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

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Abstract

Using unique objective micro-level data on tunneling for the population of large firms in Russia, we document pervasive corruption in allocation of public procurement contracts. We show that corruption exhibits political cycle: firms with public procurement revenue provide shadow financing for regional election campaigns. Using variation in the quality of tax inspectors as a source of exogenous variation in tunneling, we document a causal relationship from shadow campaign financing to obtaining procurement contracts. The relationship between shadow campaign financing and allocation of government procurement yields a locality-level measure of corruption. Based on 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; Olken and Pande, Forthcoming). 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), natural experiments, by looking at effect of revenue windfalls (e.g., Caselli and Michaels, 2009), and field experiments, by randomizing incentive schemes for corrupt behavior (e.g., Bertrand, Djankov, Hanna and Mullainathan, 2007; Olken, 2007; Ferraz and Finan, 2008). 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 based on objective data for the near-population of Russia'a large 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 and relate it to the probability that the firms obtained procurement contracts from the government.

Our main findings are as follows. We document pervasive corruption in allocation of public procurement. We show that illicit payments from firms to politicians exhibits political cycle. We provide some evidence that political connections are an important mechanism behind the relationship between between illicit payments from firms to politicians around election time and allocation of public procurement contracts. However, we show that it is not the only channel, as there is a causal relationship from tunneling around elections to obtaining procurement contracts. Finally, we show that corruption leads to an efficiency loss

in the allocation of public procurement.

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 available on the Internet, previously used by Mironov (Forthcoming). 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 large 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 firm with public procurement contracts in Russia tunnels 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 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 Moscow mayor in December 2003 and ending six months

[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.

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 exact sum tunneled in these ten months outside election period 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.[2]

The Inteko case shows 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 (albeit important) 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 firm's 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

[2]The shadow campaign financing by businessmen seems even higher at the national level: According to the testimony of a Russian tycoon Roman Abramovich in the London High Court of Justice, he together with another Russian tycoon, Boris Berezovsky, spent 50 million U.S. dollars supporting presidential election campaign of Mr. Putin in 2000 (see *the Wall Street Journal* article "Russian Tycoons Face Off in Court," November 7, 2011).

conclude that corruption has negative welfare implications and is not just an example of "efficient greasing" (Leff, 1964).

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, Di Tella and Schargrodsky, 2003; Reinikka and Svensson, 2004; Bertrand, Djankov, Hanna and Mullainathan, 2007; Olken, 2007; Fisman and Miguel, 2007; Caselli and Michaels, 2009; Cheung, Rau and Stouraitis, 2011; Ferraz and Finan, 2011, 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 large firms and show that corruption exacerbates inefficiencies. Previous estimates of corruption in Russia were based on perceptions and/or 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; Leuz and Oberholzer-Gee, 2006). Political connections drive an important part of the OLS evidence presented in this paper as well, as is illustrated by the case of Inteko. We, however, also present IV evidence of a causal link between shadow campaign financing and the allocation of procurement contracts, which is not driven by the presence of political connections. Our study is particularly related to studies which show that political connections in part determine allocation of government procurement contracts (e.g., Goldman, Rocholl and So, 2011; Amore and Bennedsen, 2010). In addition, there is a related body of research which shows benefits of official campaign financing for firms (e.g., Claessens, Feijen and Laeven, 2008; Cooper, Gulen and Ovtchinnikov, 2010). In contrast to these papers, we show that the unofficial campaign financing also may result in benefits allocated to firms in the form of government procurement.

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 illegal cash bribes that firms pay to politicians in order to obtain procurement contracts. Related to our findings, Burgess et al. (2011) show a political cycle in another illegal activity which brings cash to incumbents, namely, forest extraction in the tropics. Our finding that firms provide benefits to politicians in the face of elections is also 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) and tunneling by Russian firms (Desai, Dyck and Zingales, 2007; Mironov, Forthcoming). Our work has some methodological parallels with other papers which use unconventional data sources to provide empirical evidence on the questions that cannot be studied using conventional data (e.g., Levitt and Venkatesh, 2000; Guriev and Rachinsky, 2006; Braguinsky, Mityakov and Liscovich, 2011; Mironov, Forthcoming).

The 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

2.1 Legitimacy and Reliability of the Banking Transactions Data

Our main source of data is the data base of banking transactions among legal entities in Russia between 1999 and 2004 allegedly leaked to the public domain from the Russian Central Bank in 2005 and available freely on the Internet. These data are available both for free and also for a symbolic payment from several websites: *www.vivedata.com*, *www.rusbd.com*, *www.wmbase.com*, *www.mos-inform.com*, *www.specsoft.info*, etc. The russian press discussed widely the incident of appearance of these data in public domain.[3] The websites that demand the symbolic payment primarily charge for the service they provided by formatting the dataset to make the data more easily accessible rather than for the dataset itself, as it is also available for free. As the data were appeared in the public domain presumably without an official permission of the Central Bank of Russia, it is important to note that the Russian Government and Russian Central Bank are aware of the usage of these data by journalists and researchers and publicly discuss policy-relevant conclusions of the analyses based on the data (see, for instance, transcript of the Conference on Tax Evasion at the Ministry of Economy that took place in October of 2006 in Moscow. In addition, the authors of this paper received a request from the First Deputy Chairman of the Central Bank of Russia,

[3]See, for instance, publication in the main Russia business daily *Vedomosti* on March 30, 2005.

