CESifo WORKING PAPERS

7393
2018

December 2018

Electoral Cycles in Perceived Corruption: International Empirical Evidence

Niklas Potrafke

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editors: Clemens Fuest, Oliver Falck, Jasmin Gröschl
www.cesifo-group.org/wp

An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: www.CESifo-group.org/wp

CESifo *Working Paper No. 7393*
Category 2: Public Choice

Electoral Cycles in Perceived Corruption: International Empirical Evidence

Abstract

I examine whether elections influence perceived corruption in the public sector. Perceived corruption in the public sector is measured by the reversed Transparency International's Perception of Corruption Index (CPI). The dataset includes around 100 democracies over the period 2012-2016, a sample for which the CPI is comparable across countries and over time. The results show that the reversed CPI was about 0.4 points higher in election years than in other years, indicating that perceived corruption in the public sector increased before elections. The effect is especially pronounced before early elections (1.0 points) compared to regular elections (0.4 points). Future research needs to investigate why perceived corruption in the public sector increased before elections.

JEL-Codes: C230, D720, H110, K400.

Keywords: perceived corruption, elections, political manipulation, panel data, democracies.

Niklas Potrafke
Ifo Institute – Leibniz Institute for
Economic Research
at the University of Munich
Poschingerstrasse 5
Germany – 81679 Munich
potrafke@ifo.de

November 2018
This paper has been accepted for publication in the Journal of Comparative Economics.
I am grateful for comments from Christian Bjørnskov, Ruben Enikolopov, Rajeev Goel, Jerg Gutmann, Heinrich Ursprung, and two anonymous referees and Lisa Giani-Contini for proofreading. Isaac Cohen, Anna Dudel, Friederike Fourné, Gil Geller, and Felix Michalik provided excellent research assistance.

1. Introduction

The political business cycle theories propose that politicians – party alignment notwithstanding – implement expansionary policies before elections (Nordhaus 1975, Rogoff and Sibert 1988, Rogoff 1990). Many empirical studies have examined whether election-motivated politicians manipulate macroeconomic variables in pursuit of re-election. Politicians have been shown to increase current expenditure at the cost of public investment, increase public health and other social expenditure, and boost short-term GDP growth before elections. Governments sugarcoat the budget balance by means of creative accounting – that is, "below-the-line" operations such as transactions in financial assets that do not show up in deficit figures, but give rise to changes in public debt. Monetary aggregates were increased before elections not just to promote economic performance but to buy votes. Donor countries have been shown to use foreign aid to manipulate elections in recipient countries (see, for example, Potrafke 2010, 2012a, Faye and Nierhaus 2012, Aidt et al. 2015, Reischmann 2016, for surveys De Haan and Klomp 2013 and Dubois 2016).

Election-motivated politicians may also manipulate figures that cannot be measured by macroeconomic variables. In electoral districts or other closely-knit jurisdictions where voters directly punish or reward political candidates, politicians may be expected to behave corruptly before elections. For example, interest groups lobby and bribe politicians to implement or refrain from implementing specific policies. Corruption in the public sector is expected to be especially pronounced before elections because interest groups know that politicians are keen to gratify the needs of their constituencies and may solicit bribes. Electoral motives have been shown to cause politicians to allocate public funds inefficiently (Finan and Mazzocco 2016). Politicians may want to buy votes before elections. On the other hand, corruption is not likely to increase before elections because voters dislike corrupt politicians. Empirical studies suggest that voters punish corrupt politicians. Using cross-sectional data for 35 countries, Krause and

Mendez (2009) show that when the perceived level of government corruption increased, the total number of votes obtained by an incumbent decreased. The perceived level of government corruption was measured by Transparency International's Corruption Perception Index (CPI). Goel and Mazhar (2015) measure corruption by alternative indices and do not arrive at the conclusion that voters punish corrupt politicians. Many other studies exploit micro-data on political scandals in individual countries such as Brazil, Germany, Spain, and the United States. The evidence is mixed on whether incumbents suffer electoral consequences when involved in political scandals (see, for example, Costas-Pérez et al. 2012, Ferraz and Finan 2008, Hirano and Snyder 2012, Kauder and Potrafke 2015, Rudolph and Däubler 2016).[1] Journalists enjoy revealing political scandals. In particular, before elections, journalists make an effort to reveal political scandals to jeopardize the election of promising challengers and/or entrenched incumbents. Politicians know that political careers can end if they get caught in political scandals. Thus, the extent to which elections influence corruption remains an empirical question.

Scholars have examined electoral corruption-cycles using regional data. A paper closely related to my study is Vadlamannati (2015). The author looks at whether the political business cycle theories apply to anti-corruption policies. He uses panel data for 30 Indian states over the period 1988-2009 and investigates whether incumbent governments were active in controlling corruption. The results show that the number of corruption cases registered by the individual state anti-corruption agency increased before regular elections, but did not increase before early elections. The effect of regular elections was large in "swing states," that is, states with a small margin between the winning party and the runner-up. The results did not, however, show that the number of corruption cases being investigated by the anti-corruption agency increased before elections. The author concludes that election-motivated politicians likely manipulate

[1] On electoral consequences of inconsistencies in the counting of votes see Potrafke and Roesel (2018).

macroeconomic variables as well as variables that track "key governance related issues" (p. 15).

