Corruption in Procurement and Shadow Campaign Financing: Evidence from Russia*

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Abstract

Using a measure of tunneling out of Russian firms, constructed from banking transactions data as transfers of cash out of legitimate to fly-by-night firms, we show that firms that get public procurement contracts exhibit abnormally high tunneling rates within four weeks of regional elections. In contrast, firms with no public procurement revenue exhibit no tunneling around election cycles. Since politicians on the campaign trail need cash the most around election time, they are the most likely recipients of tunneled cash close to elections. Using variation in the quality of tax inspectors as a source of exogenous variation in tunneling, we document a causal relationship from tunneling around elections to obtaining public procurement contracts. Two placebo experiments–using placebo election dates and legitimate rather than shadow transfers–confirm the validity of our empirical strategy. Our estimates yield a locality-level measure of corruption in public procurement. Using this measure, we reject the "efficient greasing" hypothesis by showing that, in more corrupt localities, public procurement contracts are allocated to less efficient firms, and therefore, corruption has negative welfare implications.

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1 Introduction

Corruption is a widespread phenomenon in the developing and transition world. Despite this being a global consensus shared by politicians, media, public opinion, and international organizations such as the World Bank and the European Bank for Reconstruction and Development, convincing systematic evidence of corruption is still scarce in the academic literature (see, for instance, surveys by Bardhan, 1997; Rose-Ackerman, 1999; Svensson, 2005). The last two decades saw a sharp increase in the body of research focusing on measuring corruption. Most measures of corruption, however, are based on perceptions, such as expert opinions or surveys, in which individuals and firm managers are questioned about their assessment of corruption in their respective environments (e.g., neighbors and competitors). Due to the secretive nature of corruption, in most cases surveys asking direct questions on whether individuals or firms pay bribes to government officials in exchange for political favors are ineffective and their results understate the depth and breadth of the corruption phenomenon (for difficulties of survey designs attempting to measure corruption see Reinikka and Svensson, 2006).

Recently, the literature turned to evaluating corruption using policy experiments, for instance, by comparing the amount of transfers disbursed from a specific federal grant measured at the source to the amount that actually reaches the intended recipients of the transfer (e.g., Reinikka and Svensson, 2004; Olken, 2006), and field experiments, which randomize incentive schemes for corrupt behavior (e.g., Bertrand, Djankov, Hanna and Mullainathan, 2007; Olken, 2007). Experiments that allow evaluation of the scale of corruption are rare and often cover a very specific area of corrupt economic activities.

The goal of our paper is to provide a reliable measure of corruption in public procurement contracts for the near-population of Russian firms and assess the welfare implication of corruption. For this purpose, we measure the amount of cash tunneled illegally out of firms around the time of regional elections over a six-year period and relate it to the probability that the firms obtained procurement contracts from the government.

The data that made this research possible come from a list of banking transactions of the near-population of business entities in Russia over a 6-year period, leaked from the Central Bank of Russia and sold on the Internet. For a limited time these data were also available to download for free on the Russian analogue of WikiLeaks, *www.RusLeaks.com*. We identify tunneling (Johnson et al., 2000), i.e., the amount of transfers to fly-by-night firms set up to take cash out of companies, at each point in time for each legitimate firm. We apply the intuitive criterion that legitimate firms are those that pay taxes, whereas fly-by-night firms are those that do not pay taxes, but should be doing so according to Russian law.

Taxes are easily observable as they show up among a firm's banking transactions. Using difference-in-differences methodology on data of the weekly frequency for all legitimate firms in all of Russia's regions with regional elections, we show that for firms that get public procurement contracts, tunneling exhibits a strong political cycle (i.e., transfers to fly-by-night firms increase sharply around regional elections). In contrast, there is no political cycle in tunneling out of firms without public procurement revenue. Since politicians on the campaign trail are those who need cash around elections the most, we conclude that firms with public procurement contracts finance election campaigns using black cash.

We verify the validity of our estimates with two placebo experiments: We show that there is no political cycle in 1) banking transactions between legitimate firms for the actual election dates, and 2) in banking transactions to fly-by-night firms for placebo election dates. We estimate the amount of shadow campaign financing associated with corrupt distribution of public procurement to be around 2.5 million U.S. dollars for an average election campaign in an average Russian region. An average, firms with public procurement contracts in Russia tunnel out about 30,000 U.S. dollars more at election time compared to non-election periods.

The case of the Moscow-based company Inteko, owned by Yelena Baturina, the wife of the former mayor of Moscow, Yury Luzhkov, illustrates that the amount of shadow campaign financing by an average recipient of public procurement contracts in an average region is substantially smaller than those for the most notorious corruption cases. According to *Forbes*, in 2010, Yelena Baturina was the richest woman in Russia and the third richest woman in the world. Allegedly, she made her fortune through procurement contracts and concessions allocated to her company, Inteko, by Moscow city government at the time when Baturina's husband was the mayor of Moscow (between 1992 and 2010).[1] Figure 1 presents outlays from Inteko to fly-by-night firms in the banking transactions data during the period starting six months before the election of Moscows mayor in December 2003 and ending six months after the Russian presidential election of March 2004. The data show that tunneling activity is concentrated around elections. In particular, the two biggest incidents of transfers to fly-by-night firms from Inteko, in the amounts of 2.8 million U.S. dollars and 4 million U.S. dollars, occurred one week before the Moscow mayoral election and one month before the presidential elections, respectively. Altogether, in five consecutive months around these two elections (from November 2003 to March 2004), Inteko tunneled 10.3 million U.S. dollars to fly-by-night firms. This is an order of magnitude larger than the total sum tunneled during the five months preceding November 2003 and the five months starting April 2004. (The

[1] *The New York Times* and *Forbes* magazine published a series of articles on Yelena Baturina and the source of her fortune. See, for instance, the following links: http://topics.nytimes.com/top/reference/timestopics/people/b/yelena_baturina/index.html; http://www.forbes.com/profile/elena-baturina/ and http://en.rian.ru/russia/20100615/159431047.html.

exact sum tunneled in these ten months is 1.1 million U.S. dollars). An average region in Russia is much poorer and has substantially smaller rents than the capital city of Moscow.

The Inteko case illustrates that political connections are important in explaining a part of the observed correlation between tunneling around elections and the allocation of public procurement contracts. The next step in our analysis is to show that political connections are not the only mechanism. For this purpose, we find a source of exogenous variation in tunneling unrelated to political connections; namely, the variation in the enforcement capacity of tax agencies within regions. We establish a causal effect of shadow transfers on the probability of getting public procurement contracts and on the amount of procurement revenue that firms receive following the election. We find that an increase in black cash delivered to politicians increases a firms procurement revenue as well as the probability of obtaining public procurement contracts. In other words, shadow campaign financing pays off. As a reality check on our measure of corruption, we verify that an increase in the level of regional corruption measured with a standard Transparency International perception-based index is associated with a significantly higher correlation between firms' shadow transfers around elections, on the one hand, and the amount of their procurement revenue, on the other hand.

Finally, we study how the efficiency of the allocation of public procurement depends on corruption. We measure the level of corruption in each locality as the strength of the correlation between shadow transfers and the probability of winning public procurement contracts. Using the variation in this measure of corruption across different localities within a region, we show that in more corrupt environments, public procurement contracts are allocated to less productive firms, controlling for region and industry fixed effects. We conclude that corruption has negative welfare implications and is not just an example of "efficient greasing."

Our main contribution is to the literature on corruption and its implications for welfare (e.g., Shleifer and Vishny, 1993, 1994). In particular, we contribute to the recent strand of empirical literature that attempts to provide systematic evidence of corruption using objective rather than perception-based measures (see, for instance, Reinikka and Svensson, 2004; Bertrand, Djankov, Hanna and Mullainathan, 2007; Olken, 2007; Fisman and Miguel, 2007, as well as other works surveyed by Svensson, 2005). We provide objective evidence of corruption in the allocation of public procurement contracts for a comprehensive list of Russian firms and show that corruption exacerbates inefficiencies. Previous estimates of corruption in Russia were based on perceptions and cover a much smaller segment of economic activity. Our study is also related to the large body of work on corruption associated with political connections (e.g., Fisman, 2001; Johnson and Mitton, 2003; Bertrand, Kramarz,

Schoar and Thesmar, 2007; Khwaja and Mian, 2005; Faccio, 2006; Faccio, Masulis and McConnell, 2006): Political connections drive a part of the presented OLS evidence as is illustrated by the case of Inteko. We, however, also present IV evidence of a causal link betweenshadow campaign financing and the allocation of procurement contracts, which is not driven by the presence of political connections.

We also contribute to the literature on opportunistic political cycles (see, for instance, a survey by Drazen, 2001). This literature focuses primarily on in the correspondence between election cycles and benefits directed to voters (in the form of transfers and social expenditure). We document a political cycle in cash bribes that firms pay to politicians in order to obtain procurement contracts. Our findings are related to Bertrand, Kramarz, Schoar and Thesmar (2007), who document that political connections are associated with political cycle in employment granted for the political benefit of incumbent politicians. Our work is also related to the papers documenting political budget cycles for Russian gubernatorial elections (Akhmedov and Zhuravskaya, 2004) and state capture at the regional level in Russia (Slinko, Yakovlev and Zhuravskaya, 2005; Guriev, Yakovlev and Zhuravskaya, 2010).

Our paper proceeds as follows. In section 2, we describe the data. Section 3 presents evidence of political cycle in illegal cash tunneled from companies that receive public procurement revenue, and estimates the size of shadow campaign financing. Section 4 presents evidence of the causal relationship between shadow campaign financing and receiving procurement revenue. In section 5, we show that corruption leads to inefficiency in the allocation of public procurement. In section 6, we conclude.

2 Data

Our aim is to test for a relationship between transfers to fly-by-night firms from regular firms around elections, and the public procurements contracts that regular firms receive. Thus, the amounts of tunneling and public procurement revenue are the two main variables in our analysis. Both of these variables are constructed from the list of banking transactions between 1999 and 2004 that was leaked to the public from the Russian Central Bank in 2005 and sold on the Internet. The data for 2003 and 2004 were purchased on-line from *www.vivedata.com* and were used by Mironov (2011); the data for 1999-2002 were purchased from *www.rusbd.com*.[2] For a detailed description of these data and many reality checks on them see Mironov (2011). The data set contains $513,169,660$ transactions involving $1,721,914$ business and government entities, with information on the date of each transac-

[2]The Russia business daily *Vedomosti* (2005a) discussed the incident of leakage of these data on March 30, 2005 in an article entitled "Pirates unraveled bank secrecy."

tion, its payer, recipient, the amount of each transaction, and the self-reported purpose of it.

First, we follow Mironov (2011) and use these banking transactions data to measure the amount of transfers to fly-by-night firms each week in each of the years between 1999 and 2004 for each firm in our sample, which amounts to a near-population of large and medium-size legitimate firms in Russia (the construction of the sample is described below). Mironov (2011) developed the methodology of identifying fly-by-night firms, i.e., firms that have profitable banking transactions but pay no taxes, in the banking transactions data set. Intuitively, fly-by-night firms are those that do not pay taxes despite having transactions that require the payment of taxes according to Russian law. To be precise, firms are defined as fly-by-night when they satisfy all of the following three criteria: (i) the ratio of taxes paid to the difference in cash inflows and outflows is negligible (i.e., below 0.1%); (ii) social security taxes are below the amount which corresponds to the social security tax for a firm with one employee on a minimum wage (i.e., \$7.2); and (iii) cash inflows are higher than cash outflows. In contrast to fly-by-night firms, regular (or legitimate) firms are commercial entities that engage in commercial transactions and pay taxes. According to these criteria, we identified 99,925 fly-by-night firms and 166,381 regular firms among the business entities in the banking transactions data. (Note that the vast majority of these regular firms are small businesses.) For the purposes of this paper, we deem all the transfers from regular firms to the fly-by-night firms as tunneling, or "shadow transfers."

Second, we use the banking transactions data to identify revenue from public procurement contracts for each firm in our sample (described below). We define revenue from public procurement contracts as the amount of all banking transactions from government-affiliated entities to regular firms that have the reported purpose of "payment for goods and services." In the baseline analysis, we exclude payments for utilities such as electricity and water from the list of revenues from public procurement contracts because the utilities contracts are not usually allocated on a competitive basis and are automatically allocated to local monopolists. The inclusion of utilities in the definition of public procurement does not affect our main results.

We also collect data on the basic characteristics of regular firms, such as location, revenue, net income, debt, assets, employment, and industry, which we use as control variables. These data come from the registry of Russian firms published by the Russia's official statistical agency (*Rosstat*). The most recent registry contains data on near-population of industrial firms in Russia in 2003. We merge regular firms from the banking transaction database with the registry data.

Since we are interested in estimating the electoral cycle in shadow transfers, we focus on

the 87 (out of 89) Russian regions that held gubernatorial elections between 1999 and 2004. The two excluded regions are Dagestan, which has a parliamentary form of government, and Chechnya, which experienced a severe armed conflict in 1999-2000. In the 87 regions, over the period under study, 129 elections took place at 48 different points in time as shown in the column 1 of Table A.2.

We construct our sample of regular firms by taking all firms that satisfy the following criteria from these regions in their election years:

1. A firm should be present both in *Rosstat's* 2003 registry and the banking transactions database.

2. A firm's revenue should be greater than \$1M in 2003. We apply this criterion because one can expect only relatively large firms to finance elections, and the registry data can be considered as a near-population representative sample only for large firms.

