholds in the range of a 1% to a 10% margin of victory in 0.5% intervals. In the models using the smaller bandwidths, the effects are somewhat larger and noisier, but become more stable and consistently significant (as indicated by the 95% confidence interval not intersecting with the 0 axis) as the sample size grows. The figures offer additional support to the result that a firm director winning election office increases revenue and profitability for his or her affiliated firms.

FIGURE A5: MULTIPLE THRESHOLDS - TOTAL REVENUE

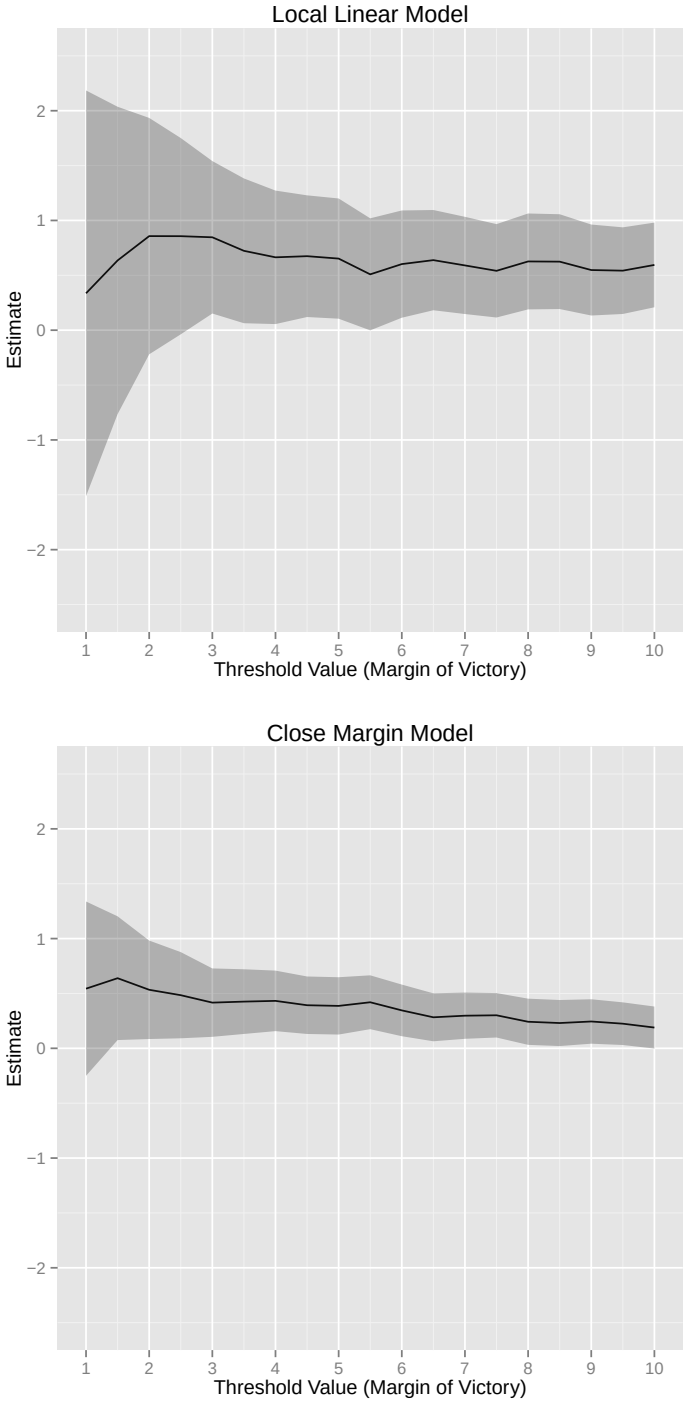
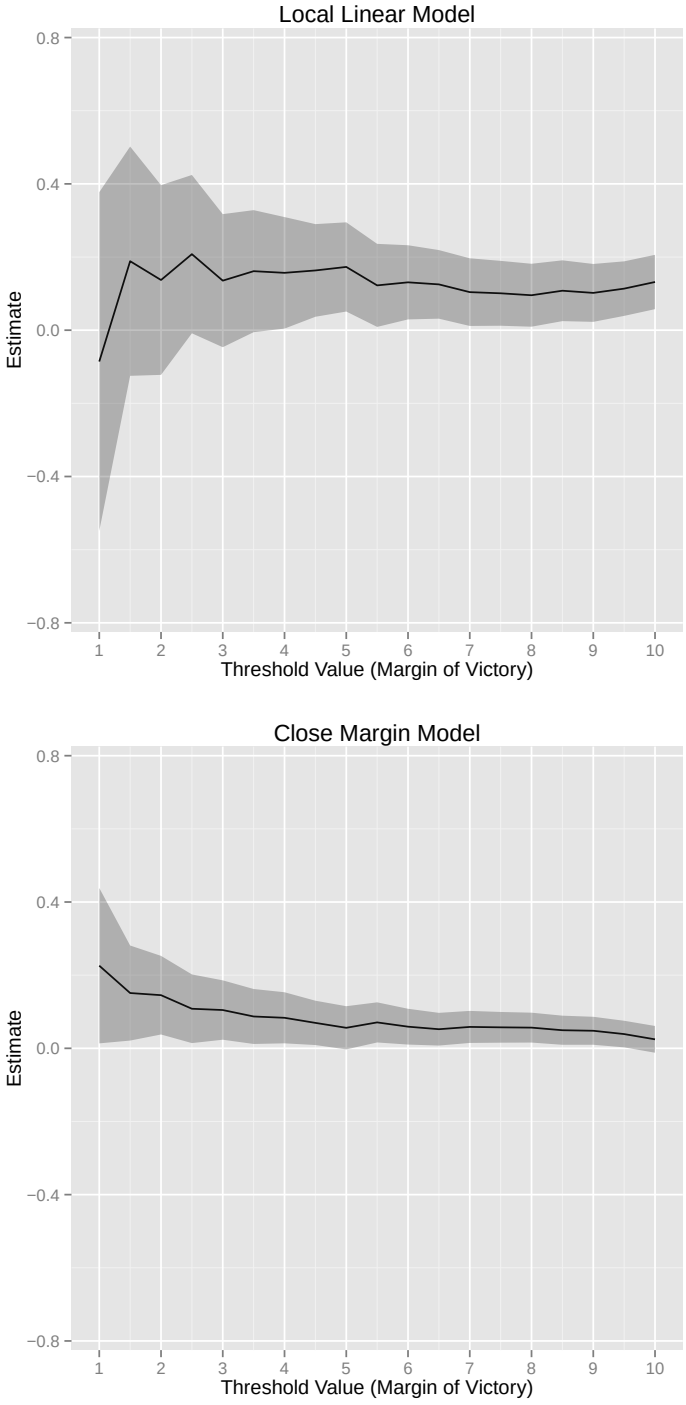