In Russia, perceived corruption was also pronounced before elections.[2] Sidorkin and Vorobyev (2018) find that corruption perceived by firms increased at the end of regional governors' terms. The dataset includes some 5000 firms in 37 Russian regions over the period 2008-2012. The authors find that governors were especially active in corruption when they knew that their terms were about to expire and they would not be re-appointed. By contrast, when their re-appointment was likely, they were hardly inclined to participate in corrupt activities. Mironov and Zhuravskaya (2016) use data on allocation of public procurement (banking transactions among legal entities) in Russian regions over the period 1999-2004. The results show that politicians were more corrupt before elections to increase campaign contributions; firms bribed politicians to win procurement contracts. Bribing politicians to win procurement contracts was especially pronounced in regions that were corrupt as measured by Transparency International's regional CPI for Russia.

My study employs macro data and elaborates on international electoral cycles in perceived corruption. Using balanced panel data for around 100 democracies over the period 2012-2016, I examine whether perceived corruption in the public sector increased before elections. The measure of perceived corruption is the reversed CPI by Transparency International. The results show that the reversed CPI was about 0.4 points higher in election years than in other years. The effect is especially pronounced before early elections (1.0 points) compared to regular elections (0.4 points) and driven by parliamentary, not presidential elections. Future research needs to investigate why perceived corruption in the public sector increased before elections.

[2] In post socialist countries, previous membership in communist parties influences bribes to public officials today: Ivlevs and Hinks (2018) conclude that corruption of former communist party members, which was transmitted through family members, helps to explain corruption in socialist countries 25 years after the fall of the Iron Curtain. On corruption in Russia see also Schulze and Zakharov (2018) and Enikolopov et al. (2018).

2. Data

2.1 Transparency International's Perception of Corruption Index (CPI)

To measure perceived corruption in the public sector, I use the reversed Transparency International's Perception of Corruption Index (CPI) over the period 2012-2016 for around 100 democracies (balanced panel). I use the year 2012 as starting point because Transparency International changed the methodology to make the CPI comparable over time. The appendix lists the countries included. The reversed CPI assumes values between 0 (no corruption) and 100 (extreme corruption). To construct the CPI, Transparency International aggregates data from several sources to provide perceptions by business people and country experts about the level of corruption in the public sector (Transparency International 2018). For example, the CPI 2018 is calculated using 13 data sources from 12 institutions that capture perceptions of corruption within the past two years.

Scholars have criticized the CPI. A good example is Byrne (2010): "For instance, perception-based corruption indexes may influence the actual perception of corruption because of the media attention they receive, thus raising the possibilities that the indexes influence the very same perceptions on which they are based. This circularity reinforces perceptions of corruption, creating a vicious cycle between perception and fact. Therefore, the perception of corruption does not always reflect the reality or complexity of the actual level or experience of corruption within a country."[3] On the extent to which perceived and experienced corruption differ see Gutmann et al. (2015). Other criticism includes Arndt and Omann (2006), Galtung (2006), The Guardian (2013), and Donchev and Ujhelyi (2014). However, studies such as Fishman and Miguel (2007) and DeBacker et al. (2015) suggest that perceived corruption as measured by the CPI was quite correlated with more objective corruption measures. In any event, the CPI is a very prominent measure for perceived corruption and has often been used in

[3] https://politicalreform.ie/2010/10/27/problems-with-transparency-international-corruption-perception-index/ (accessed on 26 June 2018).

empirical research on corruption. See, for example, Paldam (2002), Méon and Sekkat (2005), Aidt (2009), Bjørnskov (2012), Vadlamannati and Cooray (2016), Debski et al. (2018).

One may well maintain that it matters when in a year – before or after elections – survey participants on perceived corruption are asked. When elections take place early in a year and the experts are asked after the elections, they might have in mind all the distress caused by the elections that recently took place. When the experts are asked before the elections, their answers may be confounded by media pushing potential scandals at the time of questioning. Ideally, I would therefore like to consider the timing of the surveys on perceived corruption. Doing so is however not possible because the CPI is based on 13 sub indicators with different procedures for asking survey participants. What is more, surveying participants does not always take place at the same time in every country.

The CPI (2018) is available for 180 countries (unbalanced panel). Given that I would like to examine the effect of elections on the reversed CPI, I focus on democracies. I use the new data on political institutions by Bjørnskov and Rode (2018) that updates the data by Cheibub et al. (2010). Countries are coded as democratic when elections are contested. 110 out of the 180 countries for which the CPI is available are classified as democracies in an individual year. I use the sample of 94 countries that have been democracies over the period 2012-2016 (on democratic transitions see Bjørnskov and Rode 2014) and the CPI is available for. For robustness checks, I will also use the new democracy indicator by Gründler and Krieger (2016) that uses machine learning techniques to identify democracies.

2.2 Elections

The baseline sample includes 137 country-year observations with parliamentary (lower house) and presidential elections (26 in 2012, 25 in 2013, 31 in 2014, 26 in 2015, and 29 in 2016). Every country in the sample experienced at least one parliamentary or presidential election. I

self-compile data for the timing of elections and whether an election was called early or not by using manifold sources in the internet. 33 countries have more than one country-year observation with one parliamentary or presidential election. The sample includes 59 country-year observations with presidential elections. In some countries such as Indonesia, parliamentary and presidential elections take place on the same date. 42 of the 59 country-year observations with presidential elections coincide with country-year observation with parliamentary elections. Out of the 137 country-year observations with elections, there were 78 with only parliamentary elections, 42 with both parliamentary and presidential, and 17 with only presidential elections.

19 out of the 137 elections have been called early (17 parliamentary and 2 presidential early elections). I consider elections to be early only when they take place at least one year prior to scheduled elections. I distinguish between the effects of regular and early elections on perceived corruption in section 3.3.