3. A firm should have at least 10 transactions in the banking transactions dataset over the entire period. As our measures of revenue from public procurement contracts and of the transfers to fly-by-night firms are based on the banking transactions data, we apply a minimum threshold for the number of transactions.[3]

These criteria yield $52,073$ firms. In order to assess the representativeness of the sample, we compare the revenue of these firms to the total revenue generated by all Russian firms (including the small ones, which are excluded from our sample). The total revenue of the firms in our sample constitutes 78.4% of the total revenue for all firms in the Russian economy. For the analysis of the political cycle in shadow transfers within an election year, we focus on firms that made at least some transfers to fly-by-night firms during the two years around an election date.[4] This additional criterion reduces the sample to $32,735$ firms.

For the analysis of the causal effect of shadow transfers on public procurement using instrumental variables, the sample includes firms irrespective of whether they made any shadow transfers. In our main specification, as described below, we apply an additional criterion for this sample: we include only those firms for which the actual and legal addresses coincide. This criterion yields $25,108$ firms, with revenue equal to 41.6% of the total revenue for all firms in the Russian economy. The results of instrumental variables regressions are robust to using the full sample of $52,073$ firms as well, but exogeneity of the instruments (to be described below) relies on the assumption that the firms' registration address is not

[3]As described in Mironov (2011), some transactions by regional firms are missing because the banking data was leaked from the Moscow branch of the Russian Central Bank.

[4]We require that the ratio of transfers to fly-by-night firms to the firm's revenue be greater than 0.001.

chosen strategically and therefore applies only to the sub-sample of firms with the same actual and legal address.

In the appendix, we present summary statistics for the entire sample (Table A.1) and separately for each region (Table A.2). All nominal variables are expressed in thousands of constant 2003 U.S. dollars. A detailed description of variables can be found in the Data Appendix.

3 Political Cycle in Tunneling

3.1 Empirical strategy

Our first task is to estimate the electoral cycle in tunneling (i.e., shadow transfers) for firms with and without public procurement contracts. We regress the shadow transfers each firm in our sample makes each week during the election year (between 1999 and 2004) normalized by the total amount of shadow transfers made during the two years around each election on the set of dummies indicating the distance to the election date, controlling for firm and week fixed effects. We allow the electoral cycle to vary between two groups of firms: those which do and do not receive substantial revenue from public procurement contracts, as we are interested in the difference in the magnitude of the electoral cycle between the two groups. As larger firms may have higher capacity to finance elections, we also allow for differential electoral cycles depending on the size of firm's revenue. The unit of analysis here is a firm in a particular week. Altogether there are $2,380,669$ firm-week observations in the sample, i.e., firm-weeks in each region in the two years around each election among firms with non-zero shadow transfers (there are $32,735$ such firms). To be precise, we estimate the following equation:

$$\frac{ST_{ft}}{\overline{ST}_{fe}} = \sum_{w=-20}^{20} \beta_w^1 D_w Gov_{fe} + \sum_{w=-20}^{20} \beta_w^2 D_w + \sum_{w=-20}^{20} \beta_w^3 D_w \log(R_f) + \sum_{l=0}^{2} \beta_l^4 \log(I_{f,t-l}) + \tau_t + \phi_f + \varepsilon_{ft}, \tag{1}$$

where f indexes firms; t indexes time in weeks (there are 313 weeks over the entire time period under study). The index w refers to the time-distance to the election date in the region where the firm f is located, so that $w = -1$ refers to the week before the election and $w = 1$ refers to the week after the election. D_w is the dummy indicating the week that is w weeks away from the election date. ST_{ft} is the transfer by firm f to fly-by-night firms at time t. (ST stands for "shadow transfer"). $\overline{ST}_{fe}$ is the total annualized transfer

by firm f to fly-by-nights during the two years around elections (i.e., +/- one year from the election date). And e indexes elections in a particular region. Thus, it is redundant for all regions where there was only one election, and meaningful for regions where there were two elections between 1999 and 2004. Gov_{fe} is a dummy which equals 1 if the revenue from public procurement contracts as a share of the firm's f total annualized revenue +/- one year from the election e is greater than a certain threshold. As a baseline, we consider the 5% threshold. To check the robustness of our results, we repeat the analysis redefining the Gov_{fe} dummy as having 1% of revenue coming from public procurement contracts. $\log(R_f)$ is a measure of firm size, namely, the logarithm of the firm's revenue in 2003.[5] In addition, we control for cash inflows into the firm bank account $\log(I_{ft})$ along with two lags of this variable. This control is needed to make sure that the timing of inflows is not driving our results on the dynamics of outflows to fly-by-night firms. τ_t and ϕ_f are the full sets of time and firm fixed effects. Our results are robust to excluding controls for the differential political cycle depending on the size of the firm, i.e., $D_w \log(R_f)$ and to excluding controls for cash inflows, i.e., $\log(I_{f,t})$, $\log(I_{f,t-1})$, and $\log(I_{f,t-2})$. The main results (i.e., the coefficients $beta_w^1$ and $beta_w^2$) are unaffected by the deletion of any of the additional covariates from the regression equation, and by the choice of the threshold of revenue coming from procurement. The error term ε_{ft} is clustered at the level of each of $32,735$ firms.

In order to allow for the differential electoral cycle in shadow transfers, depending on the extent to which firms rely on public procurement contracts for their business, we also estimate the following equation:

$$\frac{ST_{ft}}{R_f} = \sum_{w=-20}^{20} \gamma_w^1 D_w \frac{ProcR_{fe}}{R_f} + \sum_{w=-20}^{20} \gamma_w^2 D_w + \sum_{w=-20}^{20} \gamma_w^3 D_w \log(R_f) + \sum_{l=0}^{2} \beta_l^4 \log(I_{f,t-l}) + \tau_t + \phi_f + \varepsilon_{ft}, \tag{2}$$

where, as a dependent variable we take the firm's weekly transfer to fly-by-night firms normalized by the size of the firm's revenue, i.e., $\frac{ST_{ft}}{R_f}$. $ProcR_{fe}$ stands for the size of the firm's procurement revenue +/- one year around the election date (in annualized terms, i.e., divided by 2); and therefore, $\frac{ProcR_{fe}}{R_f}$ is the share of annualized revenue from public procurement in the two years around electionsas a fraction of the firm's total revenue as of 2003. The rest of the notation is as above. Again, to insure robustness, we estimate this equation with and without controlling for the differential electoral cycle in firms of different sizes, and with and without controls for cash inflows. The inclusion or exclusion of this control does not affect the main result. As above, the error term is clustered at the firm level.

[5]Due to data limitations, our sample size decreases dramatically if we control for revenue in the election year rather than in 2003. The results are robust to this alteration. As a baseline, we report results for the larger sample.

In Specifications (1) and (2), the differences between coefficients on D_w between weeks close to and far away from election dates estimate the electoral cycle in shadow transfers for firms with procurement revenue below the specified threshold (β_w^2 in Equation 1, and for firms with zero public procurement revenue (γ_w^2 in Equation 2. Our main coefficients of interest are β_w^1 and γ_w^1: the coefficients β_w^1 of Equation 1 estimate the difference in the electoral cycles in shadow transfers between firms with procurement revenue above and below the threshold; and the coefficients γ_w^1 of Equation 2 estimate the additional electoral cycle in shadow transfers for an incremental increase in the share of revenue coming from public procurement contracts.

3.2 Results: The Political Business Cycle

Figure 2 presents point estimates of β_w^1 and β_w^2 of Equation 1. Table A.3 in the Appendix presents the full regression output along with the F-test for the equality of averages of β-coefficients within the election window of [-4; +4] weeks around elections and outside this election window. We find that while there is some increase in transfers to fly-by-night firms among firms without public procurement contracts (as shown in the lower graph), there is no statistically significant difference between β-coefficients inside and outside election windows of various different sizes. In contrast, firms that have at least 5% of their revenues coming from procurement contracts make abnormally high transfers to fly-by-night firms close to the election date. In particular, we observe a substantial increase in shadow transfers starting three weeks before elections among firms with public procurement contracts (relative to shadow transfers from firms without procurement contracts). The shadow transfers continue to be abnormally high each week until four weeks after the election. The largest spike in shadow transfers from firms with public procurement contracts occurs right after the election, in weeks +2, +3, and +4. The test for significance of an average weekly shadow transfer inside the election window of [-4; +4] weeks around elections, compared to weekly shadow transfer outside this election window yields F-statistic of 28.6 (with p-value of 0.000) for firms with procurement revenue, and 1.59 (with p-value of 0.207) for firms without procurement revenue.

In an average week an average firm transfers 1.9% of its total shadow transfers to fly-by-night firms. Outside of the election window [-4; +4], the difference in the weekly shadow transfers from firms above and below the 5% threshold level of procurement revenue equals -0.15 percentage points, and is insignificant (i.e., firms with public procurement transfer to fly-by-night firms on average slightly less outside the election window). In contrast, inside the election window, the average weekly shadow transfer from firms with public procurement

revenue above the threshold is 2.8%, which is 0.9 percentage points and 47% higher than their average, and 31.7% higher than shadow transfers from firms with procurement revenue below the threshold inside the election window.

Figure 3 presents the point estimates of γ_w^1 and γ_w^2. Table A.4 in the Appendix presents the full regression output. The lower graph (estimates of γ_w^2) confirms that there is no pronounced electoral cycle in shadow transfers among firms with no procurement revenue. One can see a slight increase in the shadow transfers among these firms in the third week after the election, but the magnitude of this increase in rather small. In contrast, the upper graph (i.e., the estimates of γ_w^1) shows that the magnitude of the cycle sharply increases with an increase in the share of revenue coming from procurement. In particular, a ten percent increase in the share of revenue from procurement contracts increases the weekly shadow transfers as a share of total revenue within the window of [-4; +4] weeks around elections by 2.1 percentage points (the mean value of the weekly shadow transfers as a share of revenue is 4.48%). The F-statistic for the difference between average γ_w^1 coefficients inside and outside the election window of [-4; +4] is 25 with p-value of 0.000. Shadow transfers exhibit a strong positive correlation with a firms public procurement revenues throughout the election campaign, starting twelve weeks before the election and ending four weeks after the election. In particular, shadow transfers increase on average by 1.6 percentage points with each ten percent increase in the share of revenue from procurement within the window of [-12; +4] weeks around elections. In contrast, there is zero correlation between the size of revenues from procurement and shadow transfers from week five five weeks after the election onward. Overall, we find strong evidence of a political cycle in transfers to fly-by-night firms from firms with public procurement.

3.3 Interpretation and the Effect of Political Competition

Fly-by-night firms are usually used to transfer large sums into cash illegally for various purposes, such as tax evasion and diverting cash from shareholders to managers and from minority shareholders to majority shareholders (as in Johnson et al., 2000). Mironov (2011) provides evidence that fly-by-night firms are usually registered on stolen passports and do not provide any real services or produce any real goods. When shadow transfers increase around elections, the cash must be tunneled to politicians, as politicians need cash the most during their campaigns. The fact that these shadow transfers increase during elections primarily in firms that rely on contracts with the government for their business suggests that these transfers might be used as informal payments (i.e., bribes) for obtaining procurement contracts.

Even though election campaigns end on the election date, politicians need cash not only before the election but also right afterwards, as many campaign-related services continue until the last minute of the campaign and payment is made right after the election. This is consistent with the increase in shadow transfers both before and right after the election. The timing of the beginning of the electoral cycle presented in Figure 3 (i.e., an increase in shadow transfers starting twelve weeks before the election), is consistent with the finding of Akhmedov and Zhuravskaya (2004), who document that a sharp rise in social budgetary spending occurs three months before regional elections in Russia. Despite the fact that many payments for goods and services delivered for the election campaign, such as printing of advertisement leaflets, T-shirts, or posters, are usually delayed until after the election, the fact that the most pronounced increase in shadow transfers occurs right after the election seems puzzling. This is because some other expenses, such as those for food, drinks, or airtime, as well as direct bribes to voters are usually incurred on the spot. One possible explanation is that firms wait for the electoral uncertainly to be resolved before bringing bribes to the elected governor, and campaigns are usually financed with bank loans and/or an incumbent's own money. The case of Inteko presented in the introduction is consistent with this view, as in that case the Moscow mayor's campaign was financed with family money. In an attempt to test systematically whether this is the case we checked whether the political cycle in shadow transfers shifts backward in time (i.e., towards having a major spike before the election) when the winning margin is very high and, therefore, electoral uncertainly is low. To the contrary, we find that whenever the cycle is present (as we show below, this occurs when the margin of victory is relatively high) the biggest spike in shadow transfers occurs after the election.[6] It might still be the case, however, that some electoral uncertainly is present in *most* elections, including those with a very high *ex-post* margin of victory. Evidently, firms that bribe politicians in order to obtain procurement contracts are not the bearers of this electoral risk.

Furthermore, we explore how the degree of electoral uncertainty and political competition affect the presence of the political cycle in tunneling during campaigns. In 74 of 129 elections the winner got more than 50% of the total vote in the first tour; and in 32 elections the winner got more than 70% of the total vote. In 27 elections the incumbent ran and lost. We re-estimated the political cycle in shadow transfers using Equations 1 and 2 for the sub-samples of elections in which the winner got above and below 50% in the first round, and the incumbent lost, won, and got above and below 70% of the vote. We also confirmed the results by estimating these equations on the full sample with additional interaction terms allowing the cycle to differ between these groups of elections.