FIGURE A6: MULTIPLE THRESHOLDS - CHANGE IN PROFIT MARGIN



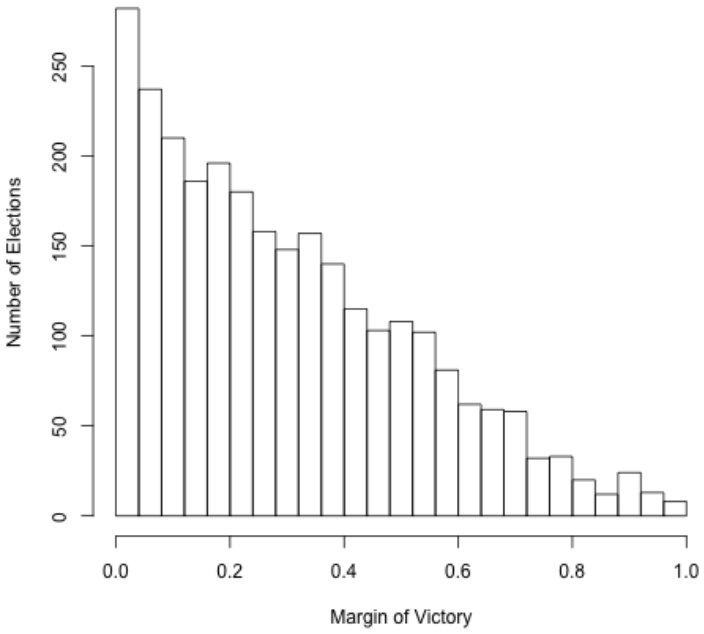
A.4 Data Description

- Table A5 presents Summary Statistics for all of the variables used in the regressions in the main text and Appendix.
- Figure A7 is a histogram of the margin of victory for candidates across SMD elections.

TABLE A5: SUMMARY STATISTICS

Statistic	N	Mean	St. Dev.	Min	Max
Age	12,113	3.810	0.252	3.045	4.394
Male	12,113	0.862	0.345	0	1
Incumbent	12,113	0.157	0.363	0	1
United Russia Party	12,113	0.195	0.396	0	1
Systemic Opposition	12,113	0.321	0.467	0	1
Other Party	12,113	0.083	0.276	0	1
Company Director	12,113	0.163	0.369	0	1
Previous Vote Share	2,152	0.326	0.214	0.000	0.956
Foreign-Owned	2,720	0.034	0.182	0	1
State-Owned	2,720	0.063	0.243	0	1
Systemic Firm	2,720	0.010	0.099	0	1
Agriculture	2,720	0.128	0.334	0	1
Construction	2,720	0.088	0.283	0	1
Natural Resources	2,720	0.032	0.175	0	1
Immobile Assets	2,720	0.629	0.483	0	1
Total Assets (logged), Start Year	2,720	11.140	2.368	2.079	19.916
Revenue (logged), Start Year	2,714	11.250	2.341	1.609	19.691
Profit Margin, Start Year	2,720	−0.010	0.479	−9.821	0.997
Leverage, Start Year	2,716	0.634	0.543	0.00002	9.364
Tax Rate, Start Year	1,859	0.262	0.246	0.00004	1.979
State Contracts (logged), Start Year	225	15.758	2.744	10.278	23.502
Total Assets (logged), End Year	2,720	11.688	2.480	1.099	20.295
Revenue (logged), End Year	2,628	11.736	2.427	1.099	20.263
Profit Margin, End Year	2,621	−0.033	0.440	−7.688	0.909
Leverage, End Year	2,683	0.669	0.577	0.0001	9.360
Tax Rate, End Year	1,656	0.214	0.211	0.00002	1.976
State Contracts (logged), End Year	1,042	16.255	3.255	4.197	25.549
Democracy Level (Region)	2,719	30.178	5.699	17	42
Percentage of UR Seats	2,720	0.614	0.186	0.172	0.974
Regional GRP (logged)	2,720	12.136	1.081	8.130	15.779
Natural Resources in Region	2,720	0.323	0.468	0	1
Sectoral Concentration	2,720	0.486	0.249	0.086	1.000
Number of Deputies from Sector	2,720	5.024	4.184	0	27

FIGURE A7: CANDIDATE MARGIN OF VICTORY (%)



A.5 Robustness Checks

- Tables A6 and A7 present regressions examining the effect of winning office on changes in revenue and profit margin respectively in an identical format to those in main tables in the paper, except only candidates that served as director or deputy director of their firms are included. The main results are robust to this restricting of the sample, though some of the standard errors are larger due to the sample size being reduced.

- Tables A8 and A9 instead restrict the sample to candidates that only ran in the plurality races. This could be a concern given that in the main regressions, I dropped all candidates which lost in the plurality races but took a spot through the party list system. We see that the point estimates on change in revenue are somewhat larger and still statistically significant. Similarly, restricting to only SMD candidates robust results on change in profit margin with this reduced sample.

