Estimating the Productivity Cost of Crony Capitalism in Russia

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Abstract:

We assemble a large dataset of Russian firms including ~60,000 firms per year covering an estimated 62% of GDP per year from 2003-2011. Our data include sales, capital stock, labor payments, and prominent people such as the CEO, Board of Directors, and large shareholders. Matching these prominent people to a newly assembled list of politicians including members of Putin's personal network, the Executive cabinet, the State Duma, and the Regional Parliaments allows us to generate a list of firms with political connections. Applying the structural model of Hsieh and Klenow (2009), we calculate firm TFPR. Panel regressions of TFPR on political connections show that connected firms are moderately less productive. Regression discontinuity analysis of businessmen candidates who run for regional and national parliament shows that the firms of those who win a seat enjoy a significant decline in capital and output frictions relative to the firms of losing candidates. The effects are large and the methods permit a causal interpretation. Again using the structural model, we translate our firm-level estimates into an estimate of the effect of connections on country-wide TFP. The estimated effect is 28.8% of GDP for 2010 and 31.3% of GDP for 2011, suggesting that crony capitalism considerably reduces Russian GDP.

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1 Introduction

Crony capitalism defines a system in which close relationships with public officials—used to secure government contracts, tax breaks, permits, or loans—are a *sine qua non* of commercial success. It is strongly suspected that such cronyism, by assigning commercial success to the politically connected rather than the entrepreneurially talented, inhibits the efficient deployment of capital and labor, reducing macro-economic productivity and retarding growth. Studies have repeatedly shown that connected firms enjoy superior stock market performance due to improved market power and/or preferential access to capital.[2] Yet estimating the macroeconomic effects reliably is difficult; the only two papers to have done so (Khwaja and Mian 2005, Claessens, Feijen, and Laeven 2008) have focused solely on capital misallocation and have arrived at starkly different magnitudes. We provide quantitative evidence on the magnitude of misallocation due to political connections in Russia including both capital and output frictions.

We focus on Russia because it is the modern paragon of crony capitalism. According to Ledeneva (2013), Putin's Russia is characterized by a network of money and power linked through informal practices, where private property is not secure and the legal system is a corruptible piece to be deployed in private power struggles. Corporate raiding by state officials, the so-called "werewolves in epaulettes" that seize private business by force, forgery, or fraud, necessitates a ceaseless search for political protection. Because the police and courts are both greedily active and also for sale, businessmen pay bribes to sidestep regulations, avoid taxes, jail rivals, and partake in the seizure of others' resources. There are no prohibitions against holding public office while running a private business. The lines between business and the state are blurred and personal networks replace the rule of law with Putin the dominant central node. If political connections—ties between public officials and corporate executives—are not relevant to the macroeconomic misallocation of resources in Putin's Russia, then it is hard to imagine them being significant in any context.

We choose this worst-case scenario because the identification and quantification of corruption is bedeviling and we have surely shone the spotlight on only one small piece of the entire system. Specifically, we compare the performance of firms with and without political connections. Our dataset includes hundreds of thousands of firms comprising an outright majority of GDP. Nonetheless, we can only identify a connection when the same person simultaneously holds both political office and a key role at the firm. We thus miss all sorts of connections due to friendships and other forms of acquaintance. Moreover, we observe only a very few individuals at each firm and only the most visible elected government officials. The majority of the bureaucracy and upper management remain unobserved. We are also, by focusing solely on the differential between connected and unconnected

[2] See section 2 for a lit review. For a partial list of papers relevant to the performance of politically connected firms: Fisman 2001; Johnson and Mitton 2003; Khwaja and Mian 2005; Faccio 2006; Ferguson and Voth 2008; Claessens, Feijen, and Laeven 2008; Li, Meng, Wang, and Zhou 2008; Bunkanwanicha and Wiwattanakantang 2009; Faccio and Parsley 2009; Goldman, Rocholl and So 2009, 2013; Braun and Raddatz 2010; Niessen and Ruenzi 2010; Desai and Olofsgård 2011; Cingano and Pinotti 2013; Earle and Gehlbach 2015

firms, not quantifying any of the frictions of corruption incumbent on all firms who navigate the system. We are simply focusing on the net effect of these highly visible transactions on the productivity and allocation of capital across firms. Because they are likely a small part of the overall cost of corruption, it behooves us to focus first on where these costs might be large. Further work might then focus on the ratio between this visible tip and the extent of the rest of the iceberg.

From a private data provider (SKRIN) which sources official government statistics, we have data on an enormous unbalanced panel of Russian firms from 2003 to 2011, including balance sheets and income statements as well as lists of executives, board members, and large shareholders. The panel averages roughly 60,000 usable firms per year covering, on average, an estimated 62% of GDP per year.[3] Our panel includes public and private firms and spans all industries. We use Benford digit tests to confirm the data are not warped by widespread fraudulent reporting and drop firms identified as fraudulent by Mironov (2006). We document connections between firms and multiple levels of government: regional parliaments, the federal parliament, the cabinet, and Putin's personal network. We find an average of just over 900 connected firms per year, comprising an average estimated 7% of GDP.

We use Hsieh and Klenow's (2009) (henceforth HK) model of monopolistic competition with heterogeneous firms to calculate firm-level TFP from data contained on the balance sheets and income statements. The model allows for firm-specific distortions that drive wedges between the marginal products of capital and labor across firms. We then relate a firm's capital and output wedges to its political connections.

We define politically connected firms as those where a chief officer, director, or large shareholder (which we collectively term *stakeholders*) is simultaneously holding office in the state duma, a regional parliament, a member of the executive cabinet, or is a member of Putin's personal network of family, friends, and allies as documented by Ledeneva (2013). Standard panel regressions with controls for firm-characteristics including charter type, ownership, geographic region, and industry deliver the familiar result. Connected firms are significantly less productive (9.5%), stemming from lower marginal revenue productivity of capital (10.7%) suggesting that such firms enjoy easier access to capital and perhaps output subsidies such as public procurement contracts. Due to the sample size, these results carry t-statistics in excess of 4.

But connections are not formed randomly. It is plausible that unobserved characteristics of the firm affect both firm productivity and the ability to attract board members. These could include deliberate strategies such as an embattled firm seeking political cover (low productivity precipitates high connections) or a dynamic firm culture attracting political allies (high productivity begets connections). To address this issue, we make use of the fact that the outcome of a close election, and thus the assignment of the resulting political connection, is nearly random. (We employ methods from McCrary

[3] We do not have firm-specific data on value added. To estimate GDP coverage, we aggregate firm-level data on payments to labor (wages plus benefits). We then calculate the total payments to labor using data on real GDP and labor share from FRED. Thus our estimated coverage for a single year is: $\sum_{firms \in SKRIN} wL_{firm,t} \Big/ (1-\alpha_t)Y_t$

2008 and Lee and Lemieux 2010 to check the validity of this assumption.) We gathered data on candidates for regional and federal parliamentary seats between 2006-2011, their existing corporate connections, and their electoral fortunes. We can thus compare firms whose stakeholders won election to those whose stakeholders failed to win election. We estimate the treatment effect under a variety of regression discontinuity designs. In each of these additional analyses, the initial result is reconfirmed.

Finally, we conduct a counter-factual thought-exercise to estimate the GDP effects of such connections. We suppose that each un-connected firm is gifted a connection, thereby reducing its capital and output frictions by our estimates of the effects of a connection. We then calculate the overall effect on GDP, allowing capital and labor to readjust in response so as to equilibrate the after-wedge rates of return. For 2010 and 2011, the two years with widespread data on both connections and TFPR, we estimate the GDP effect of these connections to be 28.8% and 31.3% respectively. This suggests that politically induced frictions can be a large factor in misallocation and a significant retardant to GDP.

This paper proceeds with a short review of prior results about the firm-specific effects of connections. In section 3 we introduce and characterize our novel data sets, including measures of firm-specific productivity and political connections. Section 4 contains the panel and regression discontinuity analyses. We estimate the macroeconomic effects in Section 5 before final remarks in section 6.

2 A Review of the Literature

A number of studies have confirmed that firms are more likely to form political connections in corrupt places, often as a substitute for protection under the rule of law (Faccio 2006; Li-Meng-Wang-Zhou 2008; Boubakri-Cosset-Saffar 2008; Braun-Raddatz 2010). This makes it difficult to determine via cross-sectional regressions whether the prevalence of such connections inhibits growth as the relationship between crony capitalism, a weak judiciary, and growth cannot be easily untangled.

In principle, one could infer the macroeconomic effect from firm-level data. Misallocation due to crony capitalism would manifest as capital and labor market distortions. One could estimate such distortions using firm-level data, attribute some fraction to cronyism, and then construct the distribution of firm productivity, and the corresponding economy-wide TFP under the counterfactual of zero cronyism.

Early work on connected firms has almost universally shown that connections are associated with better stock market performance (Fisman 2001; Johnson-Mitton 2003; Faccio 2006; Knight 2006; Claessens-Feijen-Laeven 2008; Ferguson-Voth 2008; Faccio-Parsley 2009;Goldman-Rocholl-So 2009; Niessen and Ruenzi 2010; but Fan-Wong-Zhang 2007). But studies of stock market performance are necessarily limited to publically traded firms, are unable to explain the channel by which such performance is achieved, and thus say little about the productivity effects. Subsequent work comparing the performance of connected and unconnected firms finds that the channels by which political connections pay dividends are varied. In many places, they improve access to capital (Khwaja-Mian 2005; Faccio-Masulis-McConnell 2006; Li-Meng-Wang-Zhou 2008; Claessens-Feijen-Laeven 2008; Desai-Olofsgard

2011). In others, they provide market power via protection or public contracts (Bunkanwanicha-Wiwattanakantang 2009; Desai-Olofsgard 2011; Cingano-Pinotti 2013; Goldman-Rocholl-So 2013). But connected firms may also be induced to retain inefficiently large payrolls as a quid pro quo (Menozzi-Urtiaga-Vannoni 2011; Desai-Olofsgard 2011). Each of these studies points to a channel whereby a connected firm reaps rents at the expense of misallocation and lower aggregate TFP.