3. Empirical Analysis

3.1 Empirical Specification

The baseline panel data model has the following form:

$$Perceived\ corruption_{it} = \alpha\ Election_{it} + \Sigma_l \gamma_l X_{it} + \eta_i + \varepsilon_t + u_{it} \qquad (1)$$

where the dependent variable $Perceived\ corruption_{ti}$ describes the reversed CPI in country *i* in year *t*. The variable $Election_{ti}$ assumes the value 1 if a parliamentary election takes place in country *i* in year *t* and 0 otherwise. The vector *X* includes three control variables: real per capita GDP (in logarithms – inferences regarding the election effects do not change when I use real per capita GDP in levels), the political instability index (Kaufmann et al. 2010), and government final consumption expenditure (as % of GDP). η_i and ε_t are fixed country and fixed year effects. Per capita GDP and government expenditure have been shown to be negatively and political

instability to be positively correlated with corruption (Goel and Saunoris 2018). I estimate the baseline model by using Ordinary Least Squares (OLS) with standard errors clustered at the country level. Table 1 shows descriptive statistics for all variables.

My set of control variables is small for two reasons: firstly, I only include control variables that vary over time. Clearly, many variables have been shown to be correlated with the reversed CPI (e.g. intelligence – Potrafke 2012b), but most of these variables only vary across countries and not over time; and are therefore not useful in my panel data model with fixed country effects. Secondly, my time period 2012-2016 is already quite short and I do not wish to shorten the time period by including control variables that are not available for the year 2016 (or even 2015).

3.2 Baseline results

Table 2 shows the results of the baseline model. I present results including/excluding fixed country and year effects and the individual control variables showing the extent to which the electoral effects change when individual control variables are included/excluded.

The coefficient of the election variable has a positive sign but lacks statistical significance when fixed country effects are excluded (columns 1 and 2). When I include fixed country effects and hence control for time-invariant omitted variable biases, the size of the coefficient of the election variable increases and it is statistically significant at the 5% level in columns (3) to (8). The numerical meaning of the coefficient of the election year variable in column (4) – a panel data model including fixed country and fixed year effects with the maximum number of 470 observations – is that the reversed CPI was 0.42 points higher in election years than in other years. The point estimate of the election year variable is 0.47 in column (7) when the control variables are included (the sample includes 443 country-year observations in this specification). This effect represents 26% of the within-country standard

deviation in the reversed CPI of around 1.8 points. The coefficient estimates becomes somewhat smaller in column (8) which shows results when countries with early elections are excluded. The results in section 3.3 will corroborate that early elections have a large effect on perceived corruption.

Real per capita GDP (log) has the expected negative sign and is statistically significant at the 10% level in columns (5), (6) and (8). The political instability index does not have the expected positive sign, but lacks statistical significance. The effect of political instability on perceived corruption (Goel and Saunoris 2018) seems to be driven by cross-country, rather than within-country variation. Final government expenditure (as % of GDP) does not turn out to be statistically significant as well, a result of the correlation with per capita GDP.

3.3 Regular and early elections

Elections may well be called early. Reasons for early elections include coalition compulsions (e.g., withdrawal of support by a coalition partner) or strategic considerations of an incumbent government when chances for re-election might become worse in the future. Table 3 shows the regression results for when I distinguish between regular and early elections.[4] The coefficient estimate of the regular election variable is somewhat smaller than the coefficient estimate of the overall election variable in the baseline model and lacks statistical significance in column (1). It is statistically significant at the 10% level in columns (2) to (5) when control variables are included. By contrast, the coefficient estimate of the early election variable is almost three times as large as the coefficient estimate of the regular election variable. Clearly, this estimate is based on only 19 early elections. It is quite possible that perceived corruption increases before

[4] Countries with early parliamentary elections (country-year observations) are: Bulgaria, Croatia, Greece (two), Guyana, Israel, Japan, Luxembourg, Macedonia, Mauritius, Netherlands, Serbia (two), Slovakia, Slovenia, Spain, Ukraine. Countries with early presidential elections are: Sri Lanka and Zambia. In 2014, there were early parliamentary elections and regular presidential elections in the Ukraine. In 2015, there were regular parliamentary and early presidential elections in Sri Lanka.

early elections: journalists who hold back information on political scandals of incumbents may well also report on scandals before early elections, and lobbyists have opportunities to approach politicians when elections are called early, too.

It is also conceivable that perceived corruption in the public sector gave rise to early elections. Voters may observe corruption and demand to elect a new parliament. To elaborate on this issue in some more detail, I have examined the circumstances under which early elections were called in my sample. 15 out of 19 early elections were not called because of corruption of politicians and scandals, but 4 parliamentary elections did: the 2013 elections in Luxembourg (spy scandal), the 2016 elections in Macedonia (wiretap scandal), the 2013 elections in Slovakia (gorilla scandal) and the 2016 elections in Spain (scandals of the People's Party). These designations may be controversial; for example, one can debate the extent to which the scandals in Spain gave rise to early elections. Still, inferences do not change when I do not consider the scandals in Spain as having induced the early elections. Table 4 shows regression results for when I distinguish between early elections that were (not) called because of corruption of politicians and scandals. The results show that the effect of early elections called because of corruption of politicians and scandals is numerically large and statistically significant at the 1% level. In any event, the point estimate of the regular election variable is around 0.4 and statistically significant at the 10% level in columns (1) to (5).