TABLE A6: POLITICAL CONNECTIONS AND FIRM REVENUE, ONLY DIRECTORS

Control Function:	None				Local Linear				Cubic
Bandwidth:	Global		2%	3%	5%		10%		20%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
District Win	0.313****	0.232***	0.421*	0.338*	0.483	0.537*	0.509**	0.522*	0.612*
	(0.069)	(0.083)	(0.222)	(0.172)	(0.334)	(0.315)	(0.210)	(0.291)	(0.354)
Bandwidth:	0.8	0.8	0.02	0.03	0.05	0.05	0.1	0.1	0.2
Firm and cand. covariates:	No	Yes	No	No	No	Yes	No	Yes	Yes
Year, Region, Sector FE:	No	Yes	No	No	No	No	No	Yes	Yes
Observations	2,016	2,016	75	112	170	170	362	362	787

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level as well as the lagged value for the outcome as a covariate. Columns 1-2 present OLS results using the full sample, with and without firm and candidate controls and year, sector and region fixed effects. Columns 3-4 also use OLS specifications, but restrict the bandwidth to close winning vote margins of 2% and 3% respectively. Columns 5-8 are RD specifications with a local-linear control for candidate winning vote margin, with and without controls and fixed effects. The bandwidth used in Columns 5-7 is a 10% margin of victory, while Column 8 uses a 20% to expand the number of observations as to not introduce bias into the estimation with the cubic polynomial. Firm and candidate controls include age, gender, incumbent status, membership in the ruling United Russia party, a binary indicator for state ownership, a binary indicator for foreign ownership, and logged total assets in the year prior to the election. Year fixed effects capture the year the outcome variables are measured, region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category.

TABLE A7: POLITICAL CONNECTIONS AND FIRM PROFIT, ONLY DIRECTORS

Control Function:	None				Local Linear				Cubic
Bandwidth:	Global		2%	3%	5%		10%		20%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
District Win	0.005	0.019	0.161**	0.121**	0.192**	0.192**	0.143***	0.129*	0.207*
	(0.022)	(0.022)	(0.075)	(0.054)	(0.090)	(0.094)	(0.053)	(0.076)	(0.115)
Bandwidth:	0.8	0.8	0.02	0.03	0.05	0.05	0.1	0.1	0.2
Firm and cand. covariates:	No	Yes	No	No	No	Yes	No	Yes	Yes
Year, Region, Sector FE:	No	Yes	No	No	No	No	No	Yes	Yes
Observations	2,001	2,001	75	112	170	170	361	361	783

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level. Columns 1-2 present OLS results using the full sample, with and without firm and candidate controls and year, sector and region fixed effects. Columns 3-4 also use OLS specifications, but restrict the bandwidth to close winning vote margins of 2% and 3% respectively. Columns 5-8 are RD specifications with a local-linear control for candidate winning vote margin, with and without controls and fixed effects. The bandwidth used in Columns 5-7 is a 10% margin of victory, while Column 8 uses a 20% to expand the number of observations as to not introduce bias into the estimation with the cubic polynomial. Firm and candidate controls include age, gender, incumbent status, membership in the ruling United Russia party, a binary indicator for state ownership, a binary indicator for foreign ownership, and logged total assets in the year prior to the election. Year fixed effects capture the year the outcome variables are measured, region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category.