Only two of these firm-level studies have sufficient data to have explicitly estimated macro-economic effects. They come to starkly different conclusions. Khwaja and Mian (2005) find that Pakistani banks make loans preferentially to politically connected firms, resulting in a misallocation of capital. Using the difference in rates of return between normal and corrupt loans, they estimate that the cost of rent provision is 1.9 percent of GDP per year. Claessens, Feijen, and Laeven (2008) find that Brazilian firms who contribute to federal election campaigns substantially increase their bank financing but have lower return on assets despite higher investment rates. Performing a similar calculation, they estimate the cost of inefficient loan provision to be 0.2% of GDP per year. These estimates differ by an order of magnitude, probably because they are from rather different countries. They are also confined to a single channel: capital misallocation.

The only other paper of which we are aware that relates firm-level TFP to political connections is Earle-Gehlbach (2015). They focus on a single event—the Orange Revolution in Ukraine—which shifted the geographic balance of power. The victor, Yuschenko, drew support from the Ukrainian speaking West while the vanquished, Yannikovich, drew support from the Russian-speaking East. Earle and Gelbach find that firms in regions supporting the victor enjoyed larger post-election growth in firm-productivity. As we do, they calculate firm-level TFP effects of political ties. However, they do not infer the macroeconomic effect on GDP. And they focus on an electoral shift in power whereas we focus on the persistent extent of connections between firms and office-holders.

In this context, our study has several strengths. First, we calculate firm-specific wedges and TFPR. Second, we have firm-specific political connections. Third, we have extensive coverage of a large and influential economy with a particularly stark political economy. Fourth, our regression discontinuity design enables causal inference. Finally, we directly estimate the macroeconomic effects on national GDP.

3. Data

Our source for Russian firm-level data is The Comprehensive Issuer Information Disclosure System (acronym SKRIN), which was founded in 1999 by shareholders of the National Association of Securities Market Participants and is sort of like a Russian Bloomberg or Bureau van Dijk. They source over a hundred issuers, most importantly Rosstat, the official statistical agency of the Russian Federation. By law, all firms, even small ones, must release quarterly balance sheets and income statements to Rosstat. However, prior to 2014 this was on a voluntary basis. Our sample period is 2003-2011 during which reporting was common but voluntary. Based on the number of returns from 2014, the first year in which reporting was mandatory, it seems that about one third of Russian companies, roughly 600,000 – 700,000 companies per year, are in the SKRIN database during our sample period. However, owing to inconsistent reporting of the specific balance sheet lines needed for our calculations (labor payments are especially sparse), our actual dataset averages ~60,000 firms per year. Prior to 2009, only a small fraction of these companies (10-20,000) reported information on their executive suite members. But from 2009-2011, we have data on the executive suite of roughly 400,000 companies per year. Certain types of joint stock companies are obliged to submit financial statements to the central bank including information about board members and large shareholders/owners. From this much smaller sample we have information on the board members and large shareholders of about 500-2000 companies per year.

The distribution of firms in our sample across the 83 federal regions is closely related to the distribution of population; the correlation coefficient is 0.83. The largest deviation from this relationship is the overrepresentation of the Federal City of Moscow, which contains 8.3% of the population but hosts 28.6% of firms. If it is removed from the data, the correlation between population and incorporation rises to 0.88.

Figure 2 displays the frequency of firms by industry code. Limited liability companies are by far the most common form of incorporation, comprising some 83.4% of the database. While there are some time trends during our sample in the frequency of the various uncommon types, this form remains the overwhelmingly dominant instance throughout.

We do not know which firms choose not to report to Rosstat and why. To test the accounting quality of these numbers, we perform a Benford digit analysis from which we can reject mass statistical fabrication to avoid tax thresholds or some such. We also toss out firms that Mironov (2006) explicitly identified as vehicles for tax evasion.

3.1 Construction of firm TFP

Value added is typically measured as gross revenues less the cost of materials. SKRIN does not provide a direct measure of value added. Rather, we have measures of gross revenues and various stages of profits. We have *gross revenues* from which we subtract *cost of sales*. However, *cost of sales* includes both materials and direct wages so we add back in the wage bill. To measure the capital stock, we use *total assets* on Jan 1st of the year in question. Following HK, we use *wages* plus total *fringe benefits* as our measure of labor. For the industry-level capital share, we use US data for the corresponding industry, for the very reason that the target country is suspected of being distorted. The US shares are calculated using the BEA's GDP-by-Industry data. They cover 71 industries, classified using NAICS. The Russian data are classified under OKVED. Both are converted to NACE1.1 for matching. For robustness, we also check using 6-digit capital shares, though this limits us to manufacturing only. We use a national GDP deflator for Russia to compare across years. We follow HK2009 in assuming the rental rate of capital is $R = 0.10$ and the elasticity of substitution between intermediate goods, $\sigma = 3$. We test the robustness of our results to changes in both of these parameters.

3.2 Firm TFPR

For each firm-year for which sufficient data was available, we calculated TFPR of firm *i* in industry *s* according to the following formulas from HK and its appendix of corrections.

$$TFPR_{si} = \frac{P_{si}Y_{si}}{K_{si}^{\alpha}\left(w_{si}L_{si}\right)^{1-\alpha}} = \frac{\sigma}{\sigma-1}\left(\frac{R}{\alpha_s}\right)^{\alpha_s}\left(\frac{1}{1-\alpha_s}\right)^{1-\alpha_s}\frac{\left(1+\tau_{K_{si}}\right)^{\alpha_a}}{\left(1-\tau_{Y_{si}}\right)} \tag{1}$$

$$1+\tau_{K_{si}} = \left(\frac{\alpha_s}{1-\alpha_s}\right)\left(\frac{wL_{si}}{RK_{si}}\right) \tag{2}$$

$$1-\tau_{Y_{si}} = \frac{\sigma}{\sigma-1}\frac{wL_{si}}{\left(1-\alpha_s\right)P_{si}Y_{si}} \tag{3}$$

Where $TFPR_{si}$ is the revenue total factor productivity of firm *s* from industry *i* and τ_K and τ_Y are the capital and output distortions respectively. We used the measures discussed previously: capital and reserves for K_{si}, wages and salaries for wL_{si}, revenues less cost of sales plus wages for "value added", $P_{si}Y_{si}$, assumed values for elasticity of substitution, σ, and return on capital, R, and US 2-digit capital shares for α_s.

Having calculated firm TFPR, we then normalize by the industry average TFPR and take the log. Following HK, the industry average is taken over a sample that has been trimmed of the top and bottom 1%.

$$tfpr \equiv \ln\left(TFPR_{si} / \overline{TFPR_s}\right) \tag{4}$$

$$\overline{TFPR_s} = \frac{\sigma}{1-\sigma}\left(\frac{\overline{MPRK_s}}{\alpha_s}\right)^{\alpha_s}\left(\frac{\overline{MPRL_s}}{1-\alpha_s}\right)^{1-\alpha_s} \tag{5}$$

$$\overline{MPRL_s} = \frac{w}{\left(\sum_{i=1}^{M_s}\left(1-\tau_{Ysi}\right)\frac{P_{si}Y_{si}}{P_sY_s}\right)} \tag{6}$$

$$\overline{MPRK_s} = \frac{R}{\left(\sum_{i=1}^{M_s}\frac{\left(1-\tau_{Ysi}\right)}{\left(1-\tau_{Ksi}\right)}\frac{P_{si}Y_{si}}{P_sY_s}\right)} \tag{7}$$

Figure 1 shows the distribution of *tfpr* across all firms in the sample for the years 2003-2011 which can be compared to HK Figure II.[4]

Firm *tfpr* is somewhat persistent with successive years correlated at 0.77 in the overall sample, 0.60 in manufacturing, 0.56 in agriculture and mining, 0.62 in construction, utilities, and transportation, and 0.82 in services. These figures span the range of persistence reported in previous studies (Syverson 2011 p327). The distribution of firm *tfpr* tightens between 2003 and 2009 and then becomes broader in 2010 and 2011. Table 1 quantifies and confirms this visual impression with a calculation of the IQR of *tfpr*. The distribution in Figure 2, and the IQR that we calculate for the Russian manufacturing sector (Table 1) are relatively tight compared to those in the manufacturing sectors of other developing countries. We suspect this is because we do not observe the long left tail of small, inefficient firms, most of whom are not likely to report to Rosstat.[5]

We also calculate z-scores by industry for the capital and output frictions, τ_K and τ_Y, which we refer to as: $\tilde{\tau}_K$ and $\tilde{\tau}_Y$.

3.3 Connections

We have collected four lists of politically important individuals: members of the 83 regional parliaments (e.g. The Legislative Assembly of the Irkutsk Region); members of the federal parliament (State Duma); members of the executive cabinet (e.g. Foreign Secretary); and members of Putin's inner circle (e.g. Roman Abramovich).

Members of the State Duma were obtained from the official Duma website (1,922). Dates of occupancy are adjusted to include early vacancies and special elections. Members of the regional parliaments (12,218) and cabinet members (707) were obtained from regional parliament websites and the Russian electoral statistics website, www.geliks.org. A list of Putin's inner circle (72) was obtained from Ledeneva (2013). Of these 14,140 office holders, 12,531 (89%) are male. The average age upon entry to the dataset is 49.[6]

We classify a firm as connected if one of these office holders is, while holding political office, also a stakeholder of the firm in question. We call these *zero-step* connections because they involve the same person occupying both the political and corporate offices. This measure of political connections is surely a restrictive one. Firms are frequently connected at one remove. For example, the CEO's wife might be in the State Duma or the CFO's best friend might be in the Cabinet. In our case, only Putin's network

[4] The x-axis scale differs somewhat because HK decided to graph the distribution of the logged ratio but then label the x-axis for the unlogged raw ratio. (E.g. our 0 is their 1 because ln(1)=0.) Nonetheless, we are both graphing the distribution of the log ratio and finding it normal (thus underlying TFP is log-normal).
[5] This suspected reporting bias against small firms makes us reluctant to draw inferences about the effects of crony capitalism on the firm size distribution as per Adamopoulos and Restuccia (2014).
[6] This figure excludes those in the cabinet and Putin's network.

includes links beyond the actual office holder. We also have no measure of the personal networks of the chief officers of the FSB or other crucial apparatuses of state. Moreover, we have a very limited set of individuals on either side. From the firms, we have in most cases simply the CEO. Other senior staff are missing. From the political side, we have only the elected office-holders and federal executive cabinet. None of the myriad functionaries and bureaucrats critical to policy are included. At best, we have a large sample of the closest and most consequential connections. We are likely able to see only the tip of the iceberg.