[6]The results are available from the authors upon request.

We find that the political cycle in shadow transfers decreases sharply with an increase in political competition. In particular, we do not observe a statistically significant political cycle for elections in which the winner got less that 50% in the first round and in which the incumbent lost. For the purposes of a concise presentation of these results, we re-estimated the cycle simplifying Equations 1 and 2 by replacing forty D_w dummies indicating distance to election with just a single dummy indicating the election window of [-4;+4] weeks around the election. Table 1 summarizes these results, it reports coefficients on the election window dummy and on the interaction between the election window dummy and the dummy for procurement revenue above 5% of total revenue (upper panel), or the share of procurement in total revenue (lower panel). The results presented in the table confirm that there is no pronounced political cycle in tunneling, even among firms with procurement revenue above 5% threshold for elections in which the winner got less that 50% of the vote on the first tour and in which the incumbent lost (see columns 1 and 3 of the upper panel of Table 1). The coefficients in Specification 2 are more precisely estimated and, therefore, the coefficients on the interaction between the size of procurement revenue share and the election window dummy are statistically significant in this specification, even for the elections with a low margin of victory (as reported in the lower panel of Table 1). Yet, their magnitude is about 1/8th that of elections with a larger margin of victory. Remarkably, the magnitude of the cycle is exactly the same (and large) for elections in which the winner won in the first tour and those in which the incumbent won, as long as the share of the incumbent's vote is over 50% (see columns 2, 4, 5, and 6 of the table).

One possible explanation for the absence of political cycle in tunneling among firms with procurement contracts when elections are close is that in these regions, regional governments are more accountable (due to higher political competition) and, therefore, there is less corruption. It is important to note that this is only suggestive, as all the results concerning the effect of political competition measured by the de-facto winning margin on the size of the cycle are subject to a reverse causality problem. The reason for this is that campaign financing, including informal financing through the shadow economy, should have a direct effect on the election results, such that better campaigns run by incumbents should have a better chance of winning. At the same time, sure winners do not need to campaign. Thus, one should be careful in interpreting these results.[7]

In order to check the validity of our results, we conduct two placebo experiments. First, we test for political cycle in transactions among legitimate regular firms (i.e., white transfers)

[7]We have also analyzed how various regional characteristics affect the magnitude of the cycle and found no robust correlations between the magnitude of the cycle with observables except for a positive association between the cycle and perception-based measures of regional corruption, which we report below as a reality check on our exercise.

in manufacturing industries and find no evidence of such a cycle. This is important, as it rules out the possibility that our results are driven by an unobserved increase in legitimate economic activity around election time. Figure 4 illustrates the results. It portrays the dynamics of shadow transfers and transfers to legitimate firms in industries unrelated to publishing and media for our baseline sample of firms. It is evident from the figure that only shadow transfers exhibit political cycle. The table below the figure confirms that the test for equality of coefficients inside and outside the election window of [-4;4] weeks around elections yields statistically significant difference only for shadow transfers in firms with procurement as a share of revenue above the 5% threshold.

Second, to make sure that our standard errors are not too small and our results are not driven by some differential regional trends, we re-estimate cycle in shadow transfers using Equations 1 and 2 for 200 randomly chosen combinations of placebo election dates in our regions. We draw placebo election dates randomly from the time interval that our data cover, at least 16 weeks away from the true election dates. Figure 5 presents the histograms of F-statistic from the test of equality of the means of coefficients of interest β^1 and γ^1) inside and outside the election window (i.e., the tests for $\overline{\beta}^1_{w\in[-4;4]} = \overline{\beta}^1_{w\notin[-4;4]}$ in the upper panel and $\overline{\gamma}^1_{w\in[-4;4]} = \overline{\gamma}^1_{w\notin[-4;4]}$ in the lower panel of the graph). In each of the panels, the vertical line indicates the value of the F-statistic for the same test performed on the true data, which is substantially larger than any of those generated by the placebo treatment. This experiment shows that the pattern in the data that we uncover is very unlikely to be generated by a random realization.

A simple unconditional difference-in-differences exercise can help illustrate the magnitude of the phenomenon. Table 2 summarizes the average amount of transfers to fly-by-night firms per firm in a two-by-two matrix. The rows display firms with and without any public procurement contracts, and the columns display two time periods (an 8-week-long election window and an average 8-week-long period outside the election window). As shown in the table, firms with public procurement had larger shadow transfers both inside and outside the election window. This could be explained by differences in firm size or corporate governance practices between the two groups. In addition, for both groups shadow transfers inside the election window were larger than those outside of it. This could be because politicians demand shadow campaign contribution from all firms. The difference in shadow transfers inside and outside the election window, however, was substantially larger for firms with public procurement: an average firm with public procurement contracts tunneled $30,800$ USD more for an average regional election campaign.

An average region in Russia had 81 firms that received public procurement contracts. Thus, the amount of illegal shadow financing for an average regional election campaign as-

sociated with distribution of public procurement in Russia was about 2.5 million USD.[8] On average, firms that finance elections with shadow transfers (i.e., those firms whose shadow transfers exhibit a political cycle) get about 100,000 USD more revenue from public procurement contracts per year than firms that do not engage in shadow campaign financing. Therefore, a substantial part of these receipts are likely returned to politicians as a kickback in the form of shadow election financing.

4 Does it Pay to Finance Elections?

A significant statistical association between procurement contracts and the size of the electoral cycle in shadow transfers could be interpreted not only as a causal relationship; this association could also be driven by omitted variables. For example, firms with connections to politicians may both be more likely to obtain public procurement contracts (e.g., Amore and Bennedsen, 2010) and be more likely to channel benefits to politicians at the time of elections (e.g., Bertrand, Kramarz, Schoar and Thesmar, 2007). The story of Inteko, the company of the wife of the former mayor of Moscow, which we referred to in the introduction, illustrates this. In this section, we attempt to address the question of causality: Does it pay for firms to finance elections through shadow transfers or, in other words, do shadow transfers help firms obtain public procurement contracts?

In order to understand whether it pays for firms to channel illegal cash to politicians around elections, one needs to find an exogenous source of variation in shadow transfers. We follow Mironov (2011) to assume that tax agencies in Russia differ in terms of the level of tax enforcement. Typically, there are several tax agencies in each region. For regions in our sample, the number of tax agencies varies from 2 in a few smaller regions to 35 in Moscow. Different tax agencies are staffed to a different extent: some have many more tax agents per firm assigned to this particular agency than others. In addition, tax agents differ in skills and incentives across tax agencies. As a result, there is a large variation across tax agencies in the strength of tax enforcement. To verify that this is the case, we collected data on the number of detected purely-technical violations, such as typos in tax statements and delays in tax filing (which are unlikely to be related to deliberate tax evasion by firms), the number of tax inspectors, and the number of firms assigned to each tax agency in the city of Moscow. Using these data, we verify that the number of firms assigned to each tax agent is negatively correlated with the number of technical violations detected, as shown on the Figure 6. The negative correlation between the work load per tax agency employee and the number of

[8] Regional elections in Russia were abolished in 2005. Since then regional governors have been appointed by the Russia president rather than elected.

detected technical faults suggests that there are differences in enforcement capacity across agencies. Since detecting fly-by-night firms that (by definition) do not pay taxes is the direct responsibility of tax agents, we expect shadow transfers by firms assigned to tax agencies with relatively weak enforcement capacity to have an *a priori* lower probability of being detected. Therefore, shadow transfers should be more prevalent in firms assigned to tax agencies with weak tax enforcement.

The direct measures of the quality of tax enforcement are available only for Moscow city, and therefore, we cannot use them for our analysis. However, we can use tax agency dummies as a source of exogenous variation in tax enforcement. It is important to note that the division of regions into areas within region assigned to different tax agencies is unrelated to any other administrative division, and therefore, to the level at which public procurement contracts are allocated among firms.

The assignment of firms to tax agencies depends purely on a firm's official address. An important question is whether firms can move from one tax agency to another depending on its level of tax enforcement, which would invalidate our instruments. The recent well-publicized case of the Hermitage Capital investment fund in Russia suggests that firms take assignment to a particular tax agency as given.[9] Nonetheless, we cannot rule this possibility out completely. To partially address this concern, we eliminate from the sample all the firms that had different actual and legal addresses and consider only firms for which the actual and legal addresses coincide.

Using the assignment to tax agencies as an arguably exogenous source of variation in shadow transfers, we estimate the causal relationship going from the shadow transfers made inside and outside election windows to the likelihood of obtaining procurement contracts and to the size of these contracts. For the purposes of this part of our analysis, the unit of observation is a firm following a particular election episode. Thus, our sample consists of all regular firms with the same actual and legal address (irrespective of whether these firms made any transfers to fly-by-night firms) in regions and years when elections took place. There are, therefore, $25,108$ firms and $41,983$ observations. Firms in regions with a single election between 1999 and 2004 appear in the sample only once, whereas firms in regions with two elections during this period appear twice.

First, we study the relationship between the probability of obtaining procurement con-

[9]See, for instance, the *Wall Street Journal* articles: "Russia Details Tax-Dodge Case Against Investor" of November 26, 2009 and "Swiss Launch Money Laundering Probe In Hermitage Fraud Case" of April 21, 2011 and the *New York Times* articles "Hermitage Seeks U.S. Courts Aid in Russian Case" of July 31, 2009 and "Russian Officials Said to Reap Wealth in Tax Case" of April 18, 2011.

tracts and shadow election financing. We estimate the following linear probability model:

$$Prob[ProcR_{fe} > 0] = \alpha_1 \log(1 + ST_{fe}^{window}) + \alpha_2' X_f + \alpha_3 S_f + \alpha_4 R_f + \tau_e + \varepsilon_{fe}. \quad (3)$$

As a dependent variable we take the dummy indicating firms that received any revenue from public procurement contracts in the year following a particular election e, for which $ProcR_{fe} > 0$. (As shown in Table A.1, the dummy equals one in 13.45% of observations.) The results are robust if we take procurement revenues +/- one year around elections, rather than just for the year after the elections. We also verify that the results are robust to using alternative thresholds of procurement revenue as a share of total revenue of 1% and 5% instead of zero. S_f and R_f are the industry (sector) and region dummies, which control for variation across sectors and regions in public procurement contracts and in corruption. τ_e is the year fixed effect controlling for multiple elections in a particular region. X_f is a vector of additional control variables, namely, the logarithm of firm's revenue, net income as a share of revenue, and the ratio of debt to assets. All controls are measured in 2003.[10] The error term ε_{fe} is clustered at the level of firms.

Our main explanatory variable is ST_{fe}^{window}. It denotes the average weekly transfer by firm f to fly-by-night firms within the window of [-4; +4] weeks from the election date e in the region where firm f is located; or, alternatively, it denotes the average weekly transfer by firm f to fly-by-night firms outside this election window across all weeks in the year before the election window, i.e., $w < -4$. We include these variables in the set of covariates one by one as well as together. As described above, they can be endogenous, and therefore, we instrument them with dummies for tax agencies to which the firms are assigned. As the enforcement capacity of tax agencies can change from one election to another, we use separate tax agency dummies for each election. As a result, 171 tax agency dummies are used as excluded instruments.

Further, in order to assess how shadow transfers affect the size of procurement revenue, we estimate an additional specification:

$$\log(1 + ProcR_{fe}) = \alpha_1 \log(1 + ST_{fe}^{window}) + \alpha_2' X_f + \alpha_3 S_f + \alpha_4 R_f + \tau_e + \varepsilon_{fe}. \quad (4)$$

The dependent variable in this specification is the logarithm of procurement revenue received during one year following election e. The rest of the notation is as above. Again, our main variables of interest $\log ST_{fe}^{window}$ is endogenous and we instrument it using tax agency

[10] We verified that our results are robust to using contemporary rather than 2003 controls. The sample, however, is substantially reduced when contemporary controls are included because of data limitations. None of our main results depend on inclusion of a particular set of controls.

dummies. We verified that the results are similar when the procurement revenue share of total revenue (instead of the absolute level) is taken as the dependent variable.

