Electoral Cycles in Tax Reforms

Antonio C. David and Can Sever

2022
NOV

WORKING PAPER

IMF Working Paper
African Department

Electoral Cycles in Tax Reforms
Prepared by Antonio C. David and Can Sever*

Authorized for distribution by Amadou Sy
November 2022

> ***IMF Working Papers** describe research in progress by the author(s) and are published to elicit comments and to encourage debate.* The views expressed in IMF Working Papers are those of the author(s) and do not necessarily represent the views of the IMF, its Executive Board, or IMF management.

ABSTRACT: We examine electoral cycles in tax reforms using monthly data over the period of 1990-2018 for 22 advanced economies and emerging markets. We show that governments tend to avoid announcing tax reforms during the months running up to elections. In addition, they become more likely to announce those reforms in the first few months following elections, indicating that "political capital" plays a role in the timing of reforms. These patterns are broad-based regarding the changes in tax base and rate, and for various types of taxes. We also find that the pre-election decrease in the likelihood of tax reform announcements is stronger in emerging markets, and weaker in the countries with relatively better institutional quality. Finally, our results indicate that neither fiscal rules nor IMF programs appear to have differential effects on electoral cycles in tax reforms.

Electoral Cycles in Tax Reforms

Prepared by Antonio C. David and Can Sever[1]

Contents

TABLES

1. Introduction

In the post-pandemic context, following extensive fiscal stimuli and increases in public debt, tax reforms to boost revenue collection are on top of the mind of policy makers across the globe. Large fiscal responses to the Covid-19 shock helped countries mitigate the adverse effects of the pandemic, but elevated risks related to debt sustainability. Therefore, mobilizing revenues is again a policy priority and tax reforms are an essential part of this process. In this regard, it is important to understand potential challenges to tax policy design, as well as the factors affecting the timing of tax reforms.

Although the macroeconomic implications of tax reforms, including the effects of exogenous discretionary tax policy changes on output have been widely studied (e.g., Clausing 2007, Mertens and Ravn 2012 and 2013, Biswas et al. 2017, Gunter et al. 2021), less has been said about the determinants of the timing of such reforms, particularly in the realm of political economy. In this paper, we tackle this question empirically. In particular, we focus on a specific challenge that can affect the timing of tax policy reforms, namely political pressure of upcoming elections, by exploring whether election periods play a role in the timing of tax reforms. Using monthly data from 22 advanced economies (AEs) and emerging markets (EMs) over the period of 1990-2018, we show that tax reforms tend to exhibit electoral cycles. This suggests that political pressure of upcoming elections can create a challenge in designing tax policy, and likely poses an impediment to international tax policy coordination. The results also indicate that electoral cycles in tax reforms are stronger in EMs, but less pronounced in countries with stronger institutions. However, neither fiscal rules nor IMF lending arrangements appear to have much impact on the strength of electoral cycles in tax reforms.

In general, governments can try to manipulate economic policies in a way not to hurt their popularity before elections, if they believe that such actions will make them more likely get re-elected. This can create a cycle in economic policies characterized by expansionary patterns before elections, so-called electoral cycles. Regarding electoral cycles in fiscal policy in particular, the theory suggests that election-oriented governments are likely rely on expansionary fiscal policies before elections and postpone fiscal measures to the post-election periods. However, such policy manipulation works for governments only under some conditions: e.g., when voters have short memory, cannot distinctly observe the competence of incumbent politicians, are short-sighted, or are backward-looking etc. (Nordhaus 1975, Rogoff and Sibert 1988, Persson and Tabellini 1990, Rogoff 1990, Alesina et al. 1997). If such conditions are not met, i.e., if incumbent governments are not likely to reap the short-term political dividend of their pre-election policy choices, then they may not have many incentives to engage in policy manipulation before elections. Hence, whether governments engage in economic policy changes based on the timing of elections, whereby generating electoral cycles in economic policies, is ultimately an empirical question. Although previous papers have examined this phenomenon empirically with supportive evidence on expansionary fiscal policies before elections with a postponement of fiscal adjustment measures to the post-election period, the evidence is scarce regarding tax reforms.