Our ability to detect political connections is limited by the extent to which officers and directors are recorded in the dataset. From 2009-2011, when our source on executive suites and shareholders became much more comprehensive, firms with direct political connections comprise only 0.53% of the data by numbers (~2000 firms out of ~400,000) but 43% of the data by revenues, 20% of the data by wage bill, and 55% of the data by imputed value added. This paints a picture of an economy dominated by a few large firms that maintain zero-step political connections and have no qualms about publically disclosing these connections because this is accepted, even standard, practice. To the extent that our "unconnected" firms are actually connected in ways that are not disclosed in our data, we are under-estimating the effect of connections by using an impure control group.

4. Results

4.1 Panel Analysis

As a first pass, we run panel regressions of firm *tfpr*, MRPK, $\tilde{\tau}_K$, and $\tilde{\tau}_Y$ on our indicator of political connections, controlling for firm size using logged deflated revenues and including fixed effects for firm type, ownership, region, industry, and year. We cluster standard errors by ownership. In keeping with prior literature, we find a consistently negative relationship between political connections and firm *tfpr*. The *tfpr* of connected firms is 0.100 log points lower than that of unconnected firms (table 2, column 5). At the median, this translates into a 9.5% decline in revenue total factor productivity. Similar regressions show a 10.7% decline in marginal revenue product of capital (MRPK). Estimates of the capital and output frictions shows that while both are of the expected sign, only τ_K is statistically significant (5.7% lower for connected firms.)

But connections are surely not randomly assigned; they are the product of two-sided search with intent. Perhaps more productive firms are more able to attract office-holders to their boards. Or perhaps struggling firms deliberately court political cover. Moreover, changes in the legal and political environments, which in Putin's Russia may not be adequately summarized by the legal code itself, may unobservably shift the returns to political connections or the attractiveness to a politician of serving on corporate boards. Thus, simultaneity and omitted variables inhibit drawing causal inference from the panel results.

4.2 Regression Discontinuity

4.2.1 Calculating the running variable, *margin*

Our solution is to focus on close elections for political office and compare, using regression discontinuity, board members and officers who narrowly lost election to those who narrowly won. So long as connected *individuals* are incapable of precisely manipulating election results to insure their *individual* victory, this provides a causally valid estimate of the effect of political connections on firm productivity (Lee 2008). We will interrogate this assumption shortly.

To assemble a sufficient sample of uniform quality, we use elections from the national and regional parliaments which include both single member districts (SMD) and multi-member districts (MMD) whose seats are assigned via proportional rule amongst the parties who surpass a vote-share threshold (7% during our sample period). In regional elections, voters typically cast two ballots, one for the SMD candidate of their choice and another for the party of their choice, the latter vote to be used in assigning the MMD seats. In elections to the national parliament during our sample period, voters cast a ballot for a party and all seats are assigned via proportional rule. We have data from the official Russian central election commission spanning 2003 through 2010.[7]

The running variable in our regression discontinuity is the fraction of the vote by which a candidate failed or achieved election. That is, the fraction they or their party would have to gain or lose for them to change treatment status. In SMDs, this is a straight-forward calculation. For successful candidates, we calculate the difference between their vote share and the second place candidate, and then normalize by the percent casting votes for valid candidates ("against all" is a fairly common vote choice). For unsuccessful candidates, we take the difference between their vote share and that of the successful candidate, similarly normalized. This variable, which we term *electoral margin*, has range [-100, 100] with a treatment cutoff of 0.

For the multi-member districts, we have obtained the party lists denoting the rank amongst would-be seat-holders and thus the order in which seats would be assigned by the party as they are earned via electoral performance. In the cleanest instances, these are single lists, one per party, ranking candidates from 1 to N for N seats in contention. Should the party win M < N seats, the top M names on the list are seated. In these cases, we can calculate the change in party vote-share that would have been necessary for individual X to change from the treatment (seated) to control (not seated) or vice versa.

Thresholds and the rules on seat allocations make the exact calculation rather complex and dependent on which other parties the votes are drawn from. For simplicity, we assume that if party A's vote share were to increase by V_A%, then every other party's vote share would decline in proportion to its actual vote-share. That is, we assume all voters are equally likely to switch allegiance to contribute to any particular shock. For example, suppose the actual vote-shares of parties {A, B, C, D} are {40, 30, 20, 10} and we are considering the effects of a +6% shock to party A. The hypothetical, post-shock vote-shares

[7] SMD results are available from 2003; MMD results require party lists which are not available prior to 2006. Thus our regional MMD data begin in 2006. The only Federal elections during this period are in 2007.

would be {46, 27, 18, 9} as parties B, C, and D lose votes proportionally. We can then proceed to apply the seat allocation rules to determine the change in seats that would arise from this shock. For each member of the party list, *electoral margin* is defined to be the smallest shock to their own party's vote share that would switch their treatment status.

Two complications remain. The first is that in several regions during this period, some or all parties do not rank candidates in single-file but have instead a trunk and branch structure. For example, suppose a regional parliament admits 15 PR seats. A party may then choose (as one possibility among others) to structure their list with 3 general candidates at the top, followed by 4 sub-lists of three candidates each, where each sub-list is tied to a particular geographic sub-region. The first three seats earned are assigned in order to the general candidates. Further seats are assigned to electoral districts in order of the party's relative performance across sub-regions. Seats assigned to a sub-region are then assigned within the sub-list for that electoral sub-region by rank on the sub-list. Such a structure is frequently chosen to incentivize candidates to campaign and turn out the vote in their assigned district so as to maximize the number of seats the party allocates to their sub-list. Calculating *electoral margin* for this setup is possible provided electoral data can be matched to the sub-regions around which parties form their sub-regional lists. For example, if parties in Primosky Krai have one sub-regional list for Vladivostock and another list for the rest of the Krai, then we would need votes to be reported separately for Vladivostock and the rest of the Krai. Unfortunately, in 23 regional elections, it is not possible to map the electoral districts onto the party lists. These elections are dropped leaving us with 41 regional elections and one federal election.

Secondly, even among feasible party-regions, the assignment is not perfect. Candidates who would otherwise merit a seat are sometimes disqualified or withdraw, leading the seat to be assigned to a candidate of lower rank.[8] Less frequently, a candidate of stated low rank is inexplicably given a seat against the listed priority. Fuzzy regression discontinuity is designed to handle these mis-assignments.

Once we have *electoral margin* scores for each candidate for both SMD and MMD seats, we must attach these to particular firms. We keep those candidates that were already connected to a firm prior to the election and follow these firms so long as this connection remains. This leaves 882 firm-years on which to estimate our fuzzy regression discontinuity using the *tfpr* of the firm and the *electoral margin* (positive or negative) of the board member or executive who ran for office (successfully or no).

[8] One suspects, of course, that such assignment errors are not random, but the product of influence. For instance, the LDP won 40 seats in the 2007 State Duma elections. By the formula, only the sub-lists that garnered at least 63,000 votes ought to have received a seat. But 15 more deserving regions were passed over to award a seat to a region that garnered only 42,000 votes. It is not clear why, but the man who won the seat was the former bodyguard of Vladimir Zhirinovsky, the leader of the LDP party and the vice-chair of the State Duma. There were four other lesser violations of priority in the LDP assignments. One was the son of the deputy head of administration in the province. A second was the head secretary of Zhirinovsky, the other two have no observed connections. These connections may not be atypical or relevant. But those who were passed over did not seem to have similar connections.

4.2.2 Evaluating the sample and design

Regression discontinuity relies on the inability of the subjects to precisely manipulate their treatment status. In our case, this means that firms cannot simply choose whether or not their board member or executive wins the election.[9] McCrary (2008) suggests that manipulation of the running variable will show up as a discontinuity in the density of the running variable at the cutoff. McCrary's (2006) test fails to reject the null of no discontinuity at the 5% level in our combined sample (t-stat of 1.91). However, as seen in the upper-left panel of figure 9, there are more close victories than close losses and the t-stat approaches the conventional threshold. The remaining panels of figure 9 show that this discrepancy seems to spring largely from regional seats assigned via PR and here too the difference narrowly fails to meet a 5% critical value (t-stat 1.92).

Is this suggestive of a problem with randomization of treatment near the cutoff? It is widely suspected that Russian elections during the sample period were subject to vote fraud. The ruling party, United Russia, was accused of stuffing ballot boxes in the State Duma elections of 2007 and 2011. If we drop United Russia from the sample and rerun McCrary's density test for discontinuity in the running variable on the remaining candidates for regional PR seats, the t-stat on the discontinuity declines from 1.92 to 0.54. So United Russia seems to have had a disproportionate number of close winners. We will later test the robustness of our results to removing them from the sample.

But it is not clear that such behavior would invalidate the assumption of continuous density required to ensure validity of regression discontinuity estimates (Lee 2008). Lee (2008) shows that "localized random assignment can occur even in the presence of endogenous sorting as long as agents do not have the ability to sort precisely around the threshold." The critical assumption in our context is that a businessman candidate cannot *precisely* control the relevant vote shares to ensure his/her own election: there must be some random element outside his/her control that ensures the running variable (margin) is characterized by a continuous density in the neighborhood of the treatment threshold (0).