3.4 Timing of elections

The timing of elections may well be considered in some more detail. The coding proposed by Franzese (2000) considers, for example, the exact timing of the election date. I replace my election dummy variable with a variable that still assumes the value zero in non-election years, and a value between 0.02 and 0.97 in election years. For example, the variable assumes the value 0.5 in election years when the election took place on June 30 and the value 1 when the

election took place on December 31. I include a second variable that assumes the complement value in the pre-election year: when an election took place early in a year, for example, the election year variable assumes the value 0.1 and the pre-election variable assumes the value 0.9 (the sum per election should be 1). When there were two parliamentary elections in an individual year, such as in Greece in 2012, I use the first election date in the year. In a similar vein, I use the first election date when there were both parliamentary and presidential elections in an individual year. The results in Table 5 show that using the exact timing of the elections also suggests an electoral cycle. The point estimates of the Franzese election variable in Table 5 are larger than the point estimates of the classical election dummy variable in the baseline model; this is in line with the different scales of the election variables (the standard deviation of the Franzese election variable in Table 5 is 0.27; the standard deviation of the classical election year dummy variable is 0.45).

Another alternative to measure the timing of the elections is to include dummy variables for pre-election, election and post-election years. Corruption is expected to increase in election and pre-election years, but not (or less so when scandals have repercussions) in post-election years. The results in Table 6 confirm these expectations: the election year variable is statistically significant at the 5% level in columns (1) to (5) and at the 10% level in column (6). The pre-election and post-election year variables have a positive sign, but lack statistical significance.

3.5 Income thresholds

I have examined subsamples by income and OECD-membership. One the one hand, one may well expect more contested elections in high-income and OECD countries than in low-income and non-OECD countries. Electoral cycles in perceived corruption may therefore by driven by high-income and OECD countries. On the other hand, corruption is more pronounced in low-income and non-OECD countries which, in turn, may give rise to also larger electoral cycles in

perceived corruption in low-income and non-OECD countries. The results for subsamples suggest (not shown) that electoral cycles in perceived corruption occur in low-income and non-OECD countries. The effects lack statistical significance in in high-income and OECD countries.

3.6 Disentangling parliamentary and presidential elections

The baseline model considers both parliamentary and presidential elections. I also examine electoral cycles based on parliamentary and presidential elections separately.[5] When I focus on parliamentary elections only, the results suggest electoral cycles before parliamentary elections (Table 7). When I focus on presidential elections only, the point estimate of the election year variable is positive but lacks statistical significance (Table 8).

3.7 Alternative measure for political institutions

I used the new data on political institutions by Bjørnskov and Rode (2018) that updates the data by Cheibub et al. (2010). One may well want to use an alternative measure for democracies which will, in turn, give rise to a different sample. I employ the new democracy indicator by Gründler and Krieger (2016) that uses machine learning algorithms for pattern recognition to identify democracies.[6] The binary index for democracies by Gründler and Krieger (2016) identifies some countries as democracies over the full period 2012-2016 which the index by Bjørnskov and Rode (2018) did not identify as democracies: Afghanistan, Bosnia and Herzegovina, Cambodia, Democratic Republic of the Congo, Gabon, Haiti, Iraq, Kosovo, Lesotho, Liberia, Montenegro, Morocco, Myanmar, Namibia, Nepal, Russia, South Africa, Tanzania, Togo, Turkey, and Venezuela. In a similar vein, the binary index for democracies by

[5] I focus on presidential elections with public votes; that is, I do not consider presidential elections in countries such as Germany where electoral delegates vote for the president.
[6] On how the measurement of democracy influences the democracy-growth nexus see Gründler and Krieger (2018).

Gründler and Krieger (2016) does not identify Nicaragua as a democracy over the full period 2012-2016, but the index by Bjørnskov and Rode (2018) does. When I use the somewhat larger sample of 114 democracies based on the binary index for democracies by Gründler and Krieger (2016) the inferences regarding the electoral effects on perceived corruption do not change (Table 9). The effects are driven by parliamentary elections.

4. Conclusion

Perceived corruption in the public sector was pronounced before elections. I use the reversed CPI to measure perceived corruption in the public sector over the period 2012-2016, a period for which the CPI is comparable over time and across countries. The reversed CPI was around 0.4 points higher in election years than in other years. When I control for early elections – also for those I know have been called because of corruption of politicians and scandals – the estimates suggest that the reversed CPI was larger by around 0.4 points in election years than other years. The results are driven by parliamentary, not presidential elections. My results for around 100 democracies corroborate findings based on regional variation in India and Russia (Vadlamannati 2015, Mironov and Zhuravskaya 2016, Sidorkin and Vorobyev 2018).

An important question is why perceived corruption in the public sector increased before elections. I acknowledge that I do not disentangle the reasons here; and this is certainly a key task for future research. Firstly, it is conceivable that lobby groups drastically increase their activities by approaching and, ultimately, bribing politicians before elections. Employers' associations may want to prevent increases in business taxes, lobbyists acting on behalf of carbon-intense producers may want to avoid (increases in) carbon taxes, other lobbyists wish to receive subsidies, etc. Politicians may be less likely to resist the increasing numbers of attractive offers by lobbying groups before elections. Secondly, the CPI is based on experts' ratings. The experts participating in the ratings tend to be more sensitive to the public sector in

election than non-election years because the media report on governmental issues quite frequently in election years. Thirdly, opposition parties, and especially the media, play an important role in perceived corruption in the public sector. Many political scandals leak out just a few months before elections. I conjecture that the timing of when political scandals become public is not exogenous. Opposition parties that wish to kick incumbent politicians out of office are very likely to make public information on political scandals available when potential damage is large. In a similar vein, journalists that do not support incumbent politicians are inclined to report on political scandals just before elections. In Italy, for example, many corruption scandals occurred in the regional health system: a rightwing newspaper was more active in reporting on leftwing politicians, and a leftwing newspaper was more active in reporting on rightwing politicians involved in corruption scandals before elections (Le Moglie and Turati 2018). Future research should investigate whether opposition parties and journalists hold back information on incumbents' political scandals and tend to report on such scandals immediately prior to elections instead.