We tackle this phenomenon empirically, and find that tax reforms tend to show electoral cycles. One reason why the studies on tax reforms have been scarce is the limitation of harmonized and comparable data on tax policy actions across countries over time. To overcome this issue, we adopt the Tax Policy Reform Database (TPRD) constructed by Amaglobeli et al. (2018). The database compiles information extracted from thousands of documents, including OECD surveys and the archives of the International Bureau of Fiscal Documentation (IBFD), by using text mining techniques. It provides information on the direction of tax policy

changes that is comparable across countries and over years. TPRD also provides the precise month of announcement of tax reforms.

This feature of TPRD is crucial for our analysis for several reasons. First, we use the announcement dates of reforms to avoid the issue of fiscal foresight (Leeper et al. 2013). Next, the use of high frequency data allows for the analysis of the short-term nature of electoral cycles, contrary to the vast majority of the literature focusing on electoral cycles in economic policies in a cross-country setting based on annual data. For this purpose, we first manually collect data on months of elections in 22 AEs and EMs. Together with this, TPRD allows us to employ the analysis of electoral cycles at monthly frequency by capturing the short-lived cycles more precisely without cancelling out pre- and post-election short-term effects (Akhmedov and Zhuravskaya 2004). Moreover, the use of monthly data improves the identification of electoral cycles by enabling us (i) to control for the influence of all economic and political factors on tax reforms at country level for a given year, and also (ii) to absorb the effects of common monthly shocks on reforms. Finally, our identification strategy relies on the fact that election dates are mostly predetermined relative to the announcement of tax reforms, i.e., it is unlikely that a tax reform announcement will lead to an election in the time window that we are considering in our analysis.[1]

We find that governments tend to avoid announcing tax reforms during the months running up to elections. The estimates suggest that probability of tax reform announcements decreases cumulatively by around 17 percentage points (pp) over the six-month window before elections, and becomes an additional 4.7 pp lower in the months of elections. The size of the impact is economically large, considering that the average monthly probability of tax reform announcements in the sample is 5.2 percent. The results also indicate that governments become more likely to announce tax reforms in the months following elections, but only within a short three-month period. This points to a window of opportunity for newly elected governments to leverage on their political capital to reform the tax system. These findings reveal the short-lived nature of electoral cycles in tax reforms emphasizing the importance of the use of high frequency data in identifying electoral cycles. The results also show that electoral cycles are broad-based regarding the changes in tax base and rate, and for various types of tax. The evidence on electoral cycles in tax reforms suggests that political pressures can create a challenge in designing the tax system. Beyond the domestic implications, electoral cycles in tax reforms possibly poses an impediment to cross-border coordination in tax policy.

This paper adds to the literature focusing on opportunistic political business cycles (PBCs) going back to the influential work of Nordhaus (1975). Early contributions in this literature examined the effect of elections on macroeconomic outcomes, such as economic growth and unemployment. However, given that macroeconomic outcomes are not distinctly under the control of governments, if those outcomes are influenced by electoral pressures, this impact should work through changes in economic policies. With this consideration, the literature on PBCs evolved over time to examine PBCs in economic policies rather than outcome variables.[2] The extant literature covers various policies, including electoral cycles in fiscal policy.[3] This strand of the literature mostly explored electoral cycles in government spending, budgets, debt, or fiscal deficits, with supporting evidence on the presence of electoral cycles in those policy variables (see, for instance Alesina et

---

[1] Announcement dates are also informative of the incentives of election-motivated politicians in the pre-election period, since they are likely to focus on their short-term popularity gains through such announcements rather than future implementation of tax policy changes which is more likely to take time and face additional obstacles compared to the announcement.

[2] See Dubois (2016) for a comprehensive review of the literature on PBCs.

[3] Some papers also focused on electoral cycles other policies, including monetary expansions, macroprudential policies, and capital controls (e.g., Aidt et al. 2020, Gavoille and Hofer 2021, Sever and Yücel 2022).

al. 1992, Drazen 2000, Schuknecht 2000, Brander and Drazen 2005, Shi and Svensson 2006, Ehrhart 2013, Alesina and Passalacqua 2016, and Lami and Imami 2019).