TABLE A8: POLITICAL CONNECTIONS AND FIRM REVENUE, ONLY SMD CANDIDATES

Control Function:	None				Local Linear				Cubic
Bandwidth:	Global		2%	3%	5%		10%		20%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
District Win	0.321****	0.322****	0.706***	0.531***	0.824**	0.766**	0.748****	0.748**	0.988***
	(0.065)	(0.075)	(0.230)	(0.174)	(0.351)	(0.335)	(0.219)	(0.288)	(0.364)
Bandwidth:	0.8	0.8	0.02	0.03	0.05	0.05	0.1	0.1	0.2
Firm and cand. covariates:	No	Yes	No	No	No	Yes	No	Yes	Yes
Year, Region, Sector FE:	No	Yes	No	No	No	No	No	Yes	Yes
Observations	2,094	2,094	70	109	173	173	369	369	781

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level as well as the lagged value for the outcome as a covariate. Columns 1-2 present OLS results using the full sample, with and without firm and candidate controls and year, sector and region fixed effects. Columns 3-4 also use OLS specifications, but restrict the bandwidth to close winning vote margins of 2% and 3% respectively. Columns 5-8 are RD specifications with a local-linear control for candidate winning vote margin, with and without controls and fixed effects. The bandwidth used in Columns 5-7 is a 10% margin of victory, while Column 8 uses a 20% to expand the number of observations as to not introduce bias into the estimation with the cubic polynomial. Firm and candidate controls include age, gender, incumbent status, membership in the ruling United Russia party, a binary indicator for state ownership, a binary indicator for foreign ownership, and logged total assets in the year prior to the election. Year fixed effects capture the year the outcome variables are measured, region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category.

TABLE A9: POLITICAL CONNECTIONS AND FIRM PROFIT, ONLY SMD CANDIDATES

Control Function:	None				Local Linear				Cubic
Bandwidth:	Global		2%	3%	5%		10%		20%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
District Win	0.007	0.038*	0.153*	0.104*	0.186*	0.187*	0.133**	0.109	0.226*
	(0.022)	(0.022)	(0.082)	(0.054)	(0.097)	(0.099)	(0.055)	(0.072)	(0.127)
Bandwidth:	0.8	0.8	0.02	0.03	0.05	0.05	0.1	0.1	0.2
Firm and cand. covariates:	No	Yes	No	No	No	Yes	No	Yes	Yes
Year, Region, Sector FE:	No	Yes	No	No	No	No	No	Yes	Yes
Observations	2,080	2,080	69	108	172	172	366	366	776

*p<0.1; **p<0.05; ***p<0.01

All models use robust standard errors clustered on the candidate level. Columns 1-2 present OLS results using the full sample, with and without firm and candidate controls and year, sector and region fixed effects. Columns 3-4 also use OLS specifications, but restrict the bandwidth to close winning vote margins of 2% and 3% respectively. Columns 5-8 are RD specifications with a local-linear control for candidate winning vote margin, with and without controls and fixed effects. The bandwidth used in Columns 5-7 is a 10% margin of victory, while Column 8 uses a 20% to expand the number of observations as to not introduce bias into the estimation with the cubic polynomial. Firm and candidate controls include age, gender, incumbent status, membership in the ruling United Russia party, a binary indicator for state ownership, a binary indicator for foreign ownership, and logged total assets in the year prior to the election. Year fixed effects capture the year the outcome variables are measured, region fixed effects capture the region where the election was held, and sector fixed effects capture a firm's two-digit OKVED economic category.

A.6 Coarsened Exact Matching

- Through a technique called coarsening, Coarsened Exact Matching (CEM) assigns continuous values to a small number of categories for each variable, thereby creating bins on which to match upon. Observations are then matched exactly according to their value within each bin, and weights are assigned to the control group observations to allow for the estimation of average treatment effects. This allows for a balancing of the treatment and control groups as completely as possible, since treatment group cases that have no corresponding control-group member in their bins are eliminated. The choice of smaller bin sizes leads to improved balance but at the cost of a decrease in the number of observations available to match. Notwithstanding this trade-off, CEM matches observations based on all properties of their covariate distributions, not just differences in means, and reduces bias, inefficiency and causal estimation error.

- I first restricted the sample to include only firms that were located in the regions where director candidates ran for office and that reported financial data in the years that these candidates ran for and left office (as above for losing firms, this would be the final year of the legislature convocation for which their director ran). This limitation enforces that the directors of matched firms would have also had the opportunity to run for office, but chose not to.