Andrei Kozlov, and a Deputy Chairman, Viktor Melnikov, to write a policy memo explaining the methodology of identifying fly-by-night firms using the banking transactions data as developed by Mironov (Forthcoming). In this request, the top Central Bank officials refer to "the data set from the Internet" as a legitimate source of information and acknowledge that the research department of the Central Bank uses the same data.[4] The fact that the government and the Central Bank officials take the results of research based on these data seriously is an indication of the reliability of these data.

As far as the legal issues associated with the use of these data is concerned, no lawsuits have been initiated against any party for using these data (despite a fairly large circulation of the data and publications by both journalists and researchers).[5] Lawyers within the Ministry of Interior of Russian Federation, when commenting in the press on the legitimacy of these data, explained that the Russian Central Bank never admitted that any data were leaked from the Bank and, therefore, from a legal standpoint, all data sets in the public domain are legal and no dataset is considered as illegitimate.[6]

A detailed description of the data and several important reality checks on them were done in Mironov (Forthcoming). These reality checks lead to the following conclusions. First, the Banking Transactions data match rather well with the registry of Russian firms published by the Russia's official statistical agency *Rosstat* for the group of firms that actually pay taxes (i.e., legitimate firms, as defined precisely in the next section, and used as a unit of observation in the analysis that follows) and do not match for firms, which do not pay taxes (i.e., fly-by-nights, also as defined in the next section). Second, for the firms that are present both in the Banking Transactions data and the registry, firm characteristics – available in both data sets – exactly coincide, which is another important sign of reliability of these data.

2.2 Samples and Variables

The data set used in this paper comes from the following sources. The data for 2003 and 2004 were used by Mironov (Forthcoming) and come from *www.vivedata.com*. The data

[4] A copy of the letter is available from the authors upon request.

[5] For an example of a journalist investigation using these data, see *Vedomosti* on May 20, 2005; for popular descriptions of research based on these data, see *Vedomosti* on July 24, 2006 and October 25, 2011. For research on similar datasets, see, for instance, Guriev and Rachinsky (2006) and Braguinsky, Mityakov and Liscovich (2011).

[6] See, for instance, *Financial-economic news* published by *Interfax*on April 1, 2005. See also an article, published in a journal specializing on covering the banking sector, *Bankovskoye Obozreniye (Banking Review)*, No. 11, November 2005, in which an economist from the Central Bank explains the phenomenon of the leakage of these data by the excessive regulation of secrecy and the lack of financial transparency regulations, he argues for the need to make the data officially public, see *http://bankir.ru/publikacii/s/provodki-cb-rf-vorovat-nelzya-pokypat-1378429/*.

for 1999-2002 come from *www.rusbd.com*. The data set contains 513, 169, 660 transactions involving 1, 721, 914 business and government legal entities and self-employed entrepreneurs without a legal enterprise status, with information on the date of each transaction, its payer, recipient, the amount of each transaction, and the self-reported purpose of it.

Our aim is to test for a relationship between transfers to fly-by-night firms from regular non-government firms around elections, and the public procurements contracts that these 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. We describe how we construct these variables below. As for the sample of firms, we take the universe of all entities present in the banking transaction data and eliminate all government and municipal entities, all firms with 100% state or municipal ownership, all financial institutions, all foreign companies and all self-employed entrepreneurs without a legal enterprise status. This procedure eliminates little over 85% of all entities present in the Banking Transactions data. The reminder is comprised of a near-population of domestic, non-financial, non-government business legal entities, which are the focus of our analysis. As we describe below, we further narrow the sample by eliminating firms of a small size, as they are both unlikely to get government procurement contracts and unlikely to finance elections, thus, they just add noise to our estimation (note, however, that all the results do go though if we keep small firms in the sample).

First, we follow Mironov (Forthcoming) 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 regular firm in our sample. Mironov (Forthcoming) 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 of regular – i.e., legitimate – firms (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 results.

We also collect data on the basic characteristics of regular legitimate 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*). This is the most recent registry that 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.[7]

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

These criteria yield 52,073 regular 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 as our main focus in this part of the analysis is dynamics of transfers to fly-by-night firms.[8] 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 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.

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

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.[9] 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 β_w^1 and β_w^2) are also unaffected 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.

[9]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 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 elections as 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 these controls does not affect the main result. As above, the error term is clustered at the firm level.

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 Cycle in Corruption

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 firm's 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 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 (Forthcoming) 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.[10] 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.

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 round; 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.

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. The magnitude of the cycle is exactly the same (and large) for elections in which the winner won in the first round and those in which the incumbent won in the first round (i.e, as long as the share of the incumbent's vote is over 50%).