Nevertheless, less attention has been paid to tax reforms. Some studies explored electoral cycles in tax reforms at the local level focusing on various types of taxes (e.g., Foremny and Riedel 2014, Alesina and Paradisi 2017). One recent study by Fuest et al. (2021) examines electoral cycles in tax reforms in a cross-country setting.[4] The authors use annual data and show that governments tend to increase taxes in the post-election year, but they find that neither the election year nor the pre-election year have a pronounced effect on the timing of tax reforms in a sample of countries similar to ours.

One significant contribution of our analysis is the use of data at monthly frequency by manually collecting data on election dates. This helps us identify electoral cycles distinctly by examining the duration of electoral cycles and indeed capturing their short-lived nature. Higher frequency data also helps identification by allowing us to control for the effects of all other factors, or shocks, on the timing of reforms at country-year level, and also at month level. Possibly because of these factors, our results differ from some of the previous findings, as we conclude that governments indeed avoid tax reform announcements in the months running up to elections, as opposed to their findings. We also note that the present paper is one of the first studies exploring electoral cycles in general at monthly frequency in a cross-country setting, with a few exceptions such as Aidt et al. (2020) who examine electoral cycles in money expansion using monthly data.

The paper also adds to the literature on the drivers of tax reforms, by showing evidence of the importance of political economy determinants. Vegh and Vuletin (2015) explore the cyclicality of tax reforms and show that reforms covering various types of tax are acyclical in developed economies, whereas they are procyclical in developing economies. We extend this strand of the literature by showing that the timing of elections affects the timing of tax reforms, even after controlling for economic activity at a monthly frequency.

In addition, we make one step forward relative to the earlier literature, and explore other factors affecting the strength of electoral cycles in tax reforms. Our second main finding is that electoral cycles in tax reforms are weaker in countries with stronger institutions, pointing out to that although tax reforms are not immune to political influence, strong institutions can shield it from political pressure of upcoming elections.

Subsequently, we examine the role of economic development, fiscal rules, and IMF Programs on the strength of electoral cycles in tax reforms. We show that electoral cycles in tax reforms are more pronounced in EMs relative to AEs. The results also indicate that neither fiscal rules nor IMF lending arrangements appear to affect the strength of electoral cycles in tax reforms.[5]

The rest of this paper is organized as follows. Section 2 explains the data and illustrates the stylized facts. Section 3 introduces the methodology. Section 4 shows and discusses the results. Section 5 concludes.

---

[4] Gupta and Jalles (2020) also look at various other political determinants of tax reforms in a sample of developing economies using annual data, and find some evidence on the electoral cycles in tax reforms in those countries.

[5] However, as we discuss in detail later on, the latter finding should be treated with caution given the limited presence of IMF programs in the sample.

2. Data and Stylized Facts

2.1. Tax policy reforms

Data on tax policy reforms are obtained from the most recent version of TPRD. TPRD is a comprehensive database constructed by Amaglobeli et al. (2018), which provides rich information on tax policy changes, including direction, measure, type, dates for announcements and implementation, as well as their significance ("major" or "minor").[6] The information on these different aspects is based on more than 950 OECD Economic Surveys and 53,000 tax related news from the archives of IBFD extracted by text mining techniques. In particular, TPRD reports whether there is an increase or decrease in tax at a granular level, i.e., whether the policy change is related to tax base or rate; and the corresponding tax type, namely, corporate income tax (CIT), personal income tax (PIT), value-added and sale taxes (VAT), social security contributions (SSC), excise (EXE), and property tax (PRO).[7]

Crucially for the purposes of this paper, the database includes the exact date for the announcement and implementation of a specific tax change, including the month of the year. Therefore, we are able to explore the duration of electoral cycles in tax reforms at a monthly frequency, as opposed to the vast majority of the literature focusing on electoral cycles in a cross-country setting using annual data. There are several advantages of our higher frequency data approach. To begin with, to the extent that the direction of policy changes policies shifts after elections, lower frequency data hides electoral cycles since the pre- and post-election effects can cancel out (Akhmedov and Zhruavskaya 2004). In our analysis, thus, monthly data allows us to capture short-lived electoral cycles in tax reforms in a more clear-cut way. Next, the adoption of monthly data improves the identification on two ends. First, since we are able to absorb the effects of all economic and political variables on the likelihood of tax policy changes at the country level through the inclusion of country-year fixed effects in the estimation. Second, we control for the role of all global developments or shocks in tax changes at the monthly frequency.