Moreover, the McCrary test must be applied carefully to our data. A discontinuity in the distribution is supposed to indicate that the candidate is able to sort into treatment status. But in the case of ballot stuffing, "sorting" by one candidate necessarily "anti-sorts" the other candidates. Suppose there are two candidates for a seat in single member district. Suppose that in the absence of manipulation (sorting), the first candidate's vote share is uniformly distributed between 40% and 60%. Now suppose that this candidate has the ability to look at the vote share and adjust it by up to 2%, but at some small cost of discovery. (The scenario discussed by Lee 2008 on p684.) Due to the cost, this ability would be employed only if the candidate's pre-manipulation vote total were between 48% and 50%. As a result, the observed density for the first candidate would have no weight between 48% and 50% and double weight between 50% and 52%. But the observed density for the other candidate would be adjusted in precisely the opposite direction—double weight on narrow losses and no weight on narrow wins—such that the combined density over all candidates would be unchanged. McCrary avoids this potential trap by looking

[9] Note that some influence—which would be hard to disprove in this case—does not invalidate the randomization near the cutoff and thus the validity of the quasi-experiment. See Lee and Lemieux (2010) for discussion of this point.

only at the vote shares for a single party. As there is one party that is strongly suspected of widespread fraud (United Russia), we can likewise narrow our focus to that party. McCrary's test fails to reject the null (t-stat 1.23) of no discontinuity, assuaging our fears.

It is less clear how ballot stuffing presents a problem for the RD in multi-member districts allocated according to party lists. Manipulation by one party will transfer seats from other parties and will thus change the point on the party list demarking those candidates who receive a seat from those who do not. The RD is implicitly comparing those candidates that are one seat above with those that are one seat below the cutoff. Changing the location of the cutoff would not seem to affect the validity of this comparison. For this to be a problem for us would require that the extent of the ballot stuffing be targeted to ensure that specific individuals are seated (or denied). Discussion of Russian electoral fraud makes it seem more likely that United Russia simply tried to maximize votes and seats subject to what they thought they could get away with. Moreover, it is not clear that the party had the precise control over vote shares necessary to invalidate the assumption. Nonetheless, we do rerun our results excluding candidates from United Russia as a robustness check.

A second consideration is that businessmen candidates (those with existing firm connections) are unlikely to be a random subsample of the entire set of candidates. Their executive status may influence their placement on the party list (MMD) or their electoral strength (SMD). Conversely, they may have been selected for a board seat precisely because of their party connections or prospective electoral strength. Figure 12 shows the distribution of electoral margin for all SMD candidates. Comparing with panel 2 of figure 9, we can see that businessmen candidates are unusually successful. But as we are comparing from within this distinct group of candidates, this does not invalidate Lee's continuity assumptions.

Figure 5 shows the regression discontinuity plot of *tfpr*. There is strong evidence of a discontinuity in tfpr at the threshold. On the other hand, there are similarly sized jumps between adjacent bins at other points where no such discontinuity is expected. Because this is a fuzzy RD, the latter is unfortunate but not dispositive. Figures 6 and 7 show that these conclusions are not the result of bandwidth selection or the order of the polynomial fit.

An alternate test of the validity of an RD design is to examine whether the baseline covariates are locally balanced in the neighborhood of the threshold (Lee and Lemieux 2010). We conduct such tests of the most important covariates: firm size and whether the firm is fully private or has some degree of public ownership. We fail to reject the null of no discontinuity in either of these covariates (Figure 8).

4.2.3 Regression discontinuity methods and results

We estimate the fuzzy regression discontinuity using Cattaneo, Calonico, and Titiunuk's (2014a,b; 2016) (henceforth CCT) local polynomial estimator with robust bias-corrected standard errors using a coverage-error optimal bandwidth selector. The first issue is the choice of bandwidth. Most of our results are presented using the coverage-error optimal bandwidth as coded by CCT, but we also show robustness to varying this choice. The second issue is selecting the order of the local polynomial. We have chosen a second-order polynomial to minimize AIC. But here too we present robustness to a variety of choices.

Our baseline estimates of firm productivity (*tfpr*) and firm-specific capital and output distortions ($\tilde{\tau}_K$ and $\tilde{\tau}_Y$) are of the expected signs, significant at conventional levels, and, as we will see later, of economically important magnitude (Table 3). Table 4 shows that these results are also robust to changing the two parameters that were chosen when calculating *tfpr*, σ and R. Table 5 shows that the results remain statistically significant when changing the order of the local polynomial.

If the RD is valid, then adding covariates ought not be necessary as standard economic controls, but they can still be useful for reducing the sampling variability in the RD estimates (Lee and Lemieux 2010). We add several covariates—firm size, an indicator of whether the firm is fully private, an indicator of whether the firm is located in Moscow, and seven dummies identifying (coarsely) the most prevalent industry groups in the sample.[10] Indeed, when the full set of controls is included, the point estimates change relatively little but the standard errors decline and z-stats for *tfpr*, $\tilde{\tau}_K$ and $\tilde{\tau}_Y$ rise from (2.18, 2.33, 1.66) to (3.41, 3.14, 2.00) respectively (Table 6).

Finally, we take two steps to address any lingering suspicion that election fraud may somehow interfere with the validity of the RD. Russian electoral fraud during this period is generally suspected to be mostly the work of the ruling party, United Russia. If we drop all businessmen candidates from United Russia (Table 7, column 2), the point estimates decline modestly and the standard errors rise modestly, mainly due to a decline in sample size. The effect has softened somewhat, but the coefficient remains significant at the 10% level. However, given the importance of United Russia, it is reasonable that such connections are more consequential for firm behavior. So we turn to another source of variation: not all regions are equally suspected of fraud. Kireev classifies regions into categories by the degree of ballot stuffing suspected in the 2011 State Duma elections. Splitting the sample roughly in half and rerunning our baseline RD with the less corrupt set of regions (Table 7, column 3) achieves estimates almost identical to the baseline.

Taken as a whole, the regression discontinuity analyses deliver a consistent estimate of the effect of political connections on firm productivity. Connected firms enjoy sizeable capital and output distortions that combine to deliver a marked decline in revenue total factor productivity.

5. Estimating the macroeconomic impact

We now turn to estimating the macroeconomic impact of these politically motivated frictions on economy-wide GDP. Our RD estimates show that gaining a political connection greatly decreases the capital and output frictions (τ_K and τ_Y) faced by a firm. Our thought experiment is to approximate the

[10] The seven industry groups are: agriculture, mining, manufacturing, construction, sales, transport, banking and finance. Adding a finer set of industry dummies prevents estimation due to lack of sufficient variation within a bandwidth. In fact, for Tau_Y, we have to drop one group (finance) to perform the estimation.

frictionless state by granting all firms the estimated decline in frictions that would ensue with political connections and calculate GDP under this alternate scenario.

The benefits to such counterfactual removal of frictions will come mainly from the reorganization of capital and labor toward firms that are productive but were previously discriminated against. Thus, we cannot rely on the observed distribution of capital and labor to calculate GDP. Rather, we solve the HK model to express overall GDP as a function of the inferred firm-specific TFP, A_{si}, and frictions, τ_K and τ_Y. Since the model presumes post-wedge equalization of factor returns for a given distribution of firm-wedges and firm-productivities, this solution captures the gains from redistributing factors in response to the removal of political frictions.

In essence, the overall scheme of the paper to arrive at this estimate consists of the following steps: (i) adopt the HK structural model; (ii) observe the firm-level values of revenues, capital stock, and labor payments; (iii) use the model and these firm data to infer firm-specific TFP and firm-specific capital and output frictions, τ_K and τ_Y; (iv) estimate the effect of political contributions on the firm-specific frictions via RD; (v) calculate counterfactual frictions, τ_K' and τ_Y', by adding the estimated effect of political connections from the previous step to all unconnected firms; (vi) calculate GDP via insertion of the counterfactual firm-specific frictions and TFP in the equilibrium specified by the structural model. As we are interested only in the percentage effect on GDP, we have, in the following solutions to the HK model, dropped multiplicative factors that are unaffected by the counterfactual.

$$Y = \prod_{s=1}^{S} \left[\sum_{i=1}^{M_s} \left(A_{si} \frac{\overline{TFPR_s}}{TFPR_{si}} \right)^{\sigma-1} \right]^{\frac{\theta_s}{\sigma-1}} \tag{8}$$

$$TFPR_{si} \propto \frac{\left(1+\tau_{K_{si}}\right)^{\alpha_s}}{1-\tau_{Y_{si}}} \tag{9}$$

$$\overline{TFPR_s} \propto \left[\frac{1}{\sum_{i=1}^{M_s} \left(\frac{1-\tau_{Y_{si}}}{1+\tau_{K_{si}}} \right) \left(\frac{P_{si}Y_{si}}{P_s Y_s} \right)} \right]^{\alpha_s} \left[\frac{1}{\sum_{i=1}^{M_s} \left(\frac{1-\tau_{Y_{si}}}{1} \right) \left(\frac{P_{si}Y_{si}}{P_s Y_s} \right)} \right]^{1-\alpha_s} \tag{10}$$

$$P_{si}Y_{si} \propto \left[\frac{A_{si}\left(1-\tau_{Y_{si}}\right)}{\left(1+\tau_{K_{si}}\right)^{\alpha_s}} \right]^{\sigma-1} \tag{11}$$

$$P_s Y_s \propto \left[\left(P_{si}Y_{si}\right)^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}} \tag{12}$$

To perform the counterfactual, we reduce the wedges for all firms without connections by the estimated amount.

$$\tau_z' = \tau_z + \beta_{\tau_z},\ z \in \{K, Y\} \tag{13}$$

When performing the alternate calculation, we use the estimated effect from our baseline specification (table 3, columns 2 and 3) of the causal effect of connections on GDP for the two years with the most data: 2010 and 2011. We made the same adjustments to connected firm TFPR in each year, but the ultimate effect on GDP differs because firms occupy a different fraction of the market or a different place within the raw TFPR distribution from year-to-year, and because the set of connected firms changes. We estimate that endowing all firms with the benefits of these zero-step connections would have raised 2010 GDP by 28.8% and 2011 GDP by 31.3% (see table 8). The low-end of the confidence interval cuts the effect nearly in half. Nonetheless, 16% of GDP is a significant effect.