The first-stage results yield that our instruments are weak. The F-stats from the first stage for the excludable instruments are 3.63 and 4.90 for predicting $\log ST_f^{window}$ inside and outside the election window of [-4;+4] weeks, respectively. Therefore, the conventional 2SLS or IV probit models for instrumental variable estimation yield biased estimators. Hansen, Hausman and Newey (2003) and Hasselt (2010) show that the best-performing estimation model in the case of many instruments is the Limited Information Maximum Likelihood (LIML). As we have a large number of instruments, we adopt LIML as our estimation technique. Furthermore, we report robust confidence intervals calculated using the Conditional Likelihood Ratio (CLR) approach developed by Moreira (2003) especially for the case of weak instruments taking into account the size of possible biases.[11]

4.1 Results: The Effect of Shadow Transfers on Procurement

Tables 3 and 4 present the results of the estimation of equations 4.2 and 4, respectively. Table 3 reports regressions for the relationship between shadow transfers in and outside the election window, and the probability of securing non-zero procurement revenue. The first two columns of the table present LIML IV regressions along with the robust CLR 95%-confidence intervals for the coefficients. We find that shadow transfers inside the election window have a positive and significant effect on the probability of securing a procurement contract within one year following this election. According to the point estimate, a 10 percent increase in shadow transfers close to the election is associated with an 8 percent increase in the probability of securing at least some revenue from procurement contracts. This estimate is statistically significant at the 1% level both if conventional standard or CRL errors are applied. Yet, since estimators in the presence of weak instruments are biased, the CRL confidence interval may provide better guidance to the size of the effect; they are reported in square brackets. The lower bound of the CLR 95%-confidence interval is positive and equals to a 6 percentage point increase in the probability of getting procurement contracts as a result of a 10 percent increase in shadow transfers, while the upper bound is 12 percentage points. The coefficient on the size of the shadow transfers outside the election window is also positive and statistically significant, but the point estimate is lower than one half of that inside the election window, and the CRL confidence interval for the

[11]The F-statistics in the first stage increase at least ten-fold if we suppress regional dummies, while the second stage-results are robust to such alteration. Nonetheless, we report the results with regional dummies included in the set of covariates, as variation across regions in the levels of corruption, tax enforcement capacity, and public procurement may be driven by many unobserved factors.

effect is between 2.7 and 4.4 percentage points. The third column of the table shows that shadow transfers in and outside the election window, once predicted by the variation in the tax enforcement capacity of tax agencies, are collinear and we cannot run a credible horse-race between the two in IV regressions. Shadow transfers in and outside election windows are significantly positively correlated across firms (with a pair-wise correlation coefficient of 0.75), once they are predicted by the same set of tax agency dummies, they become even more highly correlated with a pair-wise correlation coefficient of 0.98. Thus, with the help of our instruments one cannot disentangle the effect of shadow transfers inside and outside the election window. Columns 4 to 6 present the results of OLS estimation, for comparison. Both the shadow transfers within the election window and outside have a statistically significant association with public procurement contracts in OLS regressions with an approximately same-size effect, which is an order of magnitude smaller than the effect estimated with LIML IV regressions. This could be explained by the measurement error inherent in measuring shadow transfers.

Table 4 presents the results of the estimation of equation 4. The main coefficients of interest—i.e., the estimates of the effects of shadow transfers inside and outside election window on the size of procurement revenue received in the year following elections—again are highly significant even after taking into account the weakness of the instruments using CLR correction of confidence intervals. As above, we find that the effectiveness of shadow transfers within the election window is about twice as large as that of transfers outside the election window. In particular, point estimates suggest that a 10 percent increase in shadow transfers leads roughly to a 4 percent increase in procurement revenue when shadow transfers are madeinside the election window, and to a 2 percent increase in procurement revenue when shadow transfers are made outside the election window. The 95%-confidence intervals corrected for weak instrument bias are [1.5; 6.4] and [0.4; 3.9] percent, respectively (as shown in the first two columns of Table 4). The rest of the tables present results of LIML IV regression with both endogenous covariates included (which confirms collinearity) and of OLS regressions which yield statistically significant effects for shadow transfers inside and outside the election window. The coefficients on shadow transfers inside and outside the election window are similar in magnitude to each other and to the effect of shadow transfers outside the election window estimated with IV. A horse-race between shadow transfers inside and outside the election window estimated with OLS yields a 38% larger coefficient on transfers inside the election window.

4.2 Perceived Corruption and Shadow Election Financing

An important reality check on our estimates is whether the corruption that we uncover is correlated with available measures of perceived corruption. We can test whether shadow transfers are more closely associated with winning public procurement contracts in the regions considered to be most corrupt. We use variation in regional-level perceived corruption measured by the Transparency International-Russia and INDEM foundation for 40 regions in Russia in 2002.[12] This is a perception-based index compiled using enterprise-managers surveys. We describe this index in detail in the Appendix. For the purposes of simplicity of interpretation, we take the z-score of the index, so that the resulting measure has zero mean and unit variance (higher values indicate higher perceived regional corruption). We augment Equation by including the interaction between the regional-level corruption perception index and $\log ST_{fe}^{window}$. Note that as the perception of corruption index does not vary over time, region fixed-effects control for the direct effect of regional variation in perceived corruption, whereas our focus is on whether the effect of shadow transfers on wining public procurement contracts increases with the level of regional perceived corruption. In these regressions, the sample size decreases to $35,614$ observations because the perception of corruption index exists only for 40 regions. The augmented specification contains two endogenous regressors, namely, shadow transfers and their interaction with corruption level. As the LIML procedure is available for the case of a single endogenous regressor only, we use 2SLS instead of LIML. In particular, we predict the shadow transfers by a linear combination of tax agency dummies, as above. The interaction of shadow transfers and corruption is predicted by the interaction of the fitted value of the regression of shadow transfers on tax agency dummies with the corruption index.

Table 5 presents the results. In both IV and OLS regressions for the probability and the size of public procurement revenue, the coefficients on the interaction between corruption level and shadow transfers is positive and highly statistically significant in all specifications. The magnitude of the coefficients of interest is slightly larger for IV than for OLS regressions, and for shadow transfers inside the election window compared to outside the election window of [-4;4] weeks around elections. A one standard deviation difference in the level of perceived corruption leads to a 12 percentage point larger effect of shadow transfers on the size of public procurement revenue in a more corrupt region. Thus, corruption in public procurement, which we document from the objective banking transactions data, is positively correlated with a perception of corruption.

[12]The index is available at url: http://www.anti-corr.ru/rating_regions/index.htm.

5 Efficiency Losses from Corruption

An important question is whether corruption in public procurement leads to an inefficiency in allocation of procurement contracts. Does corruption help or hurt the chances of more efficient firms to gain public procurement? As the theoretical literature provides reasons in favor of and againstboth possibilities (see, for instance, the surveye by Aidt, 2003), it is an empirical question. In order to address this question, we measure the degree of corruption in public procurement at the level of each tax agency by observing the strength of the association between shadow campaign financing and distribution of government procurement contracts. In particular, we estimate Equation 3 with shadow transfers inside the election window as the main variable of interest, $ST_{fe}^{election}$, allowing the coefficient α_1 on $ST_{fe}^{election}$ to vary for each tax agency. We confine the sample to tax agencies with at least 50 firms assigned to it and take α_1 for each tax agency as a measure of corruption in public procurement. (This measure has a mean of 0.051 and SD of 0.042.) As a next step, we regress a firm-level dummy for having won large government procurement contracts (relative to the size of the firm, i.e., 1% or 5% of total revenue) on firms' labor productivity, our measure of corruption, and their interaction, controlling for all our standard firm-level controls, as well as industry, region, and year dummies. The results are presented in Table 6. We present results for the two samples: the entire sample and the sample of firms with the same legal and actual address. The first four regressions have a dummy for 1% of revenue coming from public procurement contracts as a dependent variable and the last four regressions have a dummy for 5% of revenue coming from public procurement contracts as a dependent variable. First, we find that government procurement contracts are, on average, directed to less efficient firms (as coefficient on labor productivity is negative and statistically significant). Second, firms located in more corrupt regions are more likely to obtain public procurement contracts. The third and the most important finding is that higher corruption is associated with less efficient firms obtaining public procurement contracts. This can be seen from the negative sign of the coefficients on the interaction between labor productivity and our corruption measure. These coefficients are always negative and for government procurement contracts above 1% of revenue, statistically significant. Thus, we find that, on average, less productive firms win government procurement contracts, and this relationship is more severe in more corrupt localities. Therefore, corruption leads to efficiency losses in the allocation of government procurement contracts.

6 Conclusions

Corruption in Russia is pervasive. We use objective data for a near-population of Russian firms to document that, as a rule, the allocation of public procurement contracts depends on bribes paid by firms to politicians. Bribes follow a political cycle: Politicians prefer to collect bribes around elections as black cash is used to finance election campaigns. An average firm that receives public procurement contracts pays about 30,000 U.S. dollars in bribes during a regional election campaign and gets procurement contracts that bring the firm 100,000 U.S. dollars in additional revenue per year. The total amount of cash tunneled illegally to finance an average regional election in exchange for future allocation of public procurement in Russia is about 2.5 million U.S. dollars. We find that shadow election financing in exchange for public procurement contracts is not just a pure transfer, since it has negative implications for the efficiency of the allocation of public procurement. Less productive firms are more likely to win public procurement contracts in more corrupt localities and, therefore, public procurement is less efficient with corruption.

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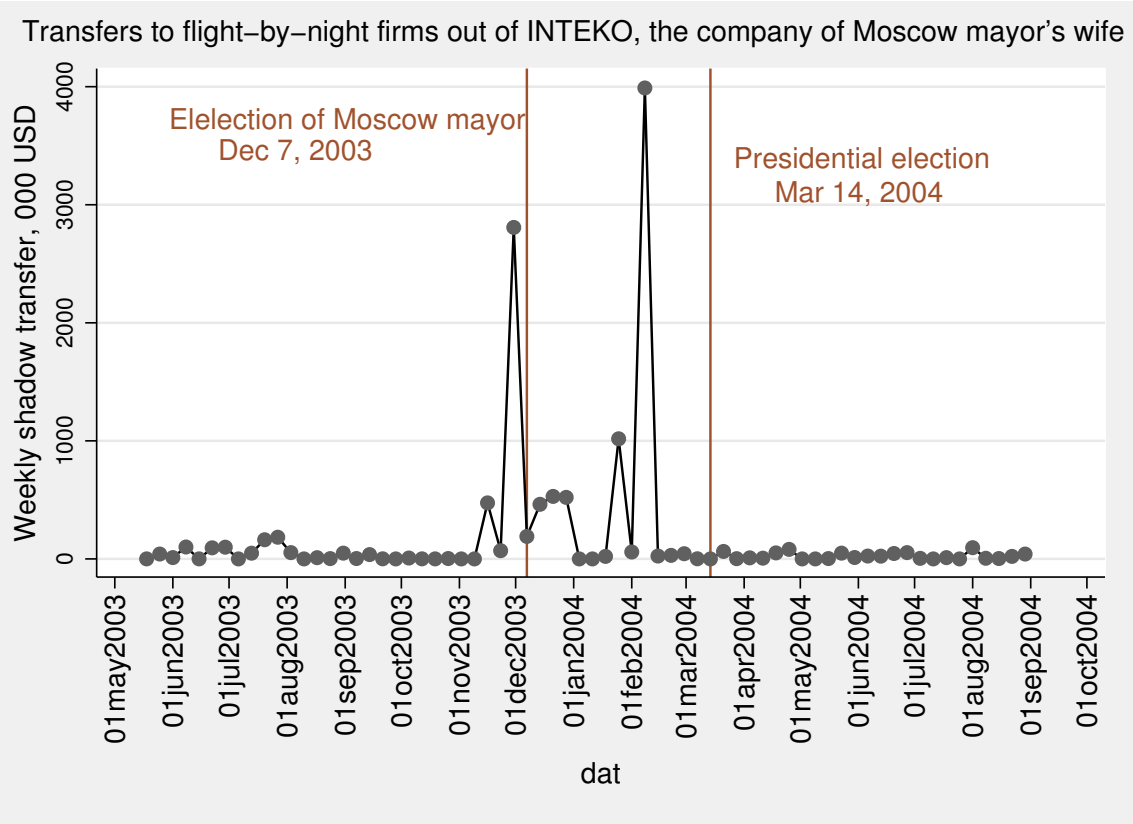


Figure 1: Transfers to fly-by-night firms by Inteko, the company of the wife of Moscow mayor

Note: The figure portrays the amount of cash tunneled from the company Inteko.

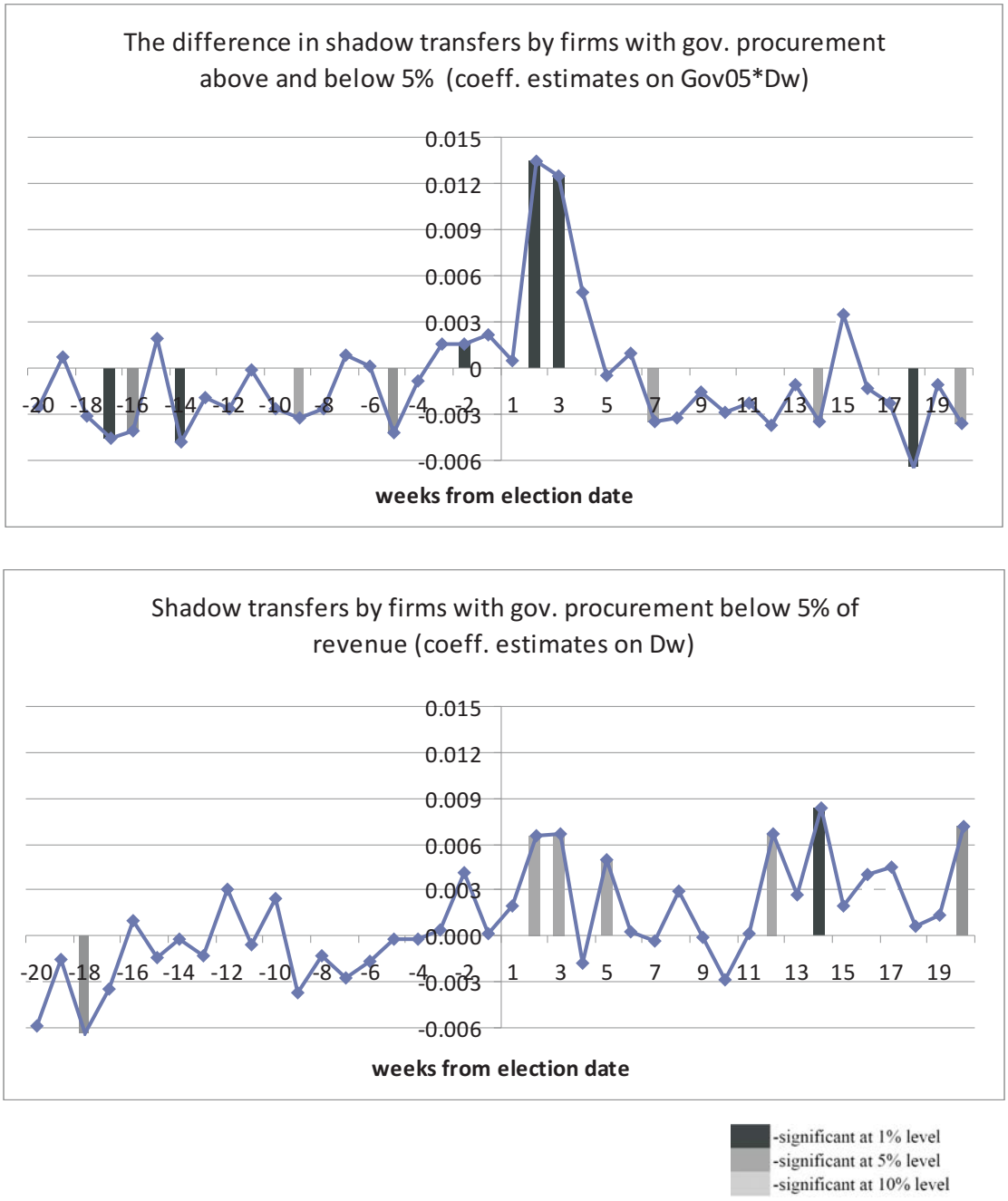


Figure 2: Political cycle in shadow transfers by firms with and without procurement contracts

Note: The figure portrays coefficient estimates of β_w^1 and β_w^2 from the estimation of Equation 1 along with their significance levels. The full regression output is presented in Table A.3 in the Appendix.