The database covers 23 AEs and EMs going back decades. The countries are Australia, Austria, Brazil, Canada, China, Czechia, Denmark, France, Greece, Germany, India, Ireland, Italy, Japan, Korea, Luxembourg, Mexico, Poland, Portugal, Spain, Turkey, United Kingdom, and the United States. We restrict the analysis to the period of 1990-2018, since TPRD has the most comprehensive coverage during this time span with the tax reforms reported in the database being relatively sparse in earlier years (Amaglobeli et al. 2018, 2022). Hence, using this period allows us to have richer information on tax changes.

In line with the literature, we use announcement dates of reforms in our empirical specifications, rather than the implementation date, in order to mitigate the issues related to fiscal foresight (Amagloebeli et al 2018, Leeper et al. 2013). Specifically, agents can adjust their behavior before the implementation of tax changes, which would lead to biased estimates of their economic effects (Mertens and Ravn 2012). In addition, the announcement date is useful to uncover the behavior of election-motivated politicians in the pre-election period, since such promises of policy changes likely affect their popularity, and in turn, electoral success.

---

[6] It is developed by the Fiscal Affairs Department of the IMF in collaboration with the IBFD, and is available at tprdportal.org. Also, see Amaglobeli et al. (2018, 2022) for a detailed description of the database.

[7] A drawback of using TPRD is that the database only compiles information on the direction of tax policy changes, but not the magnitude.

In the main analysis, to account for "tax reforms", we employ data of tax increase announcements that are identified as "major" in line with the literature (Amaglobeli et al. 2018, Gechert and Grob 2019). The database identifies a policy change as major, for rate changes, if the change is larger than 1 pp; and for base changes, if the broadening in the tax base likely affects a large group or has large potential for mobilizing new resources. For the purpose of our study, a dummy variable for tax reforms is assigned 1 whenever a tax reform is announced in a given month, i.e., when at least one major tax increase announcement takes place during a month. In separate tests, we also assign corresponding dummy variables 1, if an announcement related to a specific tax measure (i.e., base, and rate) or type (i.e., CIT, PIT, VAT, SSC, EXE, and PRO) occurs in a month.

In the database, there are 3462 announcements of tax changes. 2260 of them took place during the period of 1990-2018. Out of those 2260 events, 953 of them are related to tax increases with 772 being identified as "major". Out of these 772 announcements, 707 events have information regarding the month of announcement (about 98 percent of which were eventually implemented). Therefore, in our monthly analysis, we are able to use the data from the vast majority of major tax increase announcements in the TRPD during the period of the analysis.

In robustness checks, we consider three alternative variables to identify tax reforms. First, instead of focusing on whether a reform is announced, we aim to capture the overall direction of all policy announcements in the tax system. For this purpose, we use the net change by summing up the number of increases and decreases in a month, and assign a value of 1 to the dummy for reform if the number of increases is higher than the number of decreases. Second, we also account for the increases in taxes that are classified as minor in TPRD, instead of focusing exclusively on "major" increases, in order to avoid any possibility that the specific definition of the importance of tax changes in TPRD may drive the results. Finally, we construct a variable with the number of tax reform announcements during a month, instead of using the dummy variable approach.

2.2. Elections

We manually collect information on the months of elections for 22 countries over the period of 1990-2018 from online sources.[8] In this context, it is important to use some judgement to identify elections that are more likely to affect policy decisions based on the selection of chief executives. For instance, in presidential systems such the US, where the president has greater power, and incentives, to influence policies with re-election concerns, we use presidential elections. For parliamentary systems, such as Germany, we adopt the months of parliamentary elections. Whenever available and applicable, we also cross-check our data with the well-known datasets, such as Comparative Political Data Set (CPDS) by Armingeon et al. (2022). Table A.1 in the Appendix provides the dates of elections in over the sample period.