6. Discussion

The results raise at least two other puzzles. First, it may seem surprising that so large an effect is in evidence from the first year following the connection. But there is no obvious reason why government contracts and implicitly backed loans should take longer to secure, or why the sidestepping of regulations and taxes and the extra-legal beggaring and jailing of opponents should not begin immediately. Unfortunately, due to inconsistent reporting (especially of labor payments) and a short electoral dataset, the average firm remains in the usable dataset for a mere 1.4 years following the connection. As a result, we are unable to undertake an investigation of the effect of connections over time.

A second puzzle is the discrepancy between the panel and the regression discontinuity results, the latter being 20 times larger than the former. We run the latter for fear that connections are not randomly formed, thus casting doubt on the causal validity of the panel results. But the regression discontinuity also clearly operates on a distinct subsample: firms actively seeking political connections. One would expect such firms to benefit disproportionately from such a connection.

Even the lower end of our range, sixteen percentage points of GDP, is a large effect. If plausible, it would constitute evidence that crony capitalism is a substantial piece of the misallocation puzzle. There are three reasons this may be an overestimate. First, as just mentioned, the subsample from which this effect is estimated may not be representative. Second, to the extent that frictions are rivalrous, our thought experiment whereby all unconnected firms enjoy a decline in frictions simultaneously may not accurately represent the world without political frictions. Third, as discussed previously, our control may be dirty in that some firms that we do not see as connected are already enjoying the benefits of connections. Our counterfactual then bestows upon them a benefit they are already receiving.

Nonetheless, there are also reasons this may be an under-estimate. As we discuss in the introduction, this might be the mere tip of the iceberg. We observe only zero-step connections where the same person holds both corporate and political office. Thus we may actually be comparing firms connected explicitly with firms connected via friends and family, leading to an underestimate of the value of connections. We have also not yet looked at the effect of connections on exit, which is an important source of long-run GDP growth. We may also be ignoring the potential effects of market power. As HK note, government-guaranteed monopoly power would *increase* the TFPR of the connected firm. Thus to the extent that connected firms secure market protection rather than government contracts or access to capital, we may be underestimating the effect.

In conclusion, we find that connected firms enjoy significantly lower frictions than unconnected firms. Moreover, these effects are sufficiently large that the elimination of these additional barriers for unconnected firms would increase GDP by 15-38%. One of the few direct estimates of the GDP effects of crony capitalism, our results indicate politically induced frictions may be a large part of the misallocation puzzle.

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Figure 1: Evolution of the Distribution of Firm TFPR