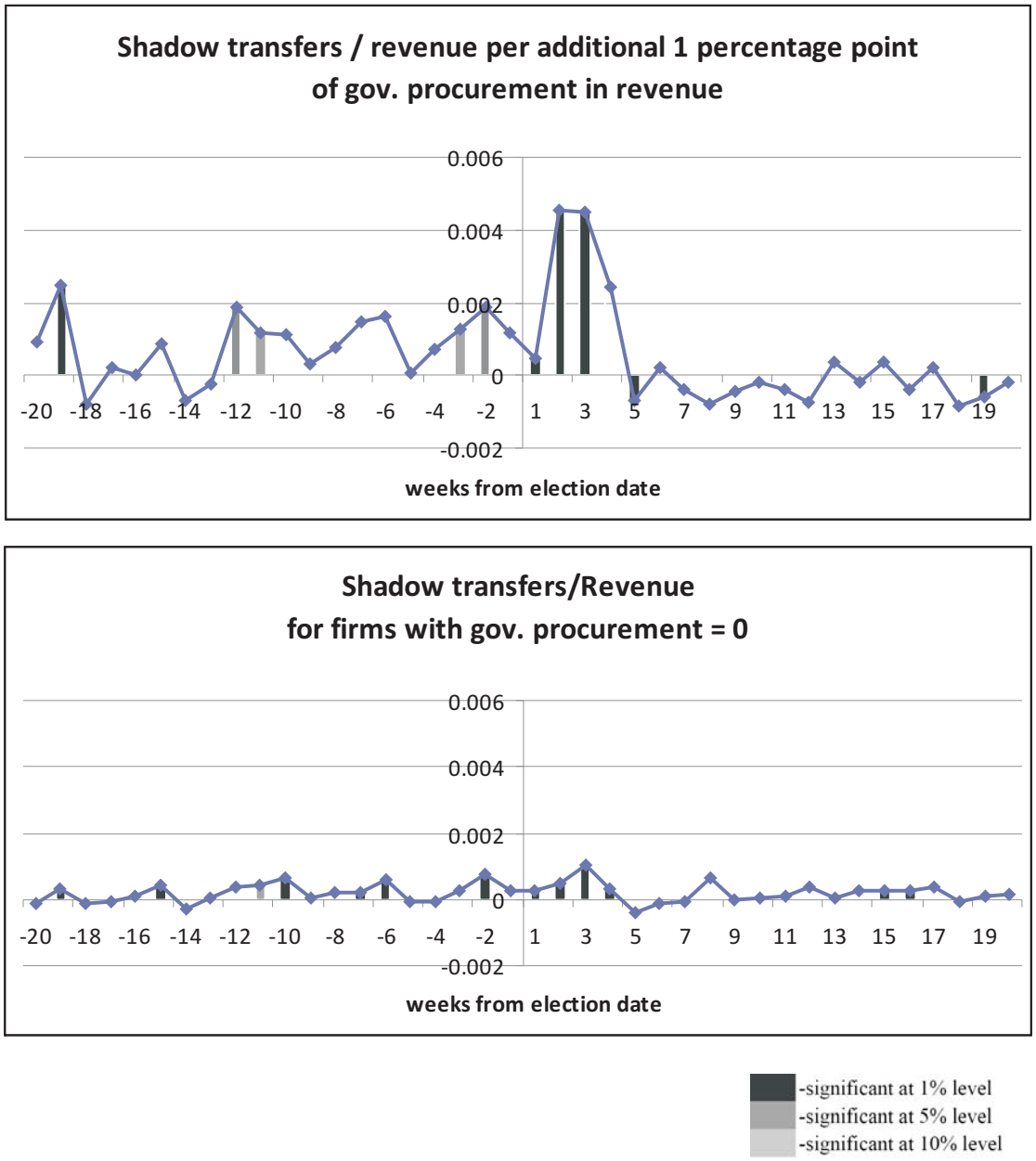
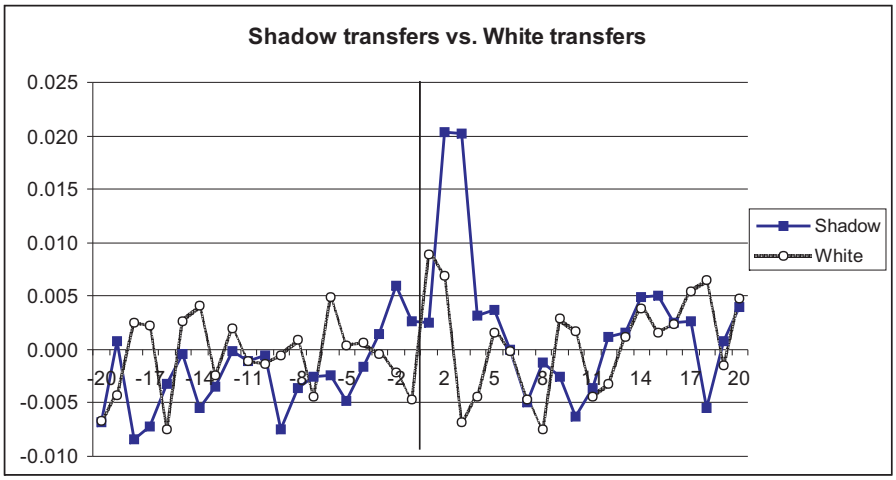


Figure 3: Political cycle in shadow transfers and the size of procurement contracts

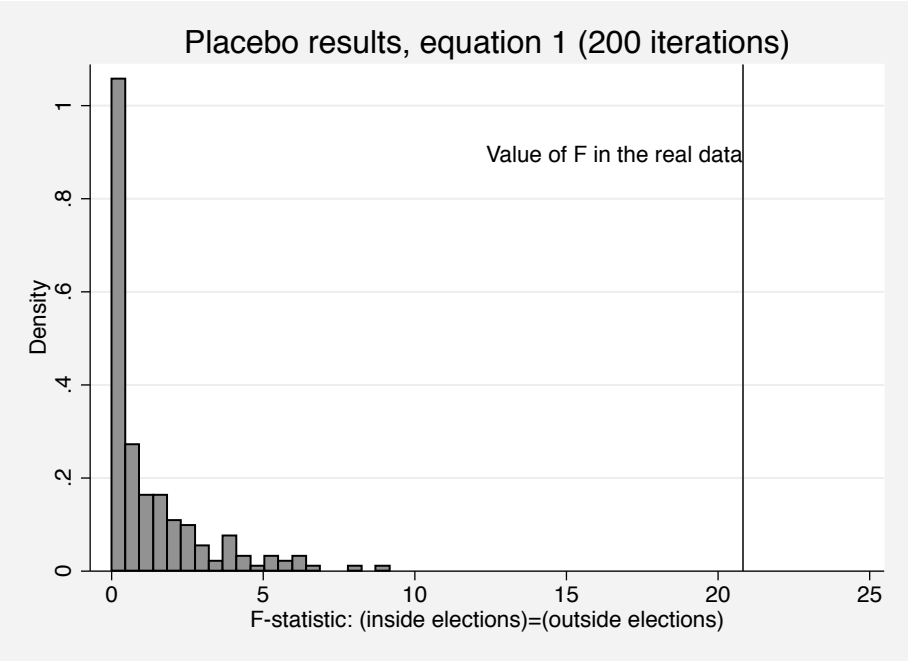
Note: The figure portrays coefficient estimates of γ_w^1 and γ_w^2 from the estimation of Equation 2 along with their significance levels. The full regression output is presented in Table A.4 in the Appendix.



	Cycle in White and Shadow transfers	
F-test for:	White	Shadow
$\beta^1_{in[-4;4]} = \beta^1_{out[-4;4]}$	F = 0.12	F = 28.59
	p = 0.73	p = 0.00
$\beta^2_{in[-4;4]} = \beta^2_{out[-4;4]}$	F = 1.77	F = 2.05
	p = 0.18	p = 0.15

Figure 4: Placebo: transfers to fly-by-night firms vs. transfers to legitimate firms

Note: The figure portrays the dynamics of total transfers, i.e., $\beta^1_w + \beta^2_w$ from the estimation of Equation 1 taking overtime distribution of transfers to fly-by-night firms (shadow transfers) and transfers to legitimate firms (white transfers) as dependent variables. The table presents F-tests for the cycle in respective transfers among firms with public procurement revenue is above (upper row) and below (lower row) the 5% of total revenue threshold.



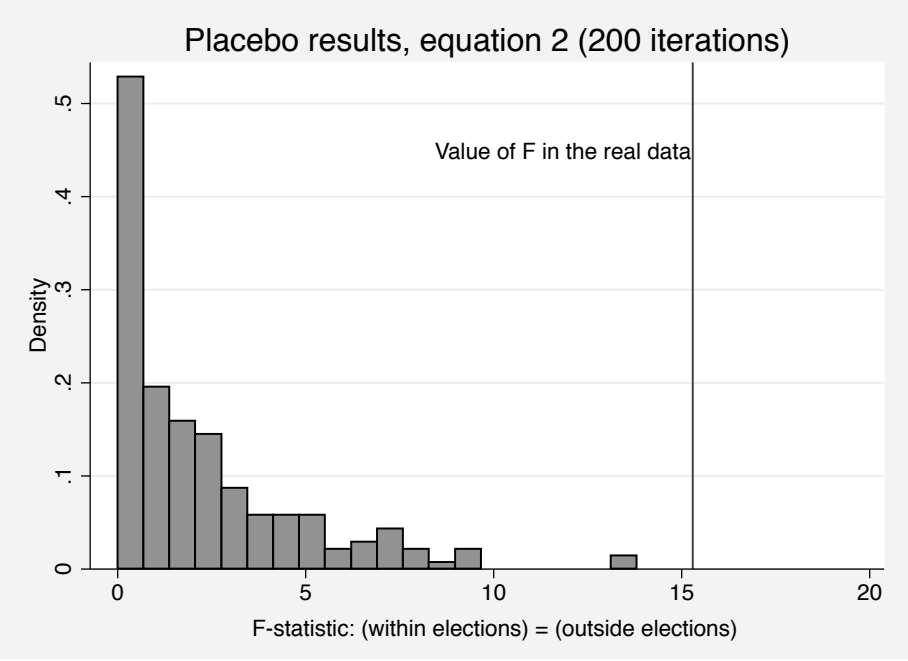


Figure 5: Placebo election dates

Table 1: Political cycle in shadow transfers and the strength of political competition

	Shadow transfers in sub-samples of elections					
	Winner:			Incumbent:		
	got <50%	got >50%	lost	won	got <70%	got >70%
	Specification 1					
Gov05 x Election window	0.003	0.006	0.001	0.006	0.006	0.006
	(0.003)	(0.001)***	(0.003)	(0.001)***	(0.002)**	(0.001)***
Election window	-0.001	0.003	0.002	0.002	0.000	0.004
	(0.003)	(0.002)*	(0.004)	(0.001)	(0.002)	(0.002)**
Number of obs	450375	1294117	191951	1384562	743683	832830
Number of firms	10561	25042	4719	27805	17240	20159
	Specification 2					
Procurement rev. share x Election window	0.0002	0.0016	0.0003	0.0016	0.0007	0.0013
	(0.0001)**	(0.0004)***	(0.0001)**	(0.0004)***	(0.0003)**	(0.0005)***
Election window	0.0001	0.0004	0.0001	0.0003	0.0002	0.0004
	(0.0001)	(0.0001)***	(0.0001)	(0.0001)***	(0.0001)**	(0.0001)***
Number of obs	450375	1294117	191951	1384562	743683	832830
Number of firms	10561	25042	4719	27805	17240	20159

Note: Standard errors corrected for clusters at the level of firms are in parentheses. Election window is a dummy indicating 4 weeks away from elections on both sides. "Gov05" is a dummy indicating that procurement revenue is above 5% of firm's revenue. "Procurement Rev share" is the share of firms revenue that comes from procurement contracts.