- I coarsened the variable measuring logged total assets into 75 bins. This coarsening takes advantage of breadth of the firms available in full control dataset and allows for very precise matching on firm size.[36] Firms were also matched on five other binary indicators: the presence of state ownership, open joint-stock company status, closed joint-stock company status, and the availability of balance sheets in years corresponding to the first and last year a treated firm would have had political representation in a regional parliament. The original sample contained roughly 416,000 untreated and between 200 and 1500 treated observations (depending on the bandwidth cutoff used). Before matching, significant differences existed between the unmatched sample of firms from SPARK and each of the two treatment groups. Firms that contested elections, regardless if they won or lost, had greater total assets, were more likely to have state-ownership, and more likely to be an open join-stock company rather than a closed joint-stock company. After conducting the CEM procedures, I was able to construct a matched sample that was considerably more balanced on each of these covariates. The average overall $\mathcal{L}$ imbalance score between the six unmatched and treated samples was 1. After matching we retained roughly 80% of the treated units in each sample, a return an average overall $\mathcal{L}$ imbalance score of 0.347, or an large average imbalance reduction of 65%.

- Tables A10-A15 present the full balance tables for the CEM matching procedures. Each table is divided into two panels. The left panel presents differences-in-means and p-value from a two-sided t-test between the unmatched

[36] Results are robust to both smaller and larger bin sizes for total assets

and treated units, that is, the pre-matched sample. The right panel also presents the differences in means, but after the CEM procedure has matched and weighted the samples. The $\mathcal{L}$ imbalance statistics are given for both the unmatched and matched samples as an overall metric of the improvements the CEM procedure offers. Tables A10-A12 show imbalance for the treatment of a firm winning office, with the treated sample being limited by bandwidths of 10%, 20% and 100% respectively (how much firm directors won elections by). Tables A13-A15 are identical, except that the treatments there are whether a firm contested but lost an election, with each table presenting samples limited by 10%, 20% and 100% vote margin in defeat.

TABLE A10: COVARIATE BALANCE IN FULL AND MATCHED SAMPLES, WINNING FIRMS - BANDWIDTH = 0.1

		Panel A				Panel B			
	Sample:	Full Sample				Matched Sample			
	Weights:	No Weights				Weighted			
	Variable	Unmatched	Treated	Diff.	p	Matched	Treated	Diff.	p
1	Total Assets (logged)	8.44	11.32	-2.88	0.00	11.14	11.22	-0.08	0.57
2	State-Owned	0.03	0.08	-0.06	0.00	0.05	0.05	0.00	1.00
3	Open Joint-Stock	0.05	0.37	-0.32	0.00	0.38	0.38	0.00	1.00
4	Closed Joint-Stock	0.84	0.54	0.30	0.00	0.57	0.57	0.00	1.00
5	Start Year Matched				No				Yes
6	End Year Matched				No				Yes
7	Observations	416606	224			18764	208		
8	L1 Statistic				1				0.37

TABLE A11: COVARIATE BALANCE IN FULL AND MATCHED SAMPLES, WINNING FIRMS - BANDWIDTH = 0.2

		Panel A				Panel B			
	Sample:	Full Sample				Matched Sample			
	Weights:	No Weights				Weighted			
	Variable	Unmatched	Treated	Diff.	p	Matched	Treated	Diff.	p
1	Total Assets (logged)	8.44	11.38	-2.93	0.00	11.16	11.33	-0.17	0.09
2	State-Owned	0.03	0.06	-0.03	0.01	0.04	0.04	0.00	1.00
3	Open Joint-Stock	0.05	0.39	-0.33	0.00	0.38	0.38	0.00	1.00
4	Closed Joint-Stock	0.84	0.55	0.29	0.00	0.57	0.57	0.00	1.00
5	Start Year Matched				No				Yes
6	End Year Matched				No				Yes
7	Observations	416606	458			35655	435		
8	L1 Statistic				1				0.36