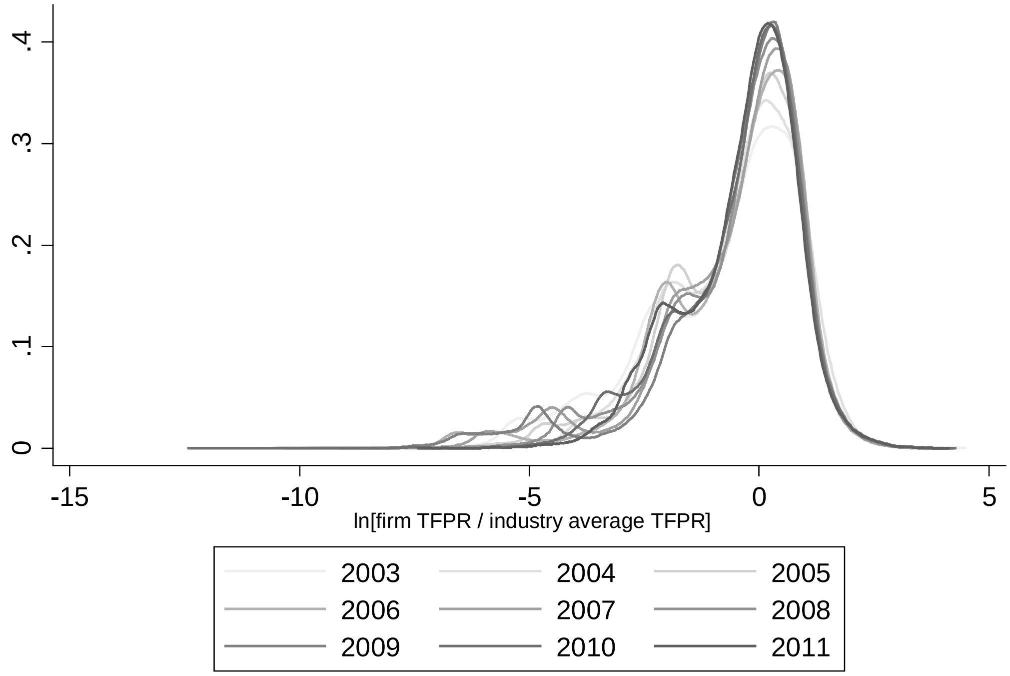

Figure 2: Evolution of the Distribution of Manufacturing Firm TFPR

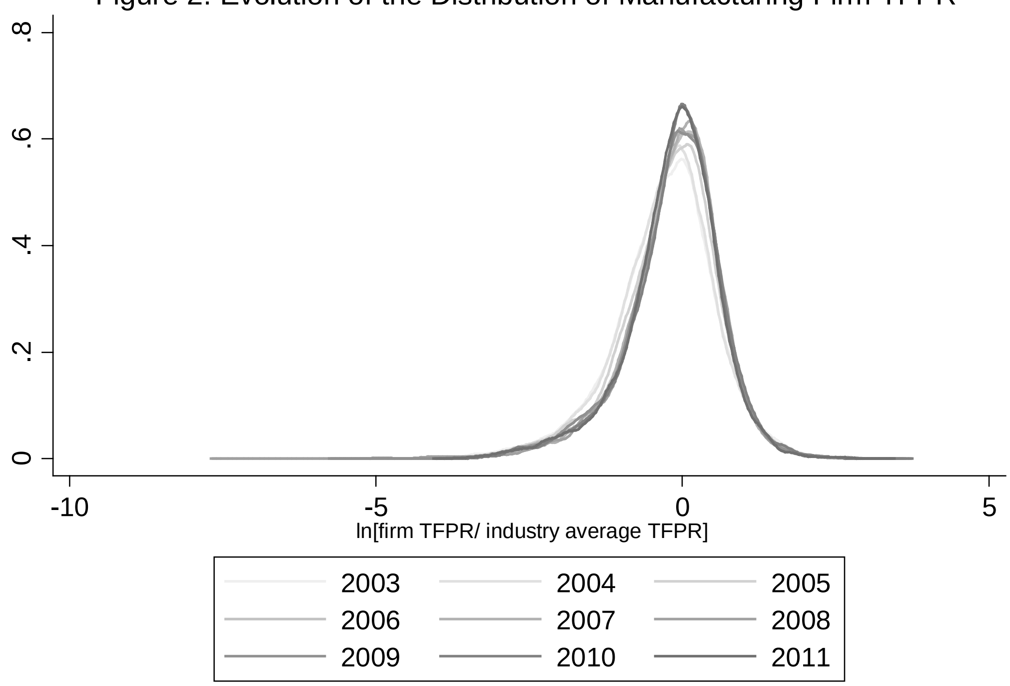

Figure 3: SKRIN Industry Coverage

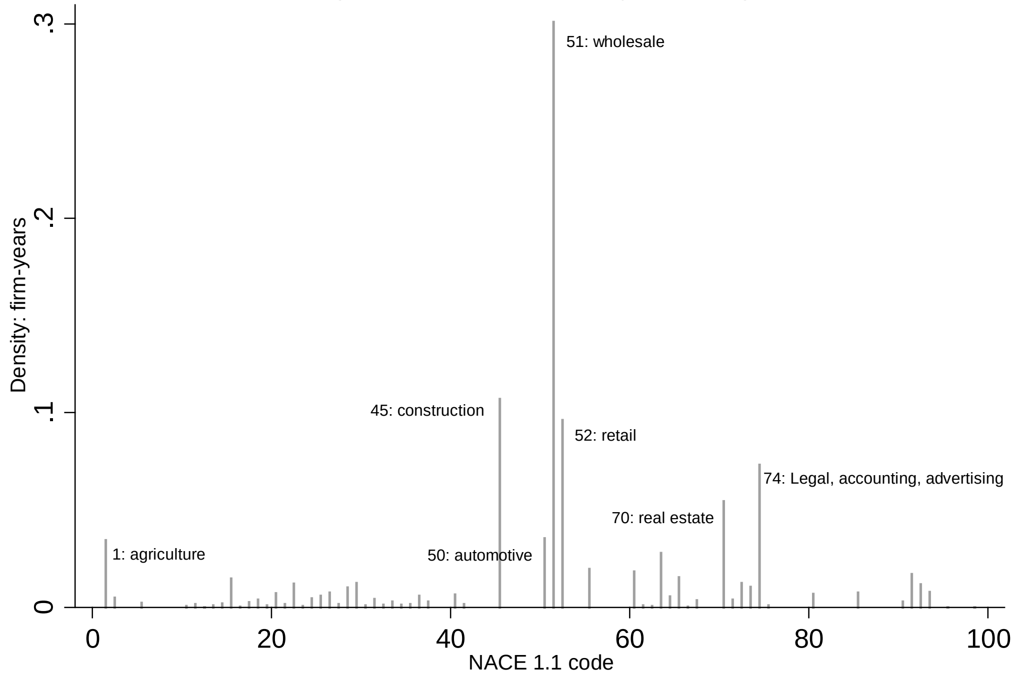

Figure 4: SKRIN Coverage: Imputed Value Added by Industry

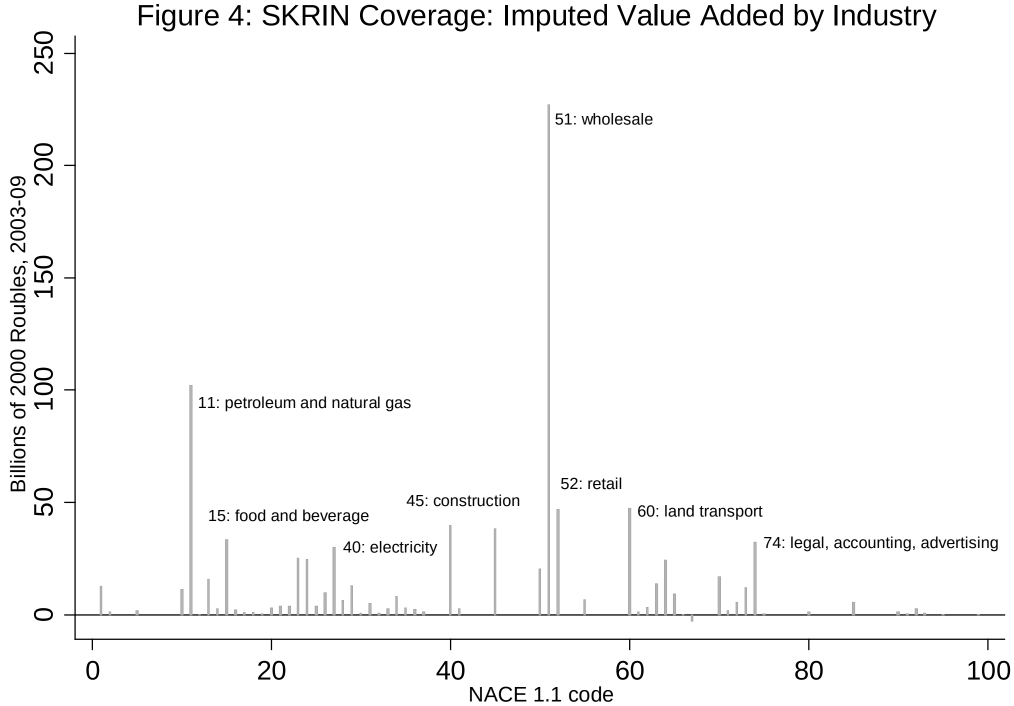

Figure 5: TFPR by margin

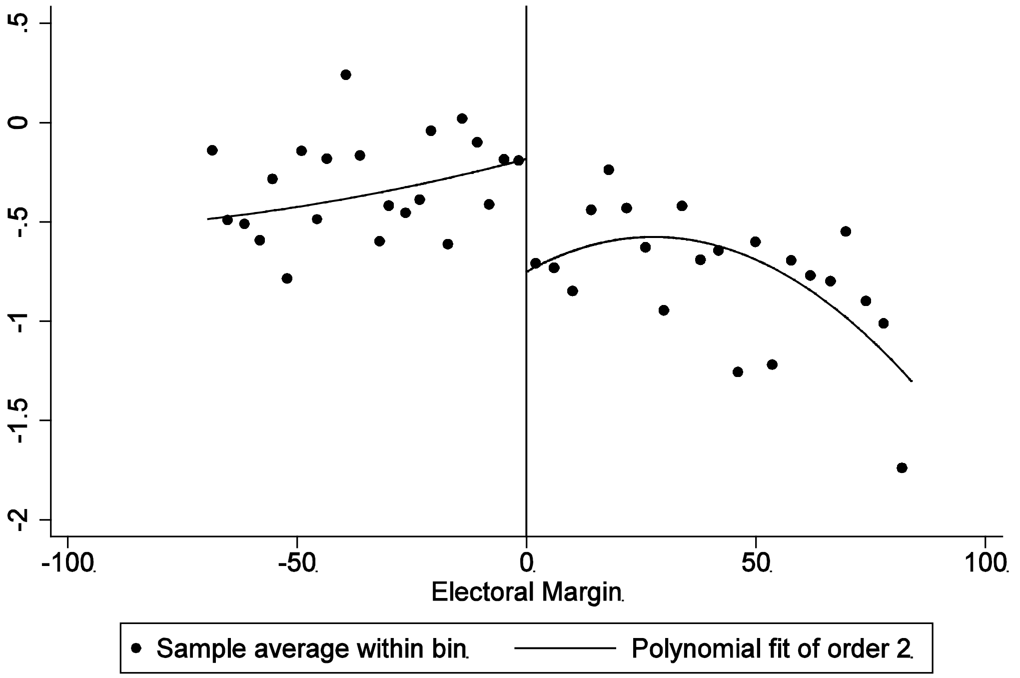

Figure 6: TFPR vs margin with varying bandwidths

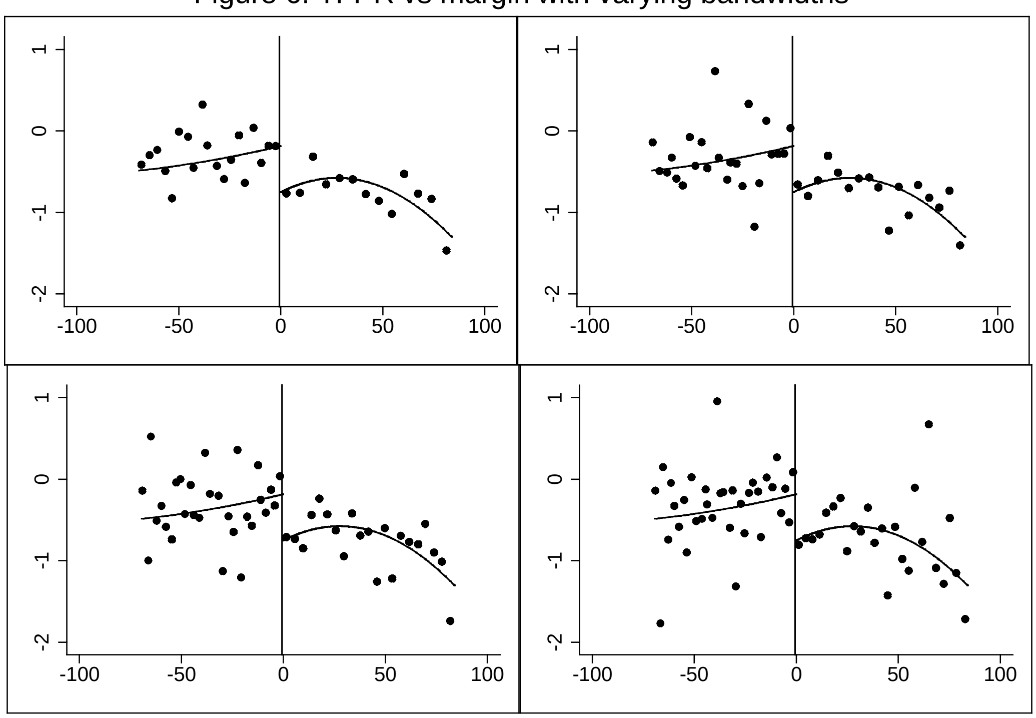