Table 2: Magnitude

Difference-in-differences estimate of the size of shadow election financing per firm with non-zero public procurement revenue			
	Window:		
	+/−4 weeks around election	average 8 weeks outside election window	Dif: (inside) - (outside)
Shadow transfer per firm | Gov>0	$ 107 760	$ 72 960	$ 34 800
Shadow transfer per firm | Gov=0	$ 24 000	$ 20 000	$ 4 000
Dif: (S.Transfer | Gov>0) - (S.Transfer | Gov=0)	$ 83 760	$ 52 960	$ 30 800
Shadow campaign financing per firm			$ 30 800
Average # of firms with Gov>0 per region			81
Average size of shadow campaign financing			$ 2 488 730

Table 3: Probability of procurement contracts and shadow transfers

	Dummy: revenue from procurement contracts > 0					
	(1)	(2)	(3)	(4)	(5)	(6)
	IV LIML	IV LIML	IV LIML	OLS	OLS	OLS
Log(1+ Shadow transfers / week), election	0.802		15.455	0.062		0.039
	(0.121)***		(564.481)	(0.002)***		(0.003)***
	[0.593; 1.188]					
Log(1+ Shadow transfers / week), outside election		0.355	-13.938		0.067	0.038
		(0.045)***	(513.556)		(0.003)***	(0.003)***
		[0.266; 0.47]				
Log(Revenue)	-0.170	-0.059	-0.218	0.021	0.019	0.016
	(0.031)***	(0.012)***	(8.105)	(0.002)***	(0.002)***	(0.002)***
Net Income/Revenue	-0.262	-0.052	-2.078	0.020	0.026	0.019
	(0.058)***	(0.023)**	(76.267)	(0.017)	(0.017)	(0.017)
Debt/Assets	-0.179	-0.107	0.492	-0.026	-0.031	-0.031
	(0.035)***	(0.018)***	(19.664)	(0.011)**	(0.011)***	(0.011)***
Industry dummy	Y	Y	Y	Y	Y	Y
Region dummy	Y	Y	Y	Y	Y	Y
Election year dummy	Y	Y	Y	Y	Y	Y
R-sq				0.191	0.190	0.196
Number of obs	41 983	41 983	41 983	41 983	41 983	41 983
Number of firms	25 108	25 108	25 108	25 108	25 108	25 108
F-stat, excluded instruments	3.63	4.90	3.63; 4.9			

Note: Standard errors corrected for clusters at the level of firms are in parentheses. The instruments are weak, which may result in a bias of second-stage estimates. To account for this, we report reliable 95% confidence intervals in square brackets calculated using the Conditional Likelihood Ratio (CLR) approach developed by Moreira (2003) and Andrews, Moreira and Stock (2007) especially for the case of weak instruments. All IV regressions are estimated with Limited Information Maximum Likelihood (LIML), which is known to perform better in the presence of a large number of instruments.

Table 4: Volume of procurement contracts and shadow transfers

	Log (1+ Procurement revenue)					
	(1)	(2)	(3)	(4)	(5)	(6)
	IV LIML	IV LIML	IV LIML	OLS	OLS	OLS
Log(1+ Shadow transfers / week), election	0.386		20.854	0.244		0.171
	(0.104)***		(209.354)	(0.011)***		(0.014)***
	[0.146; 0.636]					
Log(1+ Shadow transfers / week), outside election		0.216	-18.733		0.248	0.124
		(0.079)***	(190.466)		(0.012)***	(0.014)***
		[0.04; 0.392]				
Log(Revenue)	0.090	0.132	-0.174	0.127	0.123	0.112
	(0.027)***	(0.022)***	(3.015)	(0.008)***	(0.008)***	(0.008)***
Net Income/Revenue	-0.092	-0.003	-2.828	-0.038	-0.012	-0.043
	(0.071)	(0.063)	(28.278)	(0.068)	(0.068)	(0.068)
Debt/Assets	-0.179	-0.157	0.563	-0.150	-0.165	-0.168
	(0.046)***	(0.046)***	(7.327)	(0.045)***	(0.045)***	(0.045)***
Industry dummy	Y	Y	Y	Y	Y	Y
Region dummy	Y	Y	Y	Y	Y	Y
Election year dummy	Y	Y	Y	Y	Y	Y
R-sq				0.164	0.160	0.167
Number of obs	41 983	41 983	41 983	41 983	41 983	41 983
Number of firms	25 108	25 108	25 108	25 108	25 108	25 108
F-stat, excluded instruments	3.63	4.90	3.63; 4.90			

Note: Standard errors corrected for clusters at the level of firms are in parentheses. The instruments are weak, which may results in a bias of second-stage estimates. To account for this, we report reliable 95% confidence intervals in square brackets calculated using the Conditional Likelihood Ratio (CLR) approach developed by Moreira (2003) and Andrews, Moreira and Stock (2007) especially for the case of weak instruments. All IV regressions are estimated with Limited Information Maximum Likelihood (LIML), which is known to perform better in the presence of a large number of weak instruments.

Table 5: Effectiveness of shadow transfers in securing procurement contracts and regional corruption

	Dummy: proc. revenue >0				Log (1+ Procurement revenue)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IV	OLS	IV	OLS	IV	OLS	IV	OLS
Log(1+ Shadow transfers / week), elect	0.105	0.055			0.134	0.217		
	(0.021)***	(0.002)***			(0.083)	(0.013)***		
[Perceived Corruption x	0.013	0.023			0.126	0.090		
Log(1+ Shadow transfers / week), elect]	(0.006)**	(0.002)***			(0.029)***	(0.014)***		
Log(1+ Shadow transfers / week), out elect			0.098	0.060			0.095	0.223
			(0.019)***	(0.003)***			(0.076)	(0.013)***
[Perceived Corruption x			0.009	0.021			0.107	0.079
Log(1+ Shadow transfers / week), out elect]			(0.005)*	(0.003)***			(0.028)***	(0.014)***
Log(Revenue)	0.009	0.021	0.010	0.019	0.157	0.134	0.166	0.132
	(0.006)	(0.002)***	(0.006)*	(0.002)***	(0.025)***	(0.009)***	(0.024)***	(0.009)***
Net Income/Revenue	0.006	0.023	0.020	0.031	0.036	-0.017	0.060	0.015
	(0.023)	(0.02)	(0.022)	(0.02)	(0.092)	(0.081)	(0.087)	(0.081)
Debt/Assets	-0.037	-0.025	-0.040	-0.029	-0.129	-0.139	-0.126	-0.154
	(0.014)***	(0.013)**	(0.014)***	(0.012)**	(0.055)**	(0.051)***	(0.056)**	(0.051)***
Industry dummy	Y	Y	Y	Y	Y	Y	Y	Y
Region dummy	Y	Y	Y	Y	Y	Y	Y	Y
Election year dummy	Y	Y	Y	Y	Y	Y	Y	Y
R-sq				0.07	0.06	0.07	0.06	0.07
Number of obs	35 614	35 614	35 614	35 614	35 614	35 614	35 614	35 614
Number of firms	20 342	20 342	20 342	20 342	20 342	20 342	20 342	20 342
F-stat, excluded instruments	3.36		4.57		3.36		4.57	

Note: Standard errors corrected for clusters at the level of firms are in parentheses. "Corr" is the Transparency International and INDEM regional corruption index.

Table 6: Efficiency loss from corruption

Dependent var:	dummy: public procurement >1 % of revenue				dummy: public procurement >5 % of revenue			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
sample:	all firms	all firms	same address	same address	all firms	all firms	same address	same address
Log labor productivity	-0.012	-0.007	-0.009	-0.005	-0.005	-0.004	-0.004	-0.003
	(0.002)***	(0.002)***	(0.003)***	(0.003)*	(0.001)***	(0.001)***	(0.002)**	(0.002)
Tax-agency-level corruption	0.225	0.529	0.281	0.532	0.054	0.118	0.073	0.147
	(0.038)***	(0.089)***	(0.062)***	(0.131)***	(0.026)**	(0.061)*	(0.042)*	(0.088)*
[Log labor productivity x		-0.104		-0.087		-0.022		-0.026
Tax-agency-level corruption]		(0.026)***		(0.038)**		(0.017)		(0.024)
Log(Revenue)	0.008	0.008	0.008	0.008	0.002	0.002	0.002	0.002
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.001)**	(0.001)**	(0.001)	(0.001)
Net Income/Revenue	-0.039	-0.039	-0.030	-0.030	-0.039	-0.039	-0.048	-0.048
	(0.015)***	(0.015)***	(0.021)	(0.021)	(0.011)***	(0.011)***	(0.017)***	(0.017)***
Debt/Assets	-0.029	-0.030	-0.030	-0.030	-0.009	-0.009	-0.009	-0.009
	(0.007)***	(0.007)***	(0.012)**	(0.012)**	(0.005)*	(0.005)*	(0.009)	(0.009)
Industry dummy	Y	Y	Y	Y	Y	Y	Y	Y
Region dummy	Y	Y	Y	Y	Y	Y	Y	Y
Election year dummy	Y	Y	Y	Y	Y	Y	Y	Y
R-sq	0.06	0.06	0.07	0.07	0.04	0.04	0.04	0.04
Number of obs	36 801	36 801	17 153	17 153	36 801	36 801	17 153	17 153
Number of firms	20 792	20 792	9 732	9 732	20 792	20 792	9 732	9 732

Note: Standard errors corrected for clusters at the level of firms are in parentheses. "Tax-agency-level corruption" is our measure of corruption constructed from the association between election financing and the distribution of public procurement.

A Online Appendix

Table A.1: Summary statistics

Sample: firms x elections					
	Mean	Median	St. dev.	N of obs	N of firms
	(1)	(2)	(3)	(4)	(5)
Revenue 2003, $000's	$ 13 008	$ 2 691	$ 99 718	87528	52073
Assets 2003, $000's	$ 10 542	$ 1 138	$ 147 881	87528	52073
Net Income 2003, $000's	$ 591	$ 18	$ 18 666	87528	52073
Net Income / Revenue 2003, %	1.75	0.61	8.97	87528	52073
Debt / Assets 2003, %	4.16	0.00	14.72	87528	52073
Annualized transfers to fly-by-night firms 1999-2004, $000's	$ 221	$ 25	$ 5 024	87528	52073
Annualized transfers to fly-by-night firms 1999-2004 / Revenue 2003, %	4.79	1.09	9.11	87528	52073
Annualized revenue from procurement 1 year after election 1999-2004, $000's	$ 42	$ -	$ 1 314	87528	52073
Annualized revenue from procurement 1 year after election 1999-2004 / Revenue 2003, %	0.22	0.00	1.31	87528	52073
Log(1+procurement revenue)	0.358	0.000	1.224	87528	52073
Gov00 dummy, %	12.49	0.00	33.06	87528	52073
Gov01 dummy, %	3.70	0.00	18.88	87528	52073
Gov05 dummy, %	1.65	0.00	12.74	87528	52073
Log(1+Shadow transfers / week, election window), 1999-2004	0.450	0.000	0.972	87528	52073
Log(1+Shadow transfers / week, outside election window), 1999-2004	0.489	0.000	0.915	87528	52073
Perceived Corruption (TI Index)	0.000	0.016	1.000	75539	43113
Tax-agency-level corruption (Authors' measure)	0.051	0.055	0.042	60604	34245
Labor productivity (revenue per employee)	31.793	14.829	56.470	57658	34590
Log labor productivity	2.851	2.697	0.980	57658	34590
Sample: firms x weeks					
	Mean	Median	St. dev.	N of obs	N of firms
	(1)	(2)	(3)	(4)	(5)
Shadow transfers per week /Shadow transfers per year	0.019	0.000	0.090	2380669	32735
Shadow transfers per week / Revenue	0.001	0.000	0.004	2380669	32735
Procurement revenue / Revenue, %	0.677	0.000	3.804	2380669	32735
Gov01 dummy, %	7.382	0.000	26.148	2380669	32735
Gov05 dummy, %	3.070	0.000	17.251	2380669	32735

Note: "Gov00," "Gov01," and "Gov05" are dummies that indicate firms with (Revenue from procurement / Revenue) greater than 0, 1, and 5%, respectively.