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

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 incumbents, who run better campaigns, 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.[11]

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) 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 ("white transfers") 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 trends in regions or firms, 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 or differential trends between firms with and without

[11] 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.

government procurement revenue.

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 associated with distribution of public procurement in Russia was about 2.5 million USD.[12] 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 to be 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

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

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 (Forthcoming) 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 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.[13] Nonetheless, we cannot rule this possibility

[13]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. Court's Aid in Russian Case" of July 31, 2009 and "Russian Officials Said to Reap Wealth in Tax Case" of April 18, 2011.

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

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

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 M_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.[14] 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

[14]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.

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 dummies are used as excluded instruments.[15]

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

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

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 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.[16]

4.1 Results: The Effect of Shadow Transfers on Procurement

Tables 3 and 4 present the results of the estimation of equations 3 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

[15] Altogether, there are 93 tax agencies with at least 50 firms with the same actual and legal address assigned to them, some of which are in the regions which had several elections over the sample period.

[16] 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.

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 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 made inside 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.[17] 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 3 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

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

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.

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 against both possibilities (see, for instance, the survey 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 local level (defined by boundaries of districts for each tax agency). Intuitively, our corruption measures reflect is the strength of the association between shadow campaign financing and distribution of government procurement contracts in each locality. Considering the extent of corruption in procurement contracts at the level of locality is meaningful, as it is equivalent to small towns and boroughs of large cities. A lot of public procurement contracts are allocated at this level. We estimate the specification analogous to Equation 3 with shadow transfers inside the election window interacted with dummies for each locality l as the main variables of interest:

$$Prob[ProcR_{fe} > 0] = \sum_{l} \alpha_{1l} L_l \log(1 + ST_{fe}^{election}) + \alpha_2' X_f + \alpha_3 S_f + \alpha_4 L_{l_f} + \tau_e + \varepsilon_{fe}, \quad (5)$$

where L stands for locality dummy and l indexes localities. In order to estimate α_{1l} precisely, we restrict the sample to localities with at least 80 firms, out of which at least three have government procurement contracts. Using the results of this estimation, we define two alternative measures of local corruption in allocation of public procurement: (1) a dummy variable C_l^d indicating that the estimate of α_{1l} for the locality l is positive and statistically

significant at 10% level; and (2) a continuous measure C_l^c equal to the magnitude of the estimate of α_{1l} irrespective of its statistical significance. Both measures are defined for 59 regions and 206 localities. C_l^c varies across all localities in each region. C_l^d equals zero in every locality in 40 regions, equals 1 in every locality in 4 regions; and there is a within-region variation in C_l^d in 15 regions. Overall, the coefficients α_{1l} are positive and significant in 40% of localities.[18] Summary statistics for these variables are presented in Table A.5. The two measures of local corruption are complementary, as the first one has much larger variation and the second one is more precise. As shown below, our results are robust to using either of the two measures.

As the final step of our analysis, we regress a firm-level dummy for having won government procurement contracts (relative to the size of the firm, i.e., 0.1% or 1% of total revenue) on firms' labor productivity, a measure of local corruption, and their interaction, controlling for all our standard firm-level controls, as well as industry, region, and year dummies:

$$Prob[\frac{ProcR_{fe}}{R_f} > 0.01] = \gamma_1 Prod_{fe} + \gamma_2 C_{l_f} + \gamma_3 Prod_{fe} C_l + \gamma_4' X_f + \gamma_5 S_f + \gamma_6 M_f + \tau_e + \varepsilon_{fe}. \quad (6)$$

$Prod_{fe}$ stands for log labor productivity and C_{l_f} is one of the two corruption measures of locality l where firm f is located. As a robustness check, we estimate similar regressions adding locality fixed effects (i.e., dummies for each locality: L_{l_f}) to the list of controls and suppressing C_{l_f} as it varies only across localities:

$$Prob[\frac{ProcR_{fe}}{R_f} > 0.01] = \gamma_1 Prod_{fe} + \gamma_3 Prod_{fe} C_l + \gamma_4' X_f + \gamma_5 S_f + \gamma_6 L_{f_l} + \tau_e + \varepsilon_{fe}. \quad (7)$$

The coefficient on the interaction term $Prod_{fe}C_{l_f}$ in equations 6 and 7 shows whether in more corrupt localities public procurement contracts are allocated to more efficient firms or less efficient firms. It is worth noting that controlling for regional and industry effects is crucial because a substantial variation in procurement contracts and efficiency of firms are driven by unobserved regional and industry-level factors. The results are presented in Table 6. As dependent variable, the first four regressions in each panel have the dummy for 0.1% of revenue coming from public procurement contracts, while in the last four regressions, dependent variable is the dummy for 1% of revenue coming from procurement. The upper panel presents estimation of Equation 6 and lower panel – Equation 7. First, we find that government procurement contracts are, on average, directed to firms with lower revenue per worker (as the coefficient on labor productivity is negative and statistically significant irrespective

[18] In a few cases, estimates of α_{1l} are negative and significant at 5% level. However, this is well within statistical error, as this occurs in less than 3.8% of the cases.