2.3. Other variables

In addition, we investigate the role of several country-specific factors in electoral cycles in tax reforms. To start with, we examine whether institutional quality makes a difference in electoral cycles in tax reforms. For this purpose, we adopt three widely-used proxies for the quality of institutions in a country. First, we use (i) the law and order and (ii) the bureaucratic quality indexes from the International Country Risk Guide (ICRG)

---

[8] We exclude China from the analysis due to the peculiarities of its electoral system.

database by the Political Risk Services Group. The data has the advantage of being available at the monthly frequency. It is constructed based on the country experts' assessment. The index on law and order consists of information on the strength of the legal system and an assessment of popular observance of the law, ranging between 0 and 6. The bureaucratic quality index evaluates whether the bureaucracy tends to be autonomous from political pressure and to have independent mechanisms. The index is between 0 and 4. As the third proxy for institutional quality, we use the index on civil liberties from the Freedom House database. It compiles information from several standardized questions in the areas of freedom of expression and belief, associational and organizational rights, rule of law, and personal autonomy and individual rights, and is available at annual frequency. The score ranges between 0 and 7. Higher values of those indexes represent stronger institutions.[9]

Next, we explore the role of political system in electoral cycles. We obtain the information on political system from the Database for Political Institutions (DPI) 2020, as provided by Cruz et al. (2021). We construct a dummy variable which is assigned 1 whenever the political system in a country is presidential, and 0 otherwise. The information is available at the annual frequency.

Moreover, we explore whether ongoing IMF-supported programs or the presence of fiscal rules affect the strength of electoral cycles in tax reforms. We manually collect monthly data on the dates of IMF programs form the IMF's website. A dummy variable is assigned 1 for the months during which country has a lending arrangement with the IMF, and 0 otherwise. Regarding fiscal rules, we adopt annual data from the database constructed by Davoodi et al. (2022). A dummy variable is defined to represent the years during which a country has some sort of fiscal rule (i.e., covering expenditure, revenue, budget, or debt).

Finally, we check whether electoral cycles in tax reforms stay similar when we control for various economic variables. As a proxy for economic activity, we obtain monthly data on the industrial production index from the IMF's International Financial Statistics (IFS) database. We also adopt the monthly data on inflation based on consumer prices (year-on-year basis) from the same database.

2.4. Sample

The sample consists of monthly data from 22 AEs and EMs. over the period of 1990-2018. The number of country-month observations in the analysis is 7392. The lefthand side chart in Figure 1 documents the number of tax reform announcements and elections each year in the sample. The total number of months with reform announcements in the sample is 381, making the overall probability of reforms 5.2 percent. The number of elections is 170. The righthand side chart in Figure 1 documents the total number of country-month observations with tax reform announcements for each country over the period of 1990-2018. We observe that the total number of months with reform announcements over the sample period varies across countries. We employ various robustness checks to make sure that a specific country, or a few countries, with relatively low or high number of events do not drive the results.

---

[9] In the original Freedom House data, higher values indicate lower institutional quality. For consistency, we use the inverse of it (7 minus the index) so that higher values represent greater institutional quality in that case as well.

Figure 1: The number of tax reforms and elections

Notes: The lefthand side figure reports the number of country-month observations with tax reform announcements (bars) and elections (line) during the years in the sample. The righthand side figure documents the number of country-month observations with tax reform announcements for each country.

2.5. Stylized Facts

Before introducing the empirical strategy, we explore whether there is a systematic relationship between the likelihood of tax reform announcements and the timing of elections. Figure 2 documents the findings. On the lefthand side chart, the dashed line illustrates the overall probability of tax reform announcements, which is the ratio is the total number of announcements to the number of observations in the sample. It is 5.2 percent, as mentioned before. The first bar is the average monthly probability of reform announcements during the 6-month period running up to elections, which is about 2.5 percent. The second bar shows that the likelihood of reform announcements becomes 2.4 percent in the months of elections. Finally, the third bar represents the average monthly probability during the 6-month time span in the aftermath of elections, which turns out to be 8.3 percent.

The righthand side chart in Figure 2 takes a closer look by focusing on the monthly probability of reform announcements around elections rather than averaging the probabilities over 6-month spans. It shows that the likelihood of tax reform announcements during the 6 months running up to elections, as well as the probability in the election months, stays below the average probability in the sample. However, this probability becomes much larger in the post-election months, particularly during the third month in those periods.