Table A.2: Summary statistics by region

Region	Election Date 1 (1)	Election Date 2 (2)	N obs.	N firms (3)	Revenue, $000's (4)	Shadow tr. / Rev., % (5)	Procur. / Rev., % (6)	Gov01, % (7)	Gov05, % (8)
Karachaevo-Cherkess republic	1999-04-25	2003-08-17	48	48	3 911	0.75	0.00	0.00	0.00
Belgorod oblast	1999-05-30	2003-05-25	940	940	15 580	2.29	0.07	1.28	0.53
Sverdlovsk oblast	1999-08-29	2003-09-07	140	140	8 189	1.78	0.08	0.71	0.71
Novgorod oblast	1999-09-05	2003-09-07	294	294	20 018	6.03	0.17	2.38	1.36
Omsk oblast	1999-09-05	2003-09-07	13	13	42 863	2.52	0.00	0.00	0.00
Leningrad oblast	1999-09-19	2003-09-21	60	60	4 590	2.88	0.00	0.00	0.00
Tomsk oblast	1999-09-19	2003-09-21	76	76	116 610	8.54	0.21	2.63	1.32
Primorskii krai	1999-12-19	2001-05-27	110	55	6 018	2.95	0.03	0.91	0.00
Vologda oblast	1999-12-19	2003-12-07	260	260	5 722	0.99	0.03	0.77	0.38
Moscow oblast	1999-12-19	2003-12-07	254	254	15 079	2.73	0.00	0.00	0.00
Novosibirsk oblast	1999-12-19	2003-12-07	308	154	4 105	1.78	0.06	0.97	0.32
Orenburg oblast	1999-12-19	2003-12-07	174	174	7 993	1.47	0.10	2.30	0.57
Tambov oblast	1999-12-19	2003-12-07	256	256	17 150	2.92	0.10	1.17	0.78
Tver oblast	1999-12-19	2003-12-07	75	75	6 153	5.41	0.00	0.00	0.00
Yaroslavl oblast	1999-12-19	2003-12-07	852	852	16 937	2.84	0.12	1.53	0.82
Moscow city	1999-12-19	2003-12-07	15	15	3 473	1.19	0.00	0.00	0.00
Altai krai	2000-03-26	2004-03-14	840	420	9 056	1.54	0.14	2.02	1.07
Kirov oblast	2000-03-26	2003-12-07	178	89	8 390	1.21	0.13	1.12	1.12
Murmansk oblast	2000-03-26	2004-03-14	267	267	7 392	1.86	0.10	1.12	1.12
Saratov oblast	2000-03-26		1 164	582	5 208	1.55	0.11	1.37	0.95
Evrei autonomous oblast	2000-03-26		3 252	1 626	6 972	3.07	0.09	1.29	0.74
Khanty-Mansi autonomous okru	2000-03-26		650	650	9 276	2.55	0.07	0.92	0.46
Yamalo-Nenets autonomous okr	2000-03-26		1 282	641	7 512	1.76	0.06	0.78	0.39
St. Petersburg city	2000-05-14	2003-09-21	582	582	6 339	2.64	0.10	1.55	0.69
Samara oblast	2000-07-02		1 144	572	7 760	2.17	0.08	1.14	0.61
Udmurtia Republic	2000-10-15	2004-03-14	179	179	7 245	3.02	0.00	0.00	0.00
Kursk oblast	2000-10-22		572	286	7 298	2.18	0.06	0.52	0.52
Sakhalin oblast	2000-10-22	2003-12-07	316	158	8 357	2.04	0.07	1.27	0.63
Chita oblast	2000-10-29	2004-03-14	890	445	9 691	2.94	0.06	1.01	0.45
Aginsk Buryat autonomous	2000-10-29		558	279	6 531	3.29	0.19	2.33	1.61
Kaliningrad oblast	2000-11-05		401	401	7 481	2.72	0.34	4.24	2.99
Magadan oblast	2000-11-05	2003-02-02	1 066	533	10 130	2.64	0.03	0.56	0.19
Kaluga oblast	2000-11-12	2004-03-14	802	401	14 435	1.93	0.03	0.62	0.25
Pskov oblast	2000-11-12	2004-11-14	1 436	718	5 387	2.23	0.08	0.97	0.56
Ust-Ordyn Buryat autonom	2000-11-19	2004-11-14	215	215	5 230	2.30	0.49	5.58	4.19
Kurgan oblast	2000-11-26	2004-11-28	732	732	8 825	2.24	0.06	0.96	0.41
Mari-El republic	2000-12-03	2004-12-19	280	280	8 818	2.31	0.07	1.79	0.36
Krasnodar krai	2000-12-03	2004-03-14	656	328	7 653	4.10	0.36	4.73	2.74
Stavropol krai	2000-12-03		262	131	7 136	1.55	0.12	1.53	1.15
Arkhangelsk oblast	2000-12-03	2004-03-14	685	685	14 931	1.75	0.02	0.29	0.15
Astrakhan oblast	2000-12-03	2004-12-05	666	333	5 900	1.64	0.09	1.65	0.75
Ivanovo oblast	2000-12-03		178	178	5 298	2.52	0.20	2.25	2.25
Kamchatka oblast	2000-12-03	2004-12-05	226	113	7 850	1.25	0.17	1.77	1.33
Perm oblast	2000-12-03		243	243	8 084	1.86	0.15	2.06	1.23
Ryazan oblast	2000-12-03	2004-03-14	1 156	578	10 728	1.39	0.02	0.61	0.00
Komi-Permyak autonomous	2000-12-03		314	314	16 967	2.46	0.08	1.59	0.32
Koryak autonomous okrug	2000-12-03	2004-03-14	224	112	5 592	1.94	0.17	2.23	1.34
Khabarovsk krai	2000-12-10	2004-12-19	6 454	3 227	10 303	6.99	0.45	8.26	3.08
Bryansk oblast	2000-12-10	2004-12-05	564	282	10 863	1.62	0.07	1.06	0.53
Vladimir oblast	2000-12-10		1 174	1 174	11 437	2.92	0.16	2.39	1.19
Kostroma oblast	2000-12-10		346	173	7 501	1.59	0.07	0.58	0.58
Khakasia republic	2000-12-24	2004-12-26	1 850	925	9 767	2.58	0.11	1.46	0.81
Volgograd oblast	2000-12-24	2004-12-05	826	413	16 808	2.35	0.05	0.61	0.36
Voronezh oblast	2000-12-24	2004-03-14	638	319	12 869	1.97	0.03	0.31	0.31
Ulyanovsk oblast	2000-12-24	2004-12-05	243	243	12 035	2.42	0.17	4.53	1.23
Chelyabinsk oblast	2000-12-24		280	280	4 780	2.11	0.16	3.21	1.43
Chukotka autonomous okrug	2000-12-24		688	688	14 795	2.13	0.10	1.60	0.58
Tyumen oblast	2001-01-14		334	167	5 898	2.42	0.19	2.40	1.50
Nenets autonomous okrug	2001-01-14		1 319	1 319	7 465	3.07	0.13	1.74	1.29
Taimyr autonomous okrug	2001-01-28	2003-01-26	580	290	8 664	2.47	0.19	2.24	1.72
Tatarstan republic	2001-03-25		1 154	1 154	15 027	1.91	0.06	0.95	0.35
Amur oblast	2001-03-25		511	511	9 357	3.26	0.10	1.37	0.78
Tula oblast	2001-04-08		502	251	5 446	1.72	0.01	0.40	0.00
Evenki autonomous okrug	2001-04-08		2 774	1 387	12 237	1.86	0.06	0.97	0.36
Kemerovo oblast	2001-04-22		278	278	9 309	4.13	0.17	2.88	1.08
Nizhny Novgorod oblast	2001-07-15		352	176	5 266	1.38	0.33	4.55	2.84
Irkutsk oblast	2001-07-29		680	340	6 728	3.82	0.19	2.50	1.62
Rostov oblast	2001-09-23		646	323	9 660	2.27	0.00	0.00	0.00
Oryol oblast	2001-10-28	2002-04-12	447	447	8 673	3.36	0.22	3.58	1.57
Altai republic	2001-12-16		442	442	15 846	2.64	0.06	1.36	0.45
Komi republic	2001-12-16		632	316	20 118	2.64	0.07	0.95	0.63
Chuvash republic	2001-12-16		804	804	14 851	1.94	0.11	1.62	1.00
Sakha (Yakutia) republic	2001-12-23		212	106	6 906	2.37	0.08	1.42	0.94
Adygeya republic	2002-01-13		1 132	566	8 238	2.21	0.14	2.56	0.97
Kabardino-Balkar republic	2002-01-13		28 854	14 427	17 423	8.56	0.39	6.57	2.80
North Osetiya republic	2002-01-20		6 364	3 182	9 981	2.66	0.17	2.42	1.40
Tuva republic	2002-03-17		25	25	2 907	0.72	0.00	0.00	0.00
Ingush republic	2002-04-07		18	18	22 549	4.74	0.14	11.11	0.00
Lipetsk oblast	2002-04-12		7	7	3 670	0.41	0.00	0.00	0.00
Penza oblast	2002-04-12		20	10	17 226	1.49	0.08	0.00	0.00
Karelia republic	2002-04-28		24	24	11 245	3.04	0.00	0.00	0.00
Smolensk oblast	2002-05-19		34	17	268 774	5.41	0.14	2.94	0.00
Buryat republic	2002-06-23		8	4	1 674	0.84	0.00	0.00	0.00
Krasnoyarsk krai	2002-09-08		673	673	44 105	3.17	0.03	0.45	0.15
Kalmyk republic	2002-10-20		36	36	41 614	3.54	0.48	5.56	5.56
Mordovia republic	2003-02-16		19	19	242 803	1.47	0.00	0.00	0.00
Bashkortostan republic	2003-12-07		293	293	34 052	3.38	0.05	0.68	0.34

Table A.3: Regression results, Specification 1

Election week w	Coef. β_w^1	Std. Err.	t	Coef. β_w^2	Std. Err.	t	Coef. β_w^3	Std. Err.	t
-20	-0.0026	0.0018	-1.44	-0.0059	0.0030	-1.95	0.0005	0.0003	1.42
-19	0.0007	0.0026	0.26	-0.0015	0.0029	-0.53	0.0001	0.0003	0.20
-18	-0.0031	0.0020	-1.53	-0.0063	0.0030	-2.09	0.0006	0.0003	1.69
-17	-0.0045	0.0017	-2.71	-0.0034	0.0031	-1.11	0.0002	0.0004	0.62
-16	-0.0040	0.0019	-2.15	0.0010	0.0029	0.34	-0.0002	0.0003	-0.54
-15	0.0019	0.0028	0.67	-0.0014	0.0031	-0.44	0.0000	0.0004	-0.03
-14	-0.0049	0.0012	-3.89	-0.0002	0.0030	-0.07	-0.0001	0.0003	-0.18
-13	-0.0019	0.0018	-1.06	-0.0013	0.0028	-0.47	0.0000	0.0003	0.12
-12	-0.0027	0.0018	-1.45	0.0031	0.0030	1.02	-0.0005	0.0003	-1.35
-11	-0.0001	0.0023	-0.04	-0.0006	0.0031	-0.19	-0.0001	0.0004	-0.19
-10	-0.0027	0.0018	-1.45	0.0024	0.0032	0.75	-0.0004	0.0004	-1.09
-9	-0.0033	0.0018	-1.81	-0.0037	0.0029	-1.25	0.0004	0.0003	1.31
-8	-0.0026	0.0019	-1.39	-0.0013	0.0031	-0.43	0.0000	0.0004	-0.09
-7	0.0008	0.0021	0.37	-0.0028	0.0027	-1.00	0.0002	0.0003	0.63
-6	0.0001	0.0022	0.05	-0.0017	0.0031	-0.54	0.0002	0.0004	0.46
-5	-0.0042	0.0016	-2.56	-0.0002	0.0029	-0.07	0.0000	0.0003	-0.11
-4	-0.0008	0.0020	-0.40	-0.0002	0.0028	-0.08	-0.0001	0.0003	-0.31
-3	0.0016	0.0026	0.62	0.0004	0.0031	0.13	-0.0002	0.0004	-0.69
-2	0.0016	0.0030	0.53	0.0042	0.0032	1.31	-0.0006	0.0004	-1.53
-1	0.0021	0.0027	0.80	0.0002	0.0029	0.06	-0.0001	0.0003	-0.36
1	0.0005	0.0026	0.17	0.0020	0.0037	0.55	-0.0002	0.0004	-0.47
2	0.0135	0.0040	3.34	0.0066	0.0035	1.88	-0.0008	0.0004	-2.00
3	0.0124	0.0036	3.42	0.0067	0.0034	1.95	-0.0008	0.0004	-2.11
4	0.0049	0.0031	1.60	-0.0018	0.0035	-0.51	0.0002	0.0004	0.53
5	-0.0005	0.0023	-0.20	0.0050	0.0028	1.79	-0.0006	0.0003	-2.05
6	0.0009	0.0022	0.44	0.0003	0.0033	0.09	0.0000	0.0004	0.02
7	-0.0035	0.0018	-1.96	-0.0004	0.0030	-0.12	-0.0001	0.0003	-0.23
8	-0.0033	0.0024	-1.39	0.0029	0.0034	0.86	-0.0003	0.0004	-0.79
9	-0.0016	0.0019	-0.82	-0.0001	0.0030	-0.02	0.0000	0.0003	-0.06
10	-0.0029	0.0020	-1.41	-0.0029	0.0029	-0.99	0.0002	0.0003	0.65
11	-0.0023	0.0019	-1.17	0.0001	0.0030	0.03	-0.0003	0.0003	-0.79
12	-0.0037	0.0023	-1.64	0.0066	0.0034	1.92	-0.0006	0.0004	-1.48
13	-0.0010	0.0021	-0.51	0.0026	0.0033	0.81	-0.0003	0.0004	-0.87
14	-0.0035	0.0019	-1.83	0.0084	0.0031	2.66	-0.0009	0.0004	-2.45
15	0.0035	0.0031	1.15	0.0020	0.0031	0.62	-0.0003	0.0004	-0.91
16	-0.0013	0.0026	-0.50	0.0041	0.0031	1.32	-0.0005	0.0003	-1.54
17	-0.0022	0.0026	-0.87	0.0045	0.0035	1.29	-0.0003	0.0004	-0.87
18	-0.0065	0.0015	-4.45	0.0007	0.0033	0.20	0.0000	0.0004	-0.09
19	-0.0011	0.0021	-0.52	0.0014	0.0031	0.45	-0.0002	0.0004	-0.66
20	-0.0037	0.0022	-1.66	0.0072	0.0032	2.24	-0.0008	0.0004	-2.17
F-test for joint significance of β_w^i	F = 2.31 p = 0.000			F = 1.19 p = 0.200			F = 1.02 p = 0.428		
F-test for $\beta^i_{in[-4;4]} = \beta^i_{out[-4;4]}$	F = 28.59 p = 0.000			F = 1.59 p = 0.207			F = 1.71 p = 0.191		

Note: Number of obs.: $2,380,669$. Number of firms, i.e., clusters: $32,235$. R-sq, within: 0.65%. R-sq, between: 0.22%. Coefficients at cash inflows are suppressed from the table for brevity, they are statistically significant.