of specification). This result has two alternative interpretations. One possibility is that the distribution of government procurement contracts is simply inefficient. The alternative story, however, is that government procurement is distributed in order to support higher employment for patronage reasons. Second, we find that firms located in more corrupt localities are more likely to obtain public procurement contracts (as can be seen from positive significant estimates of the coefficient on local corruption). The third finding is our main focus: higher corruption is associated with less productive firms obtaining public procurement contracts. The sign of the coefficients on the interaction term between labor productivity and both of our corruption measures is negative and statistically significant in all specifications. It is important to note that, in contrast to the interpretation of the direct effect of $Prod_{fe}$, the interpretation of the effect of the interaction between $Prod_{fe}$ and C_l has an unambiguous interpretation of an increase in the inefficiency in distribution of government procurement contracts. This is because in more corrupt localities, by construction of our corruption proxies, governments are less paternalistic on average as they distribute procurement contracts in exchange for financing election campaigns with illegal cash rather than in exchange for supporting higher levels of employment. The magnitude of the effects is as follows: we find that in non-corrupt localities government procurement contracts are allocated to firms which have 2% lower labor productivity compared to firms which do not win public procurement contracts in the same locality and same industry. In contrast, in corrupt localities, firms with procurement revenue are 4% less productive compared to non-recipients of public procurement contracts in the same locality and same industry. Therefore, corruption leads to substantial 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, the allocation of public procurement is less efficient with corruption.

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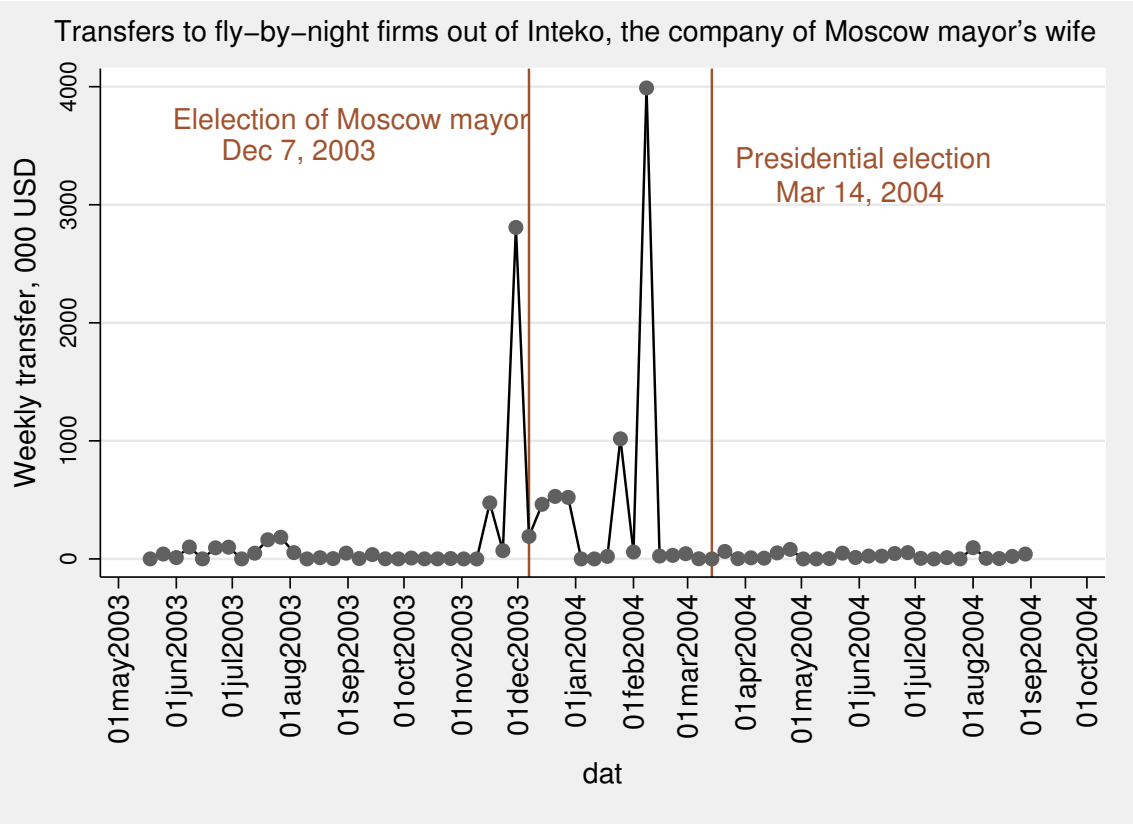


Figure 1: Transfers to fly-by-night firms by Inteko, the company of the wife of Moscow mayor