Another avenue for research is to examine electoral cycles in real, not perceived corruption. Doing so requires, of course, to compile data on corruption cases, an admittedly ambitious undertaking.

References

Aidt, TS. 2009. Corruption, institutions, and economic development. *Oxford Review of Economic Policy* 25, 271-291.

Aidt, TS., Asatryan, Z., Badalyan, L., Heinemann, F., 2015. Vote buying or political (business) cycles as usual? *CESifo Working Paper No.* 5508.

Arndt, C., Oman, C. 2006. *Uses and abuses of governance indicators*. Paris: Development Centre of the Organization for Economic Co-operation and Development.

Bjørnskov, C. 2012. Can bribes buy protection against international competition? *Review of World Economics* 148, 751-775.

Bjørnskov, C., Rode, M. 2014. Democratic transitions and institutional change: what's behind the association? *University of Aarhus, Working Paper.*

Bjørnskov, C., Rode, M. 2018. Background paper: Regime types and regime changes: A new dataset. *University of Aarhus, Working Paper.*

Cheibub, J., Gandhi, J., Vreeland, JR. (2010). Democracy and dictatorship revisited. *Public Choice* 143, 67-101.

Costas-Pérez, E., Solé-Ollé, A., Sorribas-Navarro, P. 2012. Corruption scandals, voter information, and accountability. *European Journal of Political Economy* 28, 469-484.

Debski, J., Jetter, M., Mösle, S., Stadelmann, D. 2018. Gender and corruption: the neglected role of culture. *European Journal of Political Economy*, forthcoming.

De Haan, J., Klomp, J., 2013. Conditional political budget cycles: A review of recent evidence. *Public Choice* 157, 387–410.

DeBacker, J., Heim, B.T., Tran, A., 2015. Importing corruption culture from overseas: Evidence from corporate tax evasion in the United States. *Journal of Financial Economics* 117, 122-138.

Donchev, D., Ujhelyi, G. 2014. What do corruption indices measure? *Economics and Politics* 26, 309-331.

Dubois, E. (2016). Political business cycles 40 years after Nordhaus. *Public Choice* 166, 235-259.

Enikolopov, R., Petrova, M., Sonin, K., 2018. Social media and corruption. *American Economic Journal: Applied Economics* 10, 150-174.

Faye, M., Niehaus, P. 2012. Political aid cycles. *American Economic Review* 102, 3516-3530.

Ferraz, C., Finan, F. 2008. Exposing corrupt politicians: The effects of Brazil's publicly released audits on electoral outcomes. *Quarterly Journal of Economics* 123, 703-745.

Finan, F., Mazzocco, M., 2016. Electoral incentives and the allocation of public funds. *NBER Working Paper No. 21859.*

Fishman, R., Miguel, E. 2007. Corruption, norms, and legal enforcement: Evidence from diplomatic parking tickets. *Journal of Political Economy* 115, 1020-1048.

Franzese R. 2000. Electoral and partisan manipulation of public debt in developed democracies, 1956–1990. In *Institutions, Politics and Fiscal Policy*, Strauch, R, von Hagen, J (eds.). Kluwer Academic Press, Dordrecht: 61–83.

Galtung, F. 2006. Measuring the immeasurable: Boundaries and functions of (macro) corruption. In *Measuring Corruption*, ed. Charles Sampford, Arthur Shacklock, Carmel Connors, and Fredrik Galtung, 101–30. Aldershot, UK: Ashgate

Goel, R., Mazhar, U. 2015. A replication of corruption and elections. *Public Finance Review* 43, 143-154.

Goel, R., Saunoris, JW. 2018. Political uncertainty and international corruption. *Applied Economics Letters,* forthcoming.

Gutmann, J., Padovano, F., Voigt, S., 2015. Perception vs. experience: explaining differences in corruption measures using microdata. *SSRN Working Paper No.* 2659349.

Gründler, K., Krieger, T. 2016. Democracy and growth: Evidence from a machine learning indicator. *European Journal of Political Economy* 45, 85-107.

Gründler, K., Krieger, T. 2018. Should we care about data aggregation? Evidence from the democracy-growth nexus. *Working Paper*

Hirano, S., Snyder, JMJr. 2012. What happens to incumbents in scandals? *Quarterly Journal of Political Science* 7, 447-456.

Huber, PJ. 1967. The Behavior of Maximum Likelihood Estimates Under Nonstandard Conditions. In: Lucien M. Le Cam and Jerzy Neyman (Eds.), Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability, Volume 1: Statistics, pp. 221–233. Berkeley, CA: University of California Press.

Ivlevs, A., Hinks, T. 2018, Former communist party membership and bribery in the post-socialist countries. *Journal of Comparative Economics*, forthcoming.

Kauder, B., Potrafke, N. 2015, Just hire your spouse! Evidence from a political scandal in Bavaria, *European Journal of Political Economy* 38, 42-54.

Kaufmann, D., Kraay, A., Mastruzzi, M. 2010. The Worldwide Governance Indicators: Methodology and Analytical Issues. *Policy Research Working Paper Series 5430*, The World Bank.

Krause, S., Méndez, F. 2009. Corruption and elections: An empirical study for a cross-section of countries. *Economics and Politics* 21, 179-200.