TABLE A12: COVARIATE BALANCE IN FULL AND MATCHED SAMPLES, WINNING FIRMS - BANDWIDTH = 1.0

		Panel A				Panel B			
	Sample:	Full Sample				Matched Sample			
	Weights:	No Weights				Weighted			
	Variable	Unmatched	Treated	Diff.	p	Matched	Treated	Diff.	p
1	Total Assets (logged)	8.44	11.99	-3.55	0.00	11.75	11.92	-0.18	0.00
2	State-Owned	0.03	0.04	-0.02	0.00	0.04	0.04	0.00	1.00
3	Open Joint-Stock	0.05	0.42	-0.37	0.00	0.42	0.42	0.00	1.00
4	Closed Joint-Stock	0.84	0.53	0.31	0.00	0.54	0.54	0.00	1.00
5	Start Year Matched				No				Yes
6	End Year Matched				No				Yes
7	Observations	416606	1478			92432	1419		
8	L1 Statistic				1				0.3

TABLE A13: COVARIATE BALANCE IN FULL AND MATCHED SAMPLES, LOSING FIRMS - BANDWIDTH = 0.1

		Panel A				Panel B			
	Sample:	Full Sample				Matched Sample			
	Weights:	No Weights				Weighted			
	Variable	Unmatched	Treated	Diff.	p	Matched	Treated	Diff.	p
1	Total Assets (logged)	8.44	10.70	-2.26	0.00	10.61	10.64	-0.03	0.84
2	State-Owned	0.03	0.09	-0.06	0.00	0.09	0.09	0.00	1.00
3	Open Joint-Stock	0.05	0.39	-0.33	0.00	0.37	0.37	0.00	1.00
4	Closed Joint-Stock	0.84	0.52	0.33	0.00	0.54	0.54	0.00	1.00
5	Start Year Matched				No				Yes
6	End Year Matched				No				Yes
7	Observations	416606	217			16264	205		
8	L1 Statistic				1				0.39

TABLE A14: COVARIATE BALANCE IN FULL AND MATCHED SAMPLES, LOSING FIRMS - BANDWIDTH = 0.2

		Panel A				Panel B			
	Sample:	Full Sample				Matched Sample			
	Weights:	No Weights				Weighted			
	Variable	Unmatched	Treated	Diff.	p	Matched	Treated	Diff.	p
1	Total Assets (logged)	8.44	10.52	-2.08	0.00	10.34	10.47	-0.12	0.23
2	State-Owned	0.03	0.09	-0.07	0.00	0.08	0.08	0.00	1.00
3	Open Joint-Stock	0.05	0.36	-0.31	0.00	0.36	0.36	0.00	1.00
4	Closed Joint-Stock	0.84	0.53	0.31	0.00	0.56	0.56	0.00	1.00
5	Start Year Matched				No				Yes
6	End Year Matched				No				Yes
7	Observations	416606	485			37763	463		
8	L1 Statistic				1				0.37

TABLE A15: COVARIATE BALANCE IN FULL AND MATCHED SAMPLES, LOSING FIRMS - BANDWIDTH = 1.0

		Panel A				Panel B			
	Sample:	Full Sample				Matched Sample			
	Weights:	No Weights				Weighted			
	Variable	Unmatched	Treated	Diff.	p	Matched	Treated	Diff.	p
1	Total Assets (logged)	8.44	10.23	-1.78	0.00	10.14	10.21	-0.07	0.31
2	State-Owned	0.03	0.09	-0.06	0.00	0.08	0.08	0.00	1.00
3	Open Joint-Stock	0.05	0.33	-0.27	0.00	0.32	0.32	0.00	1.00
4	Closed Joint-Stock	0.84	0.58	0.27	0.00	0.59	0.59	0.00	1.00
5	Start Year Matched				No				Yes
6	End Year Matched				No				Yes
7	Observations	416606	1140			92077	1107		
8	L1 Statistic				1				0.33