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

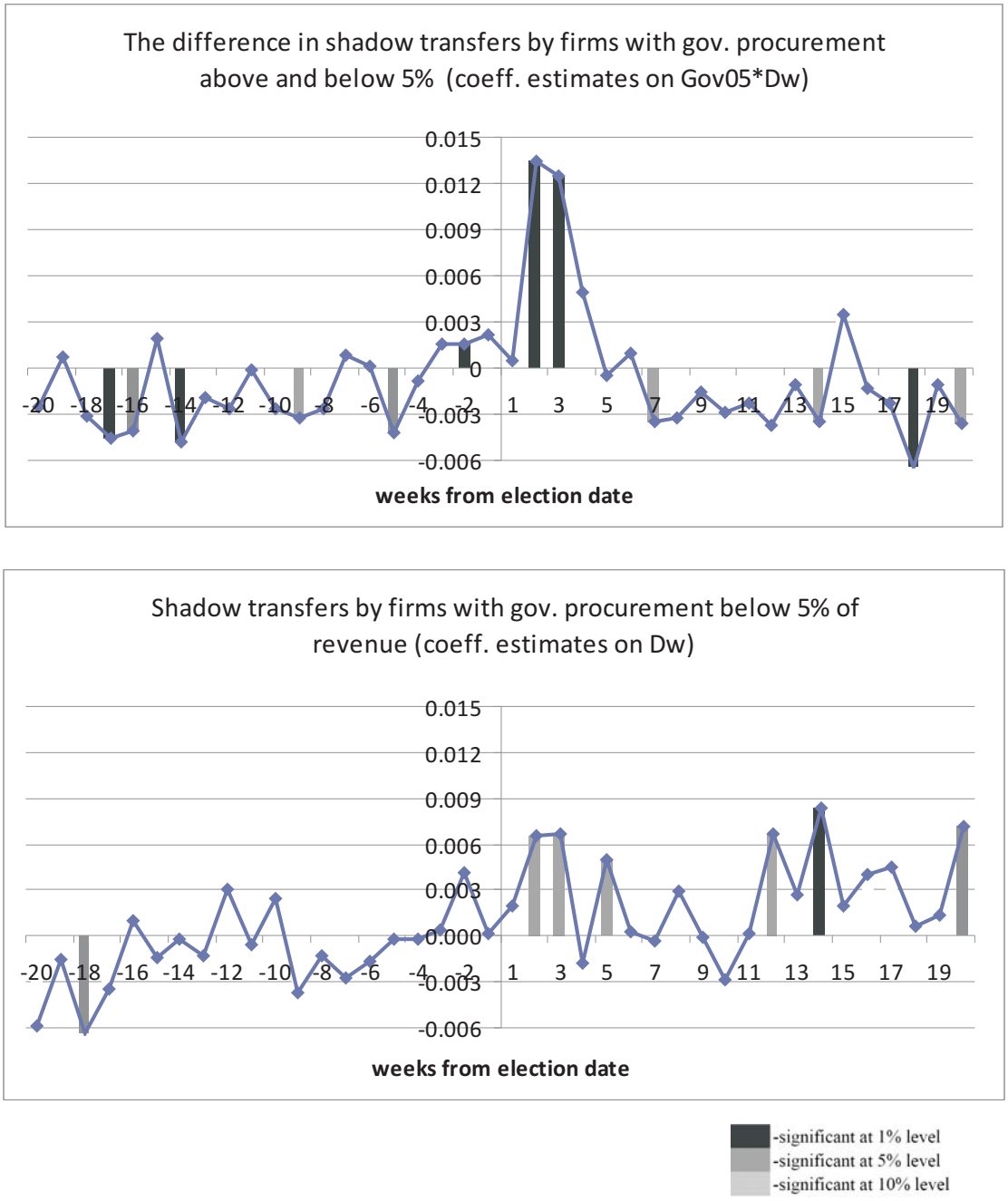


Figure 2: Political cycle in shadow transfers by firms with and without procurement contracts

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.