These findings suggest that tax reforms tend to exhibit electoral cycles: Governments seem to become less likely to announce tax reforms before and during the elections, whereas they tend to do so following elections. Motivated by these observations, the next section introduces the regression analysis to examine electoral cycles in tax reforms in a formal setup.

Figure 2: Probability of tax reforms around elections (percent)

Notes: The figure documents the probability of tax reform announcements in the sample during the months around elections in percent. The dashed lines are the overall probability of announcements in the sample. On the lefthand side chart, the first and third bars report the average monthly probability for 6-month periods before and after elections, respectively. The second bar documents the probability of reform announcements in the election months. The righthand side chart instead reports the probability for each month, rather than averaging over 6-month windows.

3. Empirical Strategy

Our main specification is a panel linear probability model with fixed effects, and as follows:

$$Prob\big(Reform_{c,t}\big) = \mu + \sum_{p=1}^{6} \alpha_p \, Election_{c,t+p} \; + \; \alpha_0 Election_{c,t} \; + \; \sum_{r=1}^{6} \alpha_{-r} \, Election_{c,t-r} \; + \; \theta_{c,y} \; + \; \theta_t \; + \; e_{c,t} \qquad (1)$$

where $Prob\big(Reform_{c,t}\big)$ is the probability of tax reforms, which takes the value of one during the months of such announcements, and 0 otherwise. $Election_{c,t}$ is a dummy variable which takes 1 during the months of elections. The set of $Election_{c,t+p}$ represents the forward values of the election dummy. Therefore, the coefficient estimates α_p for $p = 1, \dots, 6$ capture the change in the likelihood of tax reforms during the 6-month period before elections. The coefficient estimate α_0 represents the effect of the election month on this probability. Finally, we also aim to observe if the effect of elections on the likelihood of reforms is specific to pre-election months, and whether government tend to behave differently following elections. Therefore, the specification in equation 1 controls for the post-election impact as well by controlling for the lagged value of the election dummy ($Election_{c,t-r}$). The coefficient estimates α_{-r} for $r = 1, \dots, 6$ capture the change in the probability for the 6-month time span in the aftermath of elections.

We include two sets of fixed effects in the specification which are crucial for identification. The effects of all economic, political, and other unobservable factors at the country-year level on the likelihood of tax increase announcements are absorbed by the inclusion of country-year fixed effects ($\theta_{c,y}$). In addition, month fixed effects (θ_t) soak the impact of monthly developments, or shocks, that are common across countries on the likelihood of tax reform announcements. Standard errors are clustered at the country-year level.

As previously discussed, the use of monthly data improves the identification of electoral cycles markedly. First, contrary to analysis using annual data, it captures short-lived electoral cycles without cancelling out the potential changes in the direction of policies before and after elections, allowing us to explore the duration of electoral cycles distinctly (Akhmedov and Zhuravskaya 2004). This feature turns out to be important, as we discuss in more detail in the results. Second, it enables us to absorb the effects of two sets of factors on electoral cycles by the inclusion of country-year and month fixed effects, as mentioned above, which mitigates omitted variable bias. Finally, our identification strategy relies on the assumption that it is unlikely that a tax policy change announcement would lead to elections in a few months.[10]

We expect α_p and α_0 to be negative to the extent that popularity-concerned governments tend to avoid tax increases before and during elections, respectively. We also expect α_{-r} to be nonnegative, or even positive, if a lower probability of tax increase announcements is indeed driven by re-election concerns.

In the second step, we examine if various country-specific factors can influence the strength of the pre-election effect on tax reforms. For this purpose, we include the interaction between several country-specific factors ($X_{c,t}$) and the pre-election variables, as well as the variable itself to avoid omitted variables bias:

$$Prob\big(Reform_{c,t}\big) = \mu + \sum_{p=1}^{6} \alpha_p\, Election_{c,t+p} \; + \; \alpha_0 Election_{c,t} \; + \; X_{c,t} \sum_{p=1}^{6} \beta_p\, Election_{c,t+p}$$

$$+ \beta_0\, X_{c,t}\, Election_{c