Le Moglie, M., Turati, G. 2018. Electoral cycle bias in the media coverage of corruption news, *DISCE Working Paper no. 69.*

Méon, P.-G., Sekkat, K. 2005. Does corruption crease or sand the wheels of growth? *Public Choice* 122, 69-97.

Mironov, M., Zhuravaskaya, E., 2016. Corruption in procurement and the political cycle in tunneling: evidence from financial transactions data. *American Economic Journal: Economic Policy* 8, 287-321.

Nordhaus, WD. 1975. The political business cycle. *Review of Economic Studies* 42, 169-190.

Paldam, M. 2002. The cross-country pattern of corruption. Economics, culture and seesaw dynamics. *European Journal of Political Economy* 18, 215-240.

Potrafke N. 2010. The growth of public health expenditures in OECD countries: Do government ideology and electoral motives matter? *Journal of Health Economics* 29, 797–810.

Potrafke, N. 2012a. Political cycles and economic performance in OECD countries: empirical evidence from 1951–2006. *Public Choice* 150, 155–179.

Potrafke, N. 2012b. Intelligence and corruption. *Economics Letters* 114, 109-112.

Potrafke, N., Roesel, F., 2018. A banana republic? The effects of inconsistencies in the counting of votes on voting behavior. *Public Choice,* forthcoming.

Reischmann, M. 2016. Creative accounting and electoral motives: Evidence from OECD countries. *Journal of Comparative Economics* 44, 243-257.

Rogoff, KS. 1990. Equilibrium political budget cycles. *American Economic Review* 80, 21-36.

Rogoff, KS., Sibert, A. 1988. Elections and macroeconomic policy cycles. *Review of Economic Studies* 55, 1-16.

Rudolph, L., Däubler, T. 2016. Holding individual representatives accountable: the role of electoral systems. *Journal of Politics* 78, 746-762.

Schulze, G.G., Zakharov, F. 2018. Corruption in Russia – Historic legacy and systematic nature. *CESifo Working Paper No 6864*.

Sidorkin, O., Vorobyev, D. 2018. Political cycles and corruption in Russian regions. *European Journal of Political Economy* 52, 55-74.

Stock, JH., Watson, MW. 2008. Heteroskedasticity-robust standard errors for fixed effect panel data regression. *Econometrica* 76, 155–174.

The Guardian. 2013. Is Transparency International's measure of corruption still valid? Available at: https://www.theguardian.com/global-development/poverty-matters/2013/dec/03/transparency-international-measure-corruption-valid

Transparency International (2018). *Corruption Perception Index 2017*. Berlin.

Vadlamannati, KC. 2015. Fighting corruption or elections? The politics of anti-corruption policies in India: A subnational study. *Journal of Comparative Economics* 43, 1035-1052.

Vadlamannati, KC., Cooray, A. 2016. Transparency pays? Evaluating the effects of the freedom of information laws on perceived government corruption. *Journal of Development Studies* 53, 116-137.

White, H. 1980. A heteroskedasticity-consistent covariance matrix estimator and a direct test for heteroskedasticity. *Econometrica* 48, 817–838.

White, H. 1982. Maximum likelihood estimation of misspecified models. *Econometrica* 50, 1–25.

World Bank (2018). *World Bank national accounts data, and OECD National Accounts data files*. Washington DC.

Table 1: Descriptive statistics and data sources.

	Obs	Mean	Std. Dev.	Min	Max	Source
CPI (reversed)	470	49.66	19.31	8	76	Transparency International (2018)
Election	470	0.29	0.45	0	1	Own calculation
Election (regular)	470	0.26	0.44	0	1	Own calculation
Election (early)	470	0.04	0.20	0	1	Own calculation
Election (early) No corruption/scandal before election	470	0.03	0.18	0	1	Own calculation
Election (early) corruption/scandal before election	470	0.01	0.09	0	1	Own calculation
Election (Franzese, pre-election year)	470	0.18	0.33	0	1	Own calculation
Election (Franzese, election year)	470	0.14	0.27	0	0.97	Own calculation
Pre-election	470	0.29	0.46	0	1	Own calculation
Post-election	470	0.29	0.46	0	1	Own calculation
Election (parl.)	470	0.26	0.44	0	1	Own calculation
Election (pres.)	470	0.13	0.33	0	1	Own calculation
Per capita GDP (real)	465	18051.82	21626.48	368.94	108600.93	World Bank (2018)
Political Instability	470	0.18	0.80	-2.68	1.53	Kaufmann et al. (2010)
Government consumption expenditure (% of GDP)	443	17.02	6.46	5.94	82.21	World Bank (2018)

Table 2: Regression results. Baseline model.

Dependent variable: Perceived corruption (reversed CPI)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Election	0.138	0.163	0.387**	0.416**	0.432**	0.436**	0.468**	0.437**
	(1.147)	(1.157)	(0.187)	(0.183)	(0.185)	(0.185)	(0.194)	(0.218)
Per capita GDP (log)					-7.218*	-6.998*	-6.593	-8.890*
					(3.806)	(3.954)	(4.649)	(4.864)
Political Instability						-0.180	-0.117	-0.741
						(0.628)	(0.684)	(0.896)
Government consumption expenditure (% of GDP)							-0.086	-0.078
							(0.126)	(0.137)
Fixed country effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed year effects	No	Yes	No	Yes	Yes	Yes	Yes	Yes
Observations	470	470	470	470	465	465	443	358
Countries	94	94	94	94	93	93	90	73
R2 within			0.009	0.050	0.066	0.066	0.068	0.079
R2 between			0.000	0.000	0.683	0.688	0.694	0.697
R2 overall	0.000	0.000	0.000	0.000	0.678	0.682	0.690	0.694

Note: OLS with standard errors clustered at the country level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels.