Figure 7: TFPR vs margin with polynomials of increasing degree

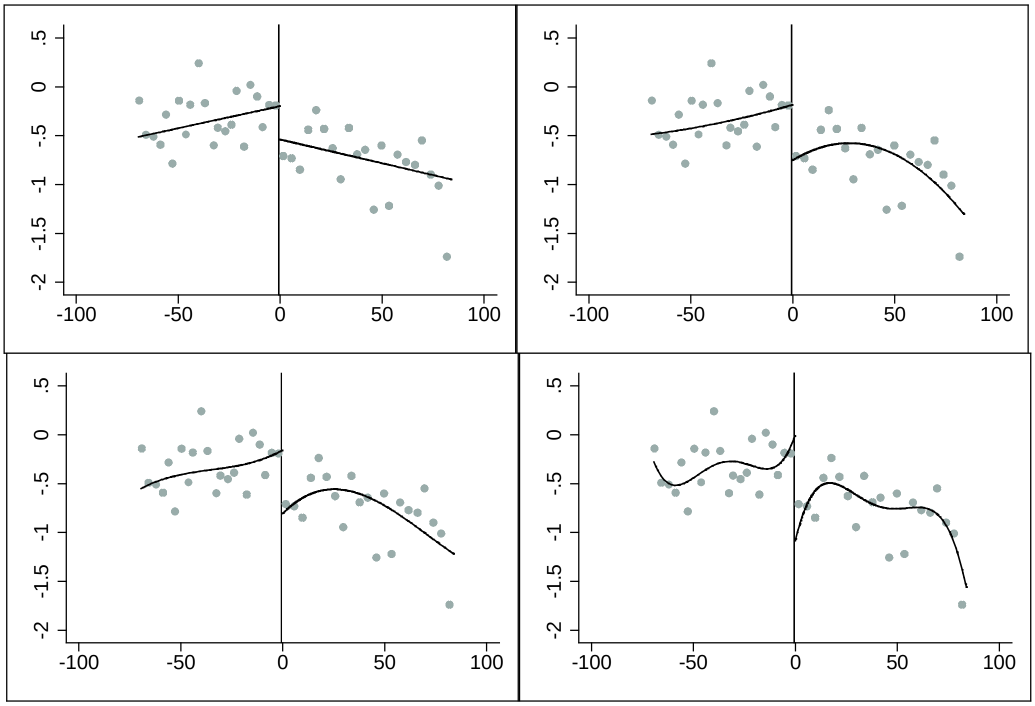

Figure 8: Covariates vs. margin

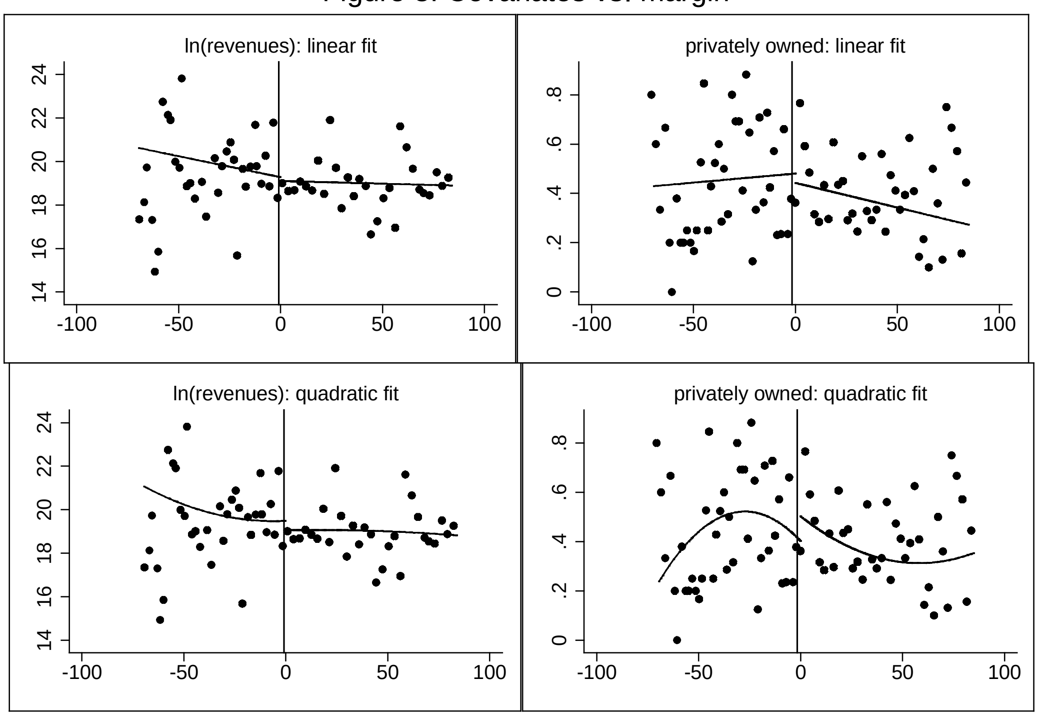

Figure 9: Density of the running variable

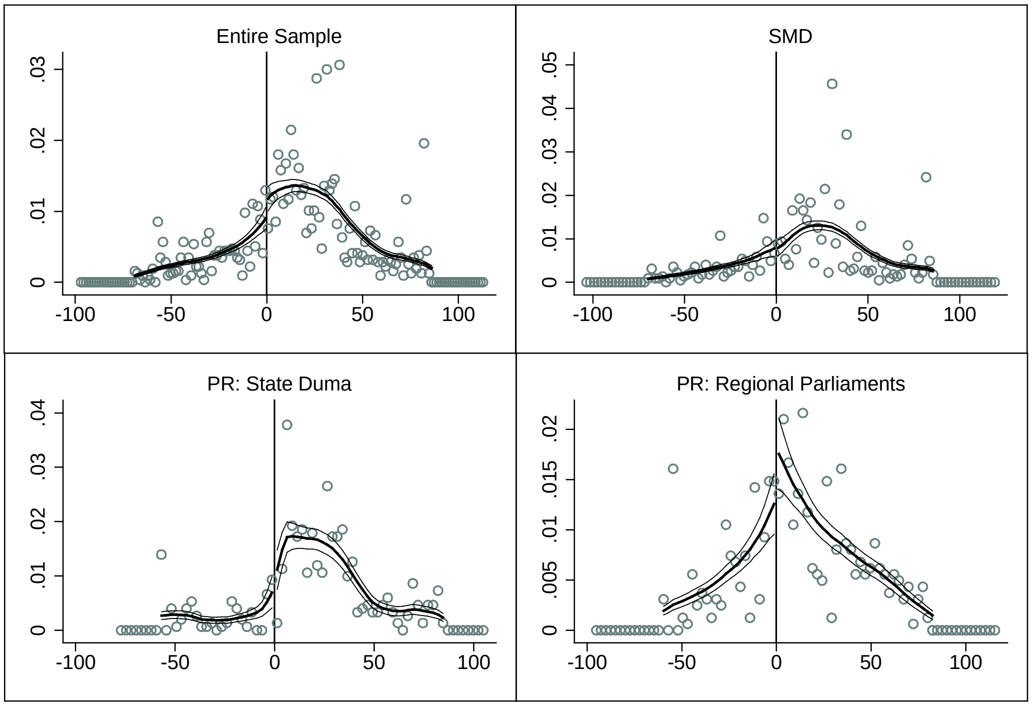

Figure 10: Baseline RD: coefficients and 95% confidence intervals at varying bandwidths.

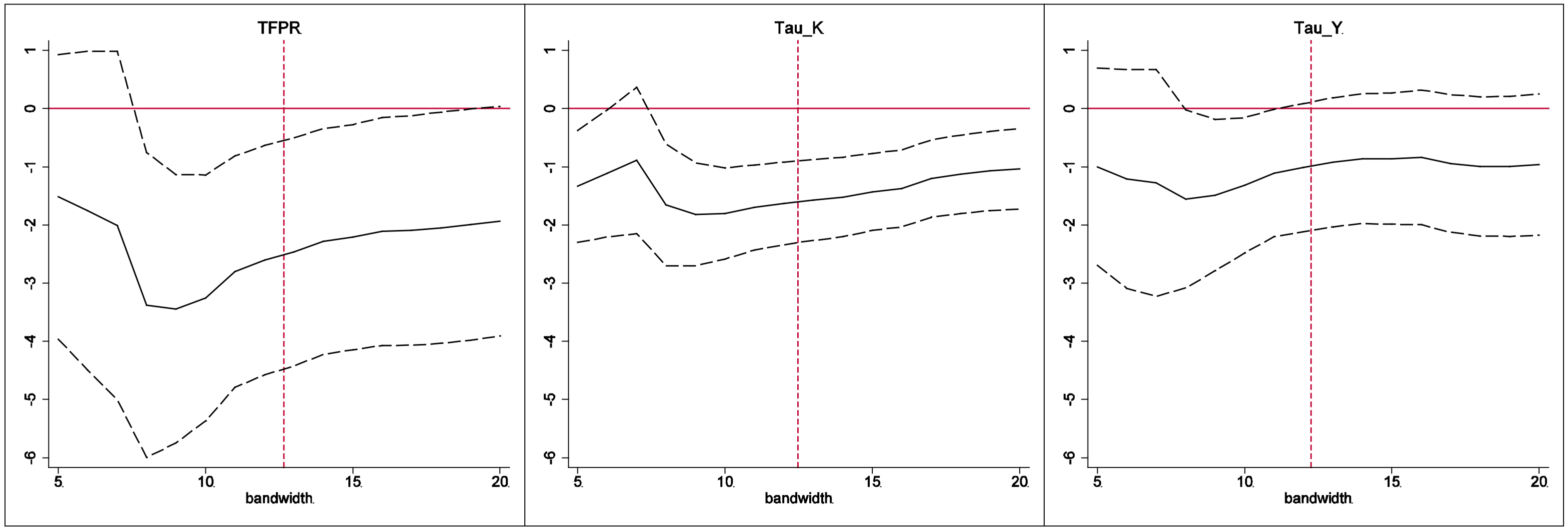

Figure 11: RD with covariates: coefficients and standard errors for varying bandwidths.