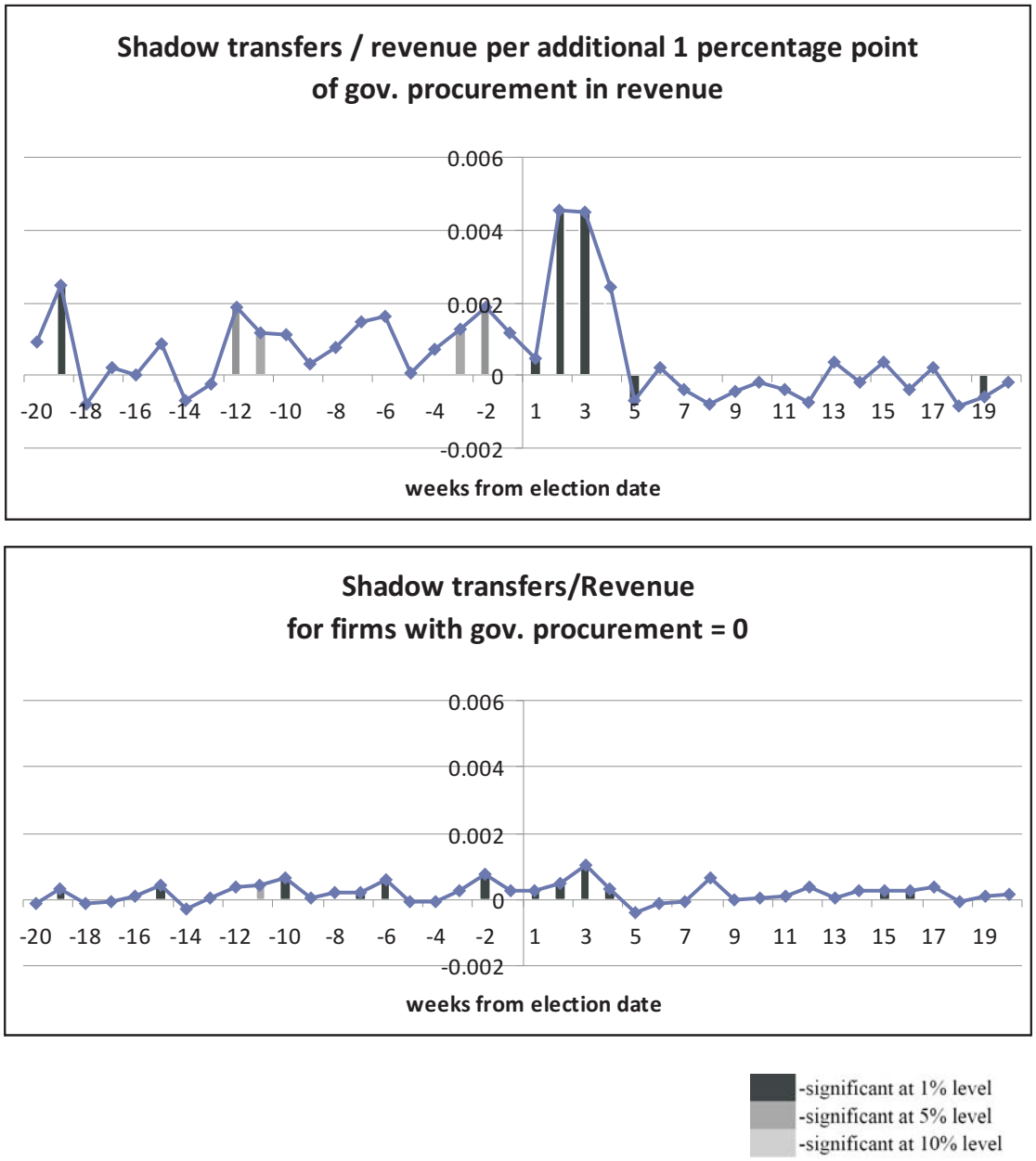
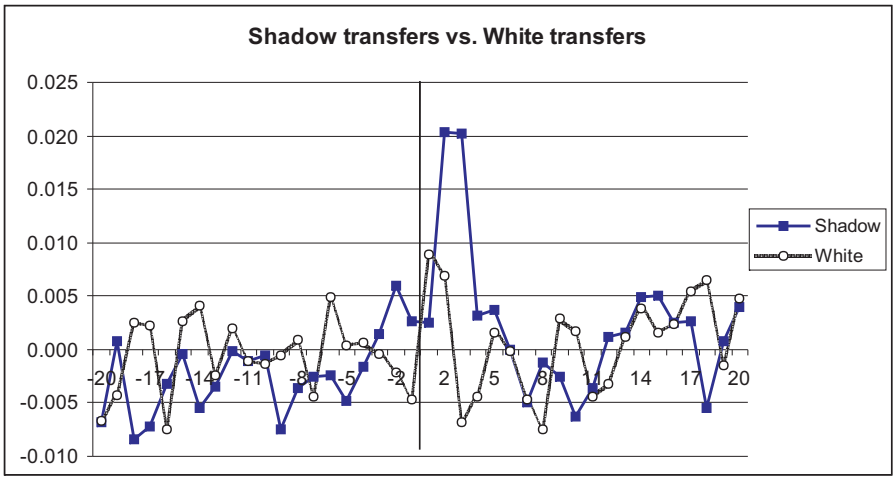


Figure 3: Political cycle in shadow transfers and the size of procurement contracts

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.