The regressions in columns (1) and (2) include a constant.

Column (8) excludes countries with early elections.

Table 3: Regression results. Regular and early elections.

Dependent variable: Perceived corruption (reversed CPI)

	(1)	(2)	(3)	(4)	(5)
Election (regular)	0.318	0.325*	0.340*	0.343*	0.371*
	(0.201)	(0.193)	(0.194)	(0.193)	(0.203)
Election (early)	0.763	0.967*	1.010**	1.007**	1.024**
	(0.498)	(0.492)	(0.504)	(0.505)	(0.495)
Per capita GDP (log)			-7.334*	-7.169*	-6.750
			(3.786)	(3.919)	(4.606)
Political Instability				-0.135	-0.072
				(0.642)	(0.695)
Government consumption expenditure (% of GDP)					-0.084
					(0.125)
Fixed country effects	Yes	Yes	Yes	Yes	Yes
Fixed year effects	No	Yes	Yes	Yes	Yes
Observations	470	470	465	465	443
Countries	94	94	93	93	90
R2 within	0.011	0.054	0.070	0.070	0.073
R2 between	0.000	0.000	0.684	0.688	0.693
R2 overall	0.000	0.001	0.679	0.682	0.690

Note: OLS with standard errors clustered at the country level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 4: Regression results. Early elections called because of corruption and scandals.
Dependent variable: Perceived corruption (reversed CPI)

	(1)	(2)	(3)	(4)	(5)
Election (regular)	0.344*	0.348*	0.363*	0.368*	0.392*
	(0.199)	(0.193)	(0.194)	(0.193)	(0.203)
Election (early) No corruption/scandal before election	-0.095	0.168	0.205	0.191	0.244
	(0.325)	(0.342)	(0.358)	(0.361)	(0.374)
Election (early) corruption/scandal before election	3.773***	3.696***	3.761***	3.782***	3.668***
	(0.886)	(1.047)	(1.088)	(1.090)	(1.053)
Per capita GDP (log)			-7.410*	-7.115*	-6.591
			(3.802)	(3.920)	(4.596)
Political Instability				-0.242	-0.189
				(0.620)	(0.675)
Government consumption expenditure (% of GDP)					-0.070
					(0.129)
Fixed country effects	Yes	Yes	Yes	Yes	Yes
Fixed year effects	No	Yes	Yes	Yes	Yes
Observations	470	470	465	465	443
Countries	94	94	93	93	90
R2 within	0.035	0.075	0.091	0.091	0.093
R2 between	0.007	0.006	0.684	0.690	0.695
R2 overall	0.000	0.000	0.679	0.685	0.692

Note: OLS with standard errors clustered at the country level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table 5: Regression results. Timing of elections: Franzese's coding.
Dependent variable: Perceived corruption (reversed CPI)

	(1)	(2)	(3)	(4)	(5)	(6)
Election (Franzese, pre-election year)	-0.137	-0.036	0.003	-0.005	0.048	-0.158
	(0.309)	(0.312)	(0.308)	(0.312)	(0.308)	(0.377)
Election (Franzese, election year)	0.772**	0.845**	0.894**	0.895**	0.977***	0.810**
	(0.345)	(0.344)	(0.345)	(0.346)	(0.358)	(0.366)
Per capita GDP (log)			-7.351*	-7.153*	-6.729	-8.819*
			(3.767)	(3.916)	(4.579)	(4.810)
Political Instability				-0.160	-0.086	-0.736
				(0.623)	(0.672)	(0.915)
Government consumption expenditure (% of GDP)					-0.083	-0.074
					(0.123)	(0.134)
Fixed country effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed year effects	No	Yes	Yes	Yes	Yes	Yes
Observations	470	470	465	465	443	358
Countries	94	94	93	93	90	73
R2 within	0.013	0.054	0.070	0.070	0.074	0.081
R2 between	0.012	0.013	0.682	0.686	0.691	0.696
R2 overall	0.001	0.000	0.676	0.681	0.688	0.693

Note: OLS with standard errors clustered at the country level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels.
Column (6) excludes countries with early elections.

Table 6: Regression results. Timing of elections: Pre- and Post-election years.
Dependent variable: Perceived corruption (reversed CPI)

	(1)	(2)	(3)	(4)	(5)	(6)
Pre-Election	0.135	0.138	0.164	0.162	0.163	0.016
	(0.231)	(0.220)	(0.216)	(0.218)	(0.211)	(0.246)
Election	0.514**	0.550**	0.574**	0.583**	0.595**	0.518*
	(0.258)	(0.249)	(0.252)	(0.251)	(0.258)	(0.295)
Post-Election	0.237	0.255	0.252	0.265	0.209	0.218
	(0.233)	(0.233)	(0.234)	(0.231)	(0.243)	(0.298)
Per capita GDP (log)			-7.210*	-6.904*	-6.664	-8.802*
			(3.817)	(3.961)	(4.708)	(4.897)
Political Instability				-0.245	-0.161	-0.830
				(0.622)	(0.680)	(0.919)
Government consumption expenditure (% of GDP)					-0.087	-0.076
					(0.127)	(0.138)
Fixed country effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed year effects	No	Yes	Yes	Yes	Yes	Yes
Observations	470	470	465	465	443	358
Countries	94	94	93	93	90	73
R2 within	0.012	0.054	0.069	0.069	0.071	0.081
R2 between	0.003	0.003	0.683	0.690	0.695	0.698
R2 overall	0.000	0.000	0.678	0.684	0.692	0.695

Note: OLS with standard errors clustered at the country level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels.
Column (6) excludes countries with early elections.