Table A.4: Regression results, Specification 2

Election week w	Coef. γ_w^1	Std. Err.	t	Coef. γ_w^2	Std. Err.	t	Coef. γ_w^3	Std. Err.	t
-20	0.00091	0.00077	1.18	-0.00014	0.00011	-1.27	0.00002	0.00001	1.24
-19	0.00248	0.00087	2.87	0.00032	0.00012	2.70	-0.00003	0.00001	-2.25
-18	-0.00078	0.00046	-1.68	-0.00014	0.00010	-1.39	0.00002	0.00001	1.49
-17	0.00020	0.00060	0.34	-0.00007	0.00010	-0.65	0.00000	0.00001	0.43
-16	0.00003	0.00067	0.04	0.00010	0.00011	0.92	-0.00001	0.00001	-0.98
-15	0.00085	0.00076	1.11	0.00040	0.00012	3.23	-0.00004	0.00001	-3.05
-14	-0.00069	0.00044	-1.56	-0.00026	0.00009	-2.82	0.00003	0.00001	2.43
-13	-0.00023	0.00049	-0.48	0.00006	0.00010	0.60	-0.00001	0.00001	-0.82
-12	0.00189	0.00087	2.18	0.00035	0.00011	3.09	-0.00004	0.00001	-3.20
-11	0.00116	0.00069	1.67	0.00040	0.00012	3.39	-0.00004	0.00001	-3.16
-10	0.00113	0.00076	1.50	0.00066	0.00012	5.29	-0.00007	0.00001	-5.07
-9	0.00030	0.00067	0.44	0.00006	0.00011	0.53	0.00000	0.00001	-0.34
-8	0.00077	0.00070	1.10	0.00023	0.00011	2.05	-0.00003	0.00001	-2.05
-7	0.00148	0.00081	1.84	0.00022	0.00011	2.04	-0.00002	0.00001	-2.00
-6	0.00162	0.00078	2.07	0.00060	0.00012	4.85	-0.00007	0.00001	-4.59
-5	0.00006	0.00058	0.11	-0.00005	0.00010	-0.45	0.00000	0.00001	0.42
-4	0.00070	0.00064	1.10	-0.00007	0.00011	-0.62	0.00000	0.00001	0.28
-3	0.00127	0.00072	1.77	0.00025	0.00011	2.21	-0.00003	0.00001	-2.31
-2	0.00187	0.00090	2.08	0.00073	0.00013	5.83	-0.00008	0.00001	-5.61
-1	0.00115	0.00077	1.48	0.00025	0.00011	2.25	-0.00003	0.00001	-2.33
1	0.00047	0.00064	0.74	0.00026	0.00012	2.23	-0.00003	0.00001	-2.08
2	0.00454	0.00103	4.39	0.00049	0.00013	3.82	-0.00005	0.00001	-3.50
3	0.00449	0.00097	4.61	0.00103	0.00014	7.20	-0.00011	0.00002	-6.49
4	0.00241	0.00088	2.75	0.00032	0.00012	2.65	-0.00004	0.00001	-2.56
5	-0.00068	0.00050	-1.36	-0.00039	0.00010	-3.98	0.00004	0.00001	4.02
6	0.00022	0.00053	0.42	-0.00012	0.00011	-1.16	0.00001	0.00001	1.20
7	-0.00040	0.00053	-0.75	-0.00006	0.00011	-0.52	0.00001	0.00001	0.65
8	-0.00081	0.00051	-1.60	0.00063	0.00013	5.04	-0.00007	0.00001	-4.77
9	-0.00043	0.00043	-1.01	0.00000	0.00010	-0.03	0.00000	0.00001	-0.15
10	-0.00016	0.00056	-0.29	0.00003	0.00011	0.31	-0.00001	0.00001	-0.54
11	-0.00038	0.00049	-0.78	0.00009	0.00011	0.83	-0.00002	0.00001	-1.42
12	-0.00075	0.00051	-1.45	0.00038	0.00012	3.17	-0.00004	0.00001	-2.92
13	0.00036	0.00052	0.70	0.00003	0.00011	0.23	-0.00001	0.00001	-0.45
14	-0.00021	0.00053	-0.39	0.00025	0.00010	2.44	-0.00003	0.00001	-2.44
15	0.00034	0.00060	0.57	0.00024	0.00011	2.14	-0.00003	0.00001	-2.23
16	-0.00041	0.00051	-0.81	0.00024	0.00011	2.19	-0.00003	0.00001	-2.15
17	0.00023	0.00062	0.37	0.00038	0.00012	3.22	-0.00004	0.00001	-2.83
18	-0.00086	0.00054	-1.61	-0.00006	0.00010	-0.59	0.00001	0.00001	0.73
19	-0.00059	0.00049	-1.21	0.00010	0.00010	0.97	-0.00001	0.00001	-0.98
20	-0.00018	0.00068	-0.26	0.00017	0.00011	1.55	-0.00001	0.00001	-1.15
F-test for joint significance of γ_w^i	F = 1.98 p = 0.000			F = 4.99 p = 0.000			F = 4.42 p = 0.000		
F-test for $\gamma_{in[-4;4]}^i = \gamma_{out[-4;4]}^i$	F = 25.43 p = 0.000			F = 23.05 p = 0.000			F = 20.90 p = 0.000		

Note: Number of obs.: $2,380,669$. Number of firms, i.e., clusters: $32,735$. R-sq, within: 2.27%. R-sq, between: 3.73%. Coefficients at cash inflows are suppressed from the table for brevity, they are statistically significant.

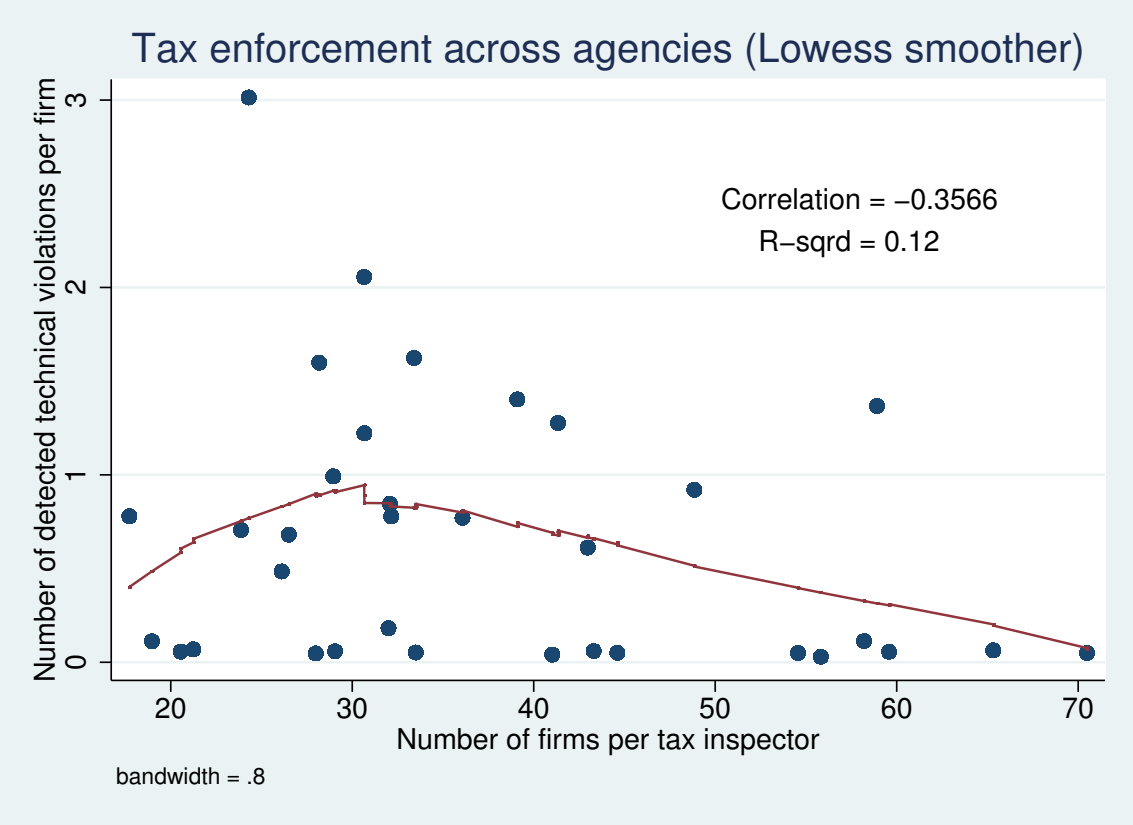


Figure 6: Tax enforcement across Moscow city tax agencies
The number of firms assigned to one tax agent negatively correlates with the number of violations detected per firm

B Data Appendix

B.1 Data Sources Used in Addition to Banking Transactions Data

In addition to the list of banking transactions, we use two other data sources. The first source is the Rosstat (Russia's official statistical agency) database of Russian companies provided by *Spark* (http://www.ispark.ru/en-US/default.aspx). This database contains a firm's INN, name, region, date of registration, industry, directors, owners, and other identifying information about the firm. In addition, it contains basic accounting data, such as revenue, profit, net income, assets, and debt. According to Russian law, all firms (even small ones) must report their balance sheets and income statements to Rosstat on a quarterly basis. Although this law does not set any explicit penalty for firms that do not report, the majority of Russian firms report their data to Rosstat to maintain good relations with the tax authorities. Rosstat contains accounting data for about 1.5 million Russian firms.

The second dataset includes the personal income of Moscow residents. It contains more than 7 million records for 2002, and more than 9 million records for 2003 and 2004. Each entry contains unique identification data (name, address, identification number) for both employer and employee. There can be multiple records per person if a person receives income from several sources. Guriev and Rachinsky (2006) use these data to measure income inequality in the presence of super-rich individuals. We use this dataset to get the number of tax agency employees.

B.2 Description of Variables Used in the Analysis

B.2.1 Sample: firms x elections

- Revenue, 2003 - Companys book revenue in 2003 taken from Rosstat
- Assets, 2003 - Companys book assets in 2003 taken from Rosstat
- Net Income, 2003 - Companys net income in 2003 taken from Rosstat
- Debt, 2003 - A sum of companys short term debt in 2003 and long term debt in 2003, which are taken from Rosstat
- Annualized transfers to fly-by-night firms 1999-2004 - Total transfers to fly-by-night firms by a firm in 1999-2004 divided by six. The transfers to fly-by-night firms are calculated using the banking transaction database. See Mironov (2011) for a detailed description of the identification procedure of fly-by-night firms. These firms are referred to as "spacemen" in Mironov (2011).
- Annualized revenue from procurement, 1 year after election - Total transfers from government-affiliated entities to a firm, which have the reported purpose of payment for goods and services during one year after the election. These transfers are calculated using the banking transaction database. In the baseline analysis, we exclude payments for utilities, e.g., electricity and water, from the list of revenues from public procurement contracts because these contracts are not usually allocated on a competitive basis

and are automatically allocated to local monopolists. If the election date happened to be in 2004, then we divide the total transfers by the number of days left until the end of 2004 and multiply by 365 (to get annualized revenue from procurement).

- Shadow transfers per week, election window - We calculate total transfers to fly-by-night firms by a firm from 4 weeks before until 4 weeks after the election and divide by 8. If the election period overlapped with our sample period and is shorter than 8 weeks, then we divide the transfers to fly-by-night firms by the actual number of weeks presented in our sample period (1999-2004). For example, if the date of the election is 2004-12-19, then we divide the transfers to fly-by-night firms by 5.7.

- Shadow transfers per week, outside election window - We calculate total transfers to fly-by-night firms by a firm starting one year before the election date and ending 4 weeks before the election. Then, we divide this number by the actual number of weeks presented in our sample period.

- Perceived corruption - see section 4.2 of the paper for description

- Tax-agency-level corruption - see section 5 of the paper for description

- Number of employees - number of firms employees in 2003 taken from Rosstat

B.2.2 Sample: Firms x Weeks

- Shadow transfers per week - Transfers to fly-by-night firms during a specific week.

- Shadow transfers per year - Annualized transfer to fly-by-night firms from one year before until one year after the election. For example, if the election date is 2004-03-14 then we take total transfers to fly-by-night firms from 2003-03-14 to 2004-12-31. After that we divide this number by the number of days in the period 2003-03-14 2004-12-31 and multiply by 365. If the election date is 2003-12-07 then we take total transfers to fly-by-night firms from 2002-12-07 to 2004-12-07 and divide them by 2.

- Revenue - Companys book revenue in 2003 taken from Rosstat

- Procurement revenue - Annualized transfers from government-affiliated entities from one year before until one year after the election. We include only transactions that have the reported purpose of payment for goods and services. We exclude payments for utilities, e.g., electricity and water, from the list of revenues from public procurement contracts.