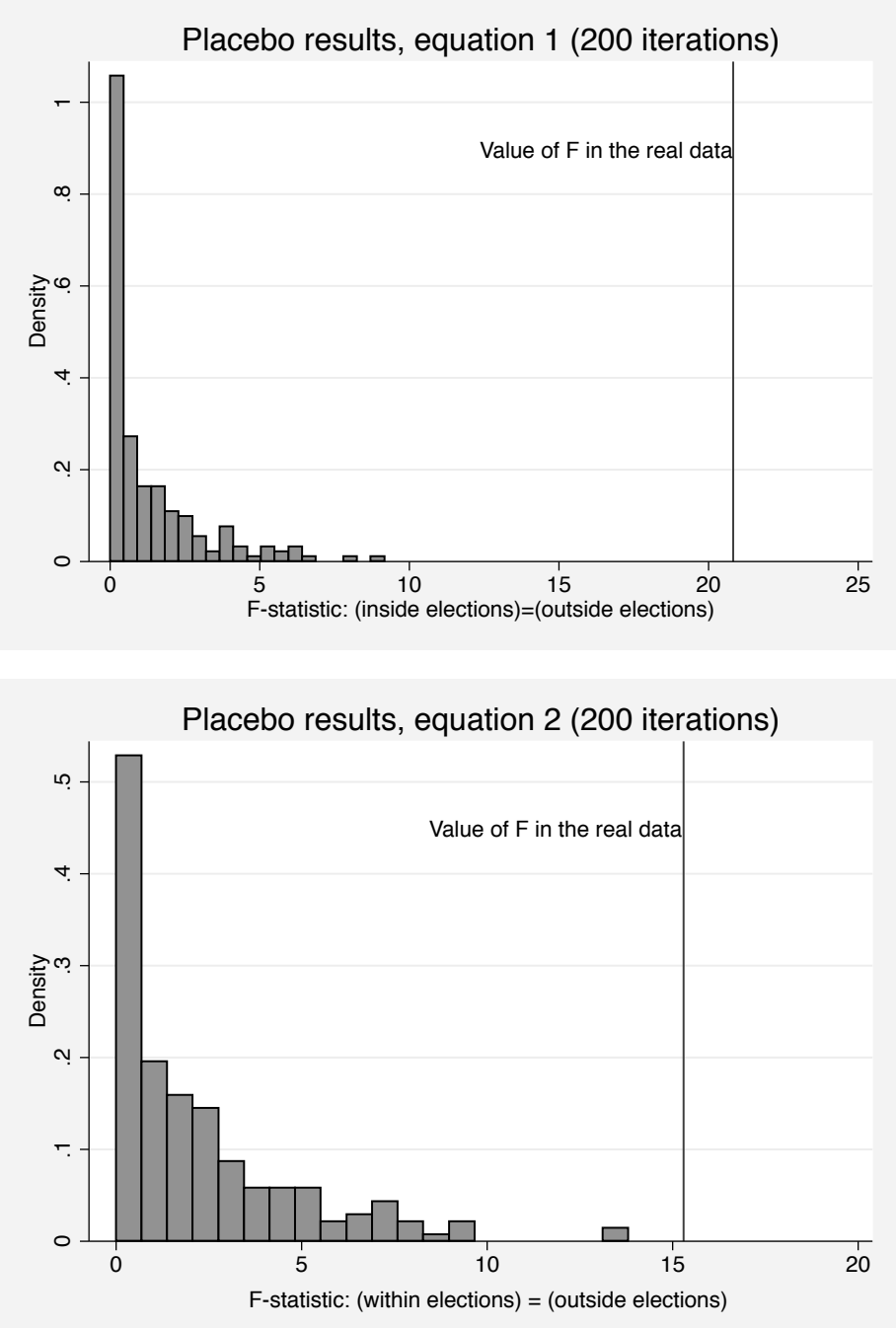


Figure 5: Placebo election dates

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: gov. procurement > 0.1 % of revenue				dummy: gov. procurement > 1 % of revenue			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local corruption measure	α_{1l}		dummy (α_{1l} is signif)		α_{1l}		dummy (α_{1l} is signif)	
Log labor productivity	-0.031	-0.025	-0.031	-0.020	-0.013	-0.009	-0.013	-0.007
	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Local corruption	0.139	0.520	0.010	0.069	0.093	0.353	0.006	0.040
	(0.069)**	(0.146)***	(0.007)	(0.013)***	(0.051)*	(0.105)***	(0.005)	(0.01)***
Log labor prod-ty x Local corruption		-0.129		-0.019		-0.088		-0.011
		(0.042)***		(0.004)***		(0.029)***		(0.003)***
Log(Revenue)	0.028	0.029	0.028	0.029	0.008	0.009	0.008	0.009
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Net Income/Revenue	-0.049	-0.049	-0.049	-0.053	-0.037	-0.038	-0.037	-0.040
	(0.022)**	(0.022)**	(0.022)**	(0.022)**	(0.016)**	(0.016)**	(0.016)**	(0.016)**
Debt/Assets	-0.056	-0.056	-0.056	-0.057	-0.032	-0.032	-0.032	-0.033
	(0.012)***	(0.012)***	(0.012)***	(0.012)***	(0.008)***	(0.008)***	(0.008)***	(0.008)***
Region FE	Y	Y	Y	Y	Y	Y	Y	Y
R-sq	0.10	0.10	0.10	0.10	0.06	0.06	0.06	0.06
Number of obs	34213	34213	34213	34213	34213	34213	34213	34213
Number of firms	18605	18605	18605	18605	18605	18605	18605	18605

Dependent var:	dummy: gov. procurement > 0.1 % of revenue				dummy: gov. procurement > 1 % of revenue			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local corruption measure	α_{1l}		dummy (α_{1l} is signif)		α_{1l}		dummy (α_{1l} is signif)	
Log labor productivity	-0.029	-0.024	-0.029	-0.020	-0.012	-0.008	-0.012	-0.007
	(0.003)***	(0.003)***	(0.003)***	(0.003)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Log labor prod-ty x Local corruption		-0.139		-0.020		-0.104		-0.012
		(0.042)***		(0.004)***		(0.029)***		(0.003)***
Log(Revenue)	0.027	0.029	0.027	0.030	0.008	0.009	0.008	0.009
	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Net Income/Revenue	-0.049	-0.049	-0.049	-0.053	-0.038	-0.037	-0.038	-0.039
	(0.021)**	(0.022)**	(0.021)**	(0.022)**	(0.015)**	(0.016)**	(0.015)**	(0.016)**
Debt/Assets	-0.052	-0.056	-0.052	-0.057	-0.029	-0.032	-0.029	-0.032
	(0.011)***	(0.012)***	(0.011)***	(0.012)***	(0.007)***	(0.008)***	(0.007)***	(0.008)***
Locality FE	Y	Y	Y	Y	Y	Y	Y	Y
R-sq	0.11	0.11	0.11	0.11	0.07	0.07	0.07	0.07
Number of obs	36801	34213	36801	34213	36801	34213	36801	34213
Number of firms	20792	18605	20792	18605	20792	18605	20792	18605

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

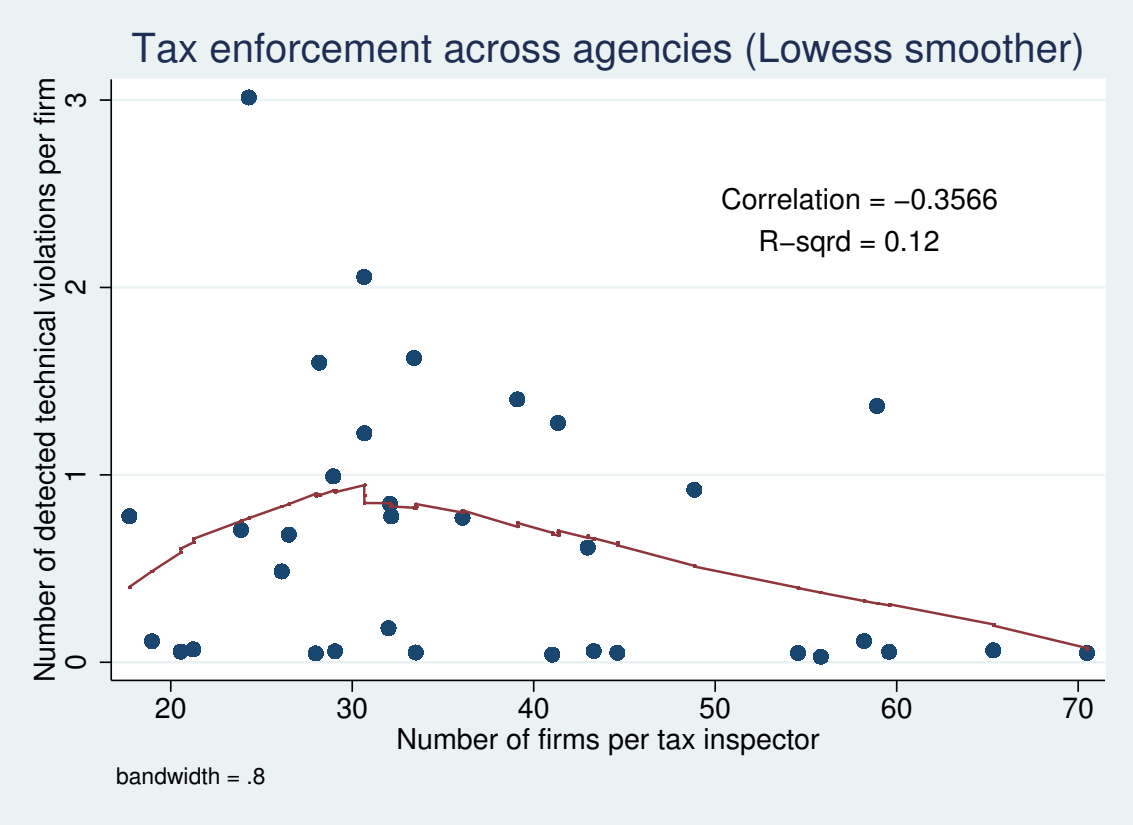


Figure 6: Tax enforcement across Moscow city tax agencies
The number of firms assigned to one tax agent negatively correlates with the number of violations detected per firm

Table A.5: Summary statistics for local corruption measures

	Obs	Mean	Std. Dev.	Min	Max
			Locality level		
Coefficient α_{1l}	206	0.053	0.046	-0.207	0.210
Dummy (α_{1l} is signif)	206	0.402	0.492	0.000	1.000
			Firm level		
Coefficient α_{1l}	34213	0.054	0.041	-0.207	0.210
Dummy (α_{1l} is signif)	34213	0.624	0.484	0.000	1.000

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 - Company's book revenue in 2003 taken from Rosstat
- Assets, 2003 - Company's book assets in 2003 taken from Rosstat
- Net Income, 2003 - Company's net income in 2003 taken from Rosstat
- Debt, 2003 - A sum of company's 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 firm's 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 from 2003-03-14 to 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 - Company's 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.