Table 7: Regression results. Baseline. Parliamentary elections.

Dependent variable: Perceived corruption (reversed CPI)

	(1)	(2)	(3)	(4)	(5)	(6)
Election (parl.)	0.392*	0.417**	0.437**	0.439**	0.464**	0.411*
	(0.205)	(0.202)	(0.204)	(0.204)	(0.215)	(0.242)
Per capita GDP (log)			-7.252*	-7.054*	-6.541	-8.732*
			(3.821)	(3.975)	(4.656)	(4.888)
Political Instability				-0.163	-0.133	-0.334
				(0.627)	(0.685)	(0.889)
Government consumption expenditure (% of GDP)					-0.085	-0.075
					(0.126)	(0.135)
Fixed country effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed year effects	No	Yes	Yes	Yes	Yes	Yes
Observations	470	470	465	465	443	368
Countries	94	94	93	93	90	75
R2 within	0.009	0.050	0.066	0.066	0.068	0.070
R2 between	0.000	0.000	0.683	0.688	0.694	0.692
R2 overall	0.000	0.000	0.678	0.682	0.691	0.690

Note: OLS with standard errors clustered at the country level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Column (6) excludes countries with early elections.

Table 8: Regression results. Baseline. Presidential elections.

Dependent variable: Perceived corruption (reversed CPI)

	(1)	(2)	(3)	(4)	(5)	(6)
Election (pres.)	0.323	0.349	0.405	0.407	0.432	0.420
	(0.306)	(0.313)	(0.318)	(0.318)	(0.327)	(0.342)
Per capita GDP (log)			-7.342*	-7.180*	-6.715	-6.822
			(3.846)	(3.977)	(4.685)	(4.688)
Political Instability				-0.133	-0.053	-0.279
				(0.651)	(0.704)	(0.740)
Government consumption expenditure (% of GDP)					-0.082	-0.087
					(0.127)	(0.128)
Fixed country effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed year effects	No	Yes	Yes	Yes	Yes	Yes
Observations	470	470	465	465	443	433
Countries	94	94	93	93	90	88
R2 within	0.003	0.044	0.060	0.060	0.061	0.065
R2 between	0.067	0.067	0.683	0.686	0.691	0.695
R2 overall	0.009	0.002	0.678	0.681	0.688	0.692

Note: OLS with standard errors clustered at the country level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Column (6) excludes countries with early elections.

Table 9: Regression results. Baseline model.
Democracies as measured by Gründler and Krieger (2016).
Dependent variable: Perceived corruption (reversed CPI)

	(1)	(2)	(3)	(4)	(5)	(6)
Election	0.350**	0.385**	0.418**	0.430**	0.444**	0.367*
	(0.164)	(0.164)	(0.167)	(0.169)	(0.176)	(0.196)
Per capita GDP (log)			-6.801*	-5.910	-4.398	-7.462*
			(3.953)	(4.118)	(4.330)	(4.208)
Political Instability				-0.781	-0.634	-0.631
				(0.687)	(0.728)	(0.802)
Government consumption expenditure (% of GDP)					-0.102	-0.101
					(0.106)	(0.111)
Fixed country effects	Yes	Yes	Yes	Yes	Yes	Yes
Fixed year effects	No	Yes	Yes	Yes	Yes	Yes
Observations	570	570	558	558	535	432
Countries	114	114	112	112	109	88
R2 within	0.007	0.034	0.049	0.053	0.049	0.066
R2 between	0.001	0.001	0.638	0.668	0.684	0.686
R2 overall	0.000	0.000	0.642	0.670	0.687	0.683

Note: OLS with standard errors clustered at the country level in parentheses; ***, **, and * denote significance at the 1%, 5%, and 10% levels.
Column (6) excludes countries with early elections.

Appendix: Countries included - democracies as measured by Bjørnskov and Rode

Albania, Argentina, Armenia, Australia, Austria, Belgium, Benin, Bhutan, Bolivia, Botswana, Brazil, Bulgaria, Cabo Verde, Canada, Chile, Colombia, Comoros, Costa Rica, Croatia, Cyprus, Czech Republic, Denmark, Dominican Republic, Ecuador, El Salvador, Estonia, Finland, France, Georgia, Germany, Ghana, Greece, Guatemala, Guinea, Guyana, Honduras, Hungary, Iceland, India, Indonesia, Ireland, Israel, Italy, Jamaica, Japan, Kenya, Korea, Kyrgyzstan, Latvia, Lithuania, Luxembourg, Macedonia, Malawi, Malta, Malaysia, Mauritius, Mexico, Moldova, Mongolia, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Romania, Sao Tome and Principe, Senegal, Serbia, Sierra Leone, Slovakia, Slovenia, Spain, Sri Lanka, Suriname, Sweden, Switzerland, Taiwan, Timor-Leste, Trinidad and Tobago, Tunisia, Ukraine, United Kingdom, United States of America, Uruguay, Zambia

Additional democracies as measured by Gründler and Krieger

Afghanistan, Bosnia and Herzegovina, Cambodia, Democratic Republic of the Congo, Gabon, Haiti, Iraq, Kosovo, Lesotho, Liberia, Montenegro, Morocco, Myanmar, Namibia, Nepal, Russia, South Africa, Tanzania, Togo, Turkey, and Venezuela.
Nicaragua excluded as compared to Bjørnskov and Rode.