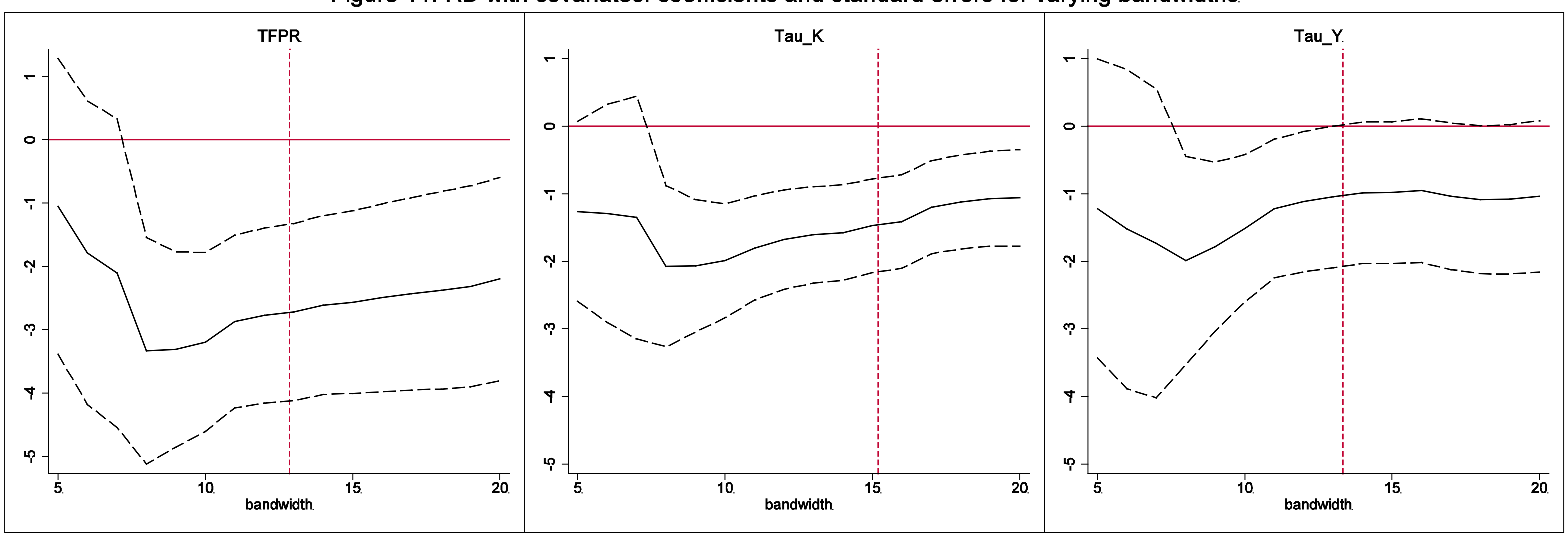

Figure 12: Density of Electoral Margin: All Candidates for SMD

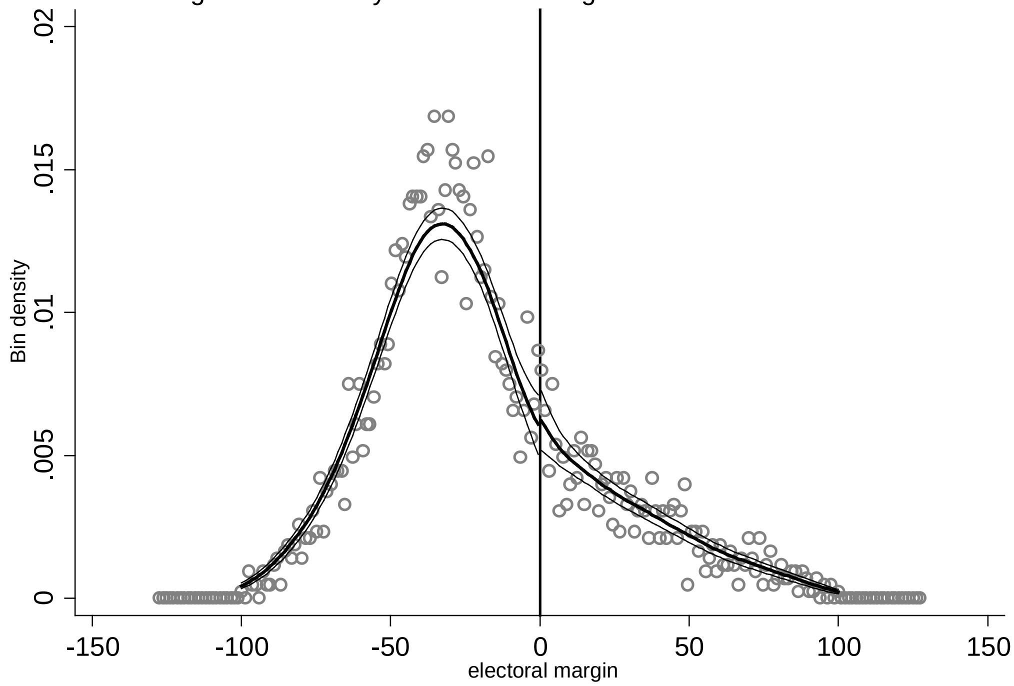

Table 1: Interquartile Ratio of TFPR		
Year	**All**	**Manufacturing**
2003	10.26	2.71
2004	7.05	2.63
2005	7.04	2.58
2006	6.98	2.47
2007	6.27	2.43
2008	5.64	2.39
2009	4.90	2.48
2010	5.21	2.37
2011	5.01	2.34

Table 2: Panel regressions show connected firms are moderately less productive

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Dependent variable	TFPR	MRPK	Tau K	Tau Y	TFPR	MRPK	Tau K	Tau Y
Political connection defined as	current				current or past			
Estimator	Panel random effects with standard errors clustered by ownership type							
Connected	-0.090***	-0.140***	-0.048***	-0.015	-0.100***	-0.113***	-0.058***	-0.026
	(0.022)	(0.027)	(0.014)	(0.023)	(0.016)	(0.024)	(0.014)	(0.020)
log (revenues)	0.117***	0.066	-0.045***	0.089***	0.117***	0.065	-0.045***	0.090***
	(0.025)	(0.051)	(0.005)	(0.005)	(0.025)	(0.051)	(0.005)	(0.005)
Constant	-2.712***	-3.487***	0.662***	-1.434***	-2.714***	-3.480***	0.669***	-1.440***
	(0.473)	(0.967)	(0.099)	(0.106)	(0.475)	(0.967)	(0.100)	(0.107)
year FE	yes	yes	yes	yes	yes	yes	yes	yes
N	47537	44322	43729	44721	47797	44581	43965	44958

Table 3: Baseline RD results

	[1]		[2]		[3]	
Dependent Variable	TFPR		Tau_K		Tau_Y	
Estimator	Local polynomial (fuzzy) regression discontinuity estimation with robust bias-corrected standard errors and coverage-error optimal bandwidth selector					
First stage						
electoral margin	0.852***		0.898***		0.855***	
	(0.160)		(0.157)		(0.156)	
Second stage						
Coefficient	-1.914**		-1.058***		-0.854*	
Std Error	(0.878)		(0.318)		(0.516)	
95% confidence upper	-0.191		-0.435		0.158	
95% confidence lower	-3.640		-1.681		-1.865	
robust p-value	0.029		0.001		0.098	
	Left	**Right**	**Left**	**Right**	**Left**	**Right**
Obs	275	607	259	558	269	591
Effective Obs	88	157	86	140	83	151
Order of Local Polynomial	2	2	2	2	2	2
Bandwidth	12.664	12.664	12.480	12.480	12.263	12.263

Table 4: Robust to changes in parameters

	[1]		[2]		[3]		[4]	
Dependent Variable	TFPR		TFPR		TFPR		TFPR	
Estimator	Local polynomial (fuzzy) regression discontinuity estimation with robust bias-corrected standard errors and coverage-error optimal bandwidth selector							
TFPR calculation parameters								
R	10%		8%		10%		8%	
σ	3		3		4		4	
First stage								
electoral margin	0.852***		0.852***		0.852***		0.852***	
	(0.160)		(0.160)		(0.160)		(0.160)	
Second stage								
Coefficient	-1.914**		-1.892**		-1.920**		-1.898**	
Std Error	(0.878)		(0.860)		(0.875)		(0.857)	
95% confidence upper	-0.191		-0.206		-0.205		-0.218	
95% confidence lower	-3.640		-3.578		-3.635		-3.578	
robust p-value	0.029		0.028		0.028		0.027	
	Left	**Right**	**Left**	**Right**	**Left**	**Right**	**Left**	**Right**
Obs	275	607	275	607	275	607	275	607
Effective Obs	88	157	88	157	88	157	88	157
Order of Local Polynomial	2	2	2	2	2	2	2	2
Bandwidth	12.664	12.664	12.631	12.631	12.628	12.628	12.671	12.671

Table 5: Robust to order of polynomial

	[1]		[2]		[3]		[4]	
Dependent Variable	TFPR		TFPR		TFPR		TFPR	
Estimator	Local polynomial (fuzzy) regression discontinuity estimation with robust bias-corrected standard errors and coverage-error optimal bandwidth selector							
First stage								
electoral margin	0.832***		0.852***		0.838***		0.796***	
	(0.148)		(0.160)		(0.170)		(0.178)	
Second stage								
Coefficient	-1.627*		-1.914**		-2.259**		-2.582**	
Std Error	(0.858)		(0.878)		(0.927)		(1.044)	
95% confidence upper	0.056		-0.191		-0.442		-0.536	
95% confidence lower	-3.309		-3.640		-4.076		-4.628	
robust p-value	0.058		0.029		0.015		0.013	
	Left	**Right**	**Left**	**Right**	**Left**	**Right**	**Left**	**Right**
Obs	275	607	275	607	275	607	275	607
Effective Obs	88	157	88	157	97	215	109	233
Order of Local Polynomial	1	1	2	2	3	3	4	4
Bandwidth	7.699	7.699	12.664	12.664	16.656	16.656	19.004	19.004

Table 6: Robust to inclusion of covariates

	[1]		[2]		[3]		[4]		[5]		[6]	
Dependent Variable	TFPR		TFPR		TFPR		TFPR		Tau_K		Tau_Y	
Estimator	Local polynomial (fuzzy) regression discontinuity estimation with robust bias-corrected standard errors and coverage-error optimal bandwidth selector											
First stage												
electoral margin	0.852***		0.865***		0.881***		0.814***		0.856***		0.805***	
	(0.160)		(0.160)		(0.162)		(0.154)		(0.144)		(0.154)	
Second stage												
Coefficient	-1.914**		-2.081**		-2.072**		-2.018***		-0.998***		-0.981**	
Std Error	(0.878)		(0.849)		(0.840)		(0.591)		(0.318)		(0.491)	
95% confidence upper	-0.191		-0.434		-0.428		-0.860		-0.438		-0.0182	
95% confidence lower	-3.640		-3.614		-3.569		-3.175		-1.622		-1.945	
robust p-value	0.029		0.014		0.014		0.001		0.002		0.046	
Covariates			log (revenues)		log (revenues) Fully Private		log (revenues) Fully Private Industry Dummies		log (revenues) Fully Private Industry Dummies		log (revenues) Fully Private Industry Dummies^	
	Left	**Right**	**Left**	**Right**	**Left**	**Right**	**Left**	**Right**	**Left**	**Right**	**Left**	**Right**
Obs	275	607	275	607	275	607	272	602	256	553	266	587
Effective Obs	88	157	88	157	88	157	87	153	87	173	82	158
Order of Local Polynomial	2	2	2	2	2	2	2	2	2	2	2	2
Bandwidth	12.664	12.664	10.581	10.581	10.394	10.394	12.04	12.04	14.89	14.89	13.326	13.326

^ the finance dummy is dropped else the rd is unable to compute on account of insufficient variability

Table 7: Robust to removal of suspected fraud

	[1]		[2]		[3]	
Dependent Variable	TFPR		TFPR		TFPR	
Sample	All		Drop United Russia		Drop Suspect Regions^	
Estimator	Local polynomial (fuzzy) regression discontinuity estimation with robust					
First stage						
electoral margin	0.852***		0.813***		1.066***	
	(0.160)		(0.160)		(0.090)	
Second stage						
Coefficient	-1.914**		-1.628*		-2.083**	
Std Error	(0.878)		(0.953)		(0.873)	
95% confidence upper	-0.191		0.240		-0.371	
95% confidence lower	-3.640		-3.496		-3.795	
robust p-value	0.029		0.088		0.017	
	Left	**Right**	**Left**	**Right**	**Left**	**Right**
Obs	275	607	111	537	124	293
Effective Obs	88	157	54	161	48	76
Order of Local Polynomial	2	2	2	2	2	2
Bandwidth	12.664	12.664	14.779	14.779	12.331	12.331

^ Suspected of more than 15% vote fraud in the 2011 Duma elections according to Kireev's analysis

Table 8: Estimated GDP effects of endowing all firms with zero-step connections		
year	2010	2011
	Efficiency gain from eliminating connections	
Point estimate	28.8%	31.3%
95% Confidence Interval	34.3%	38.2%
	15.5%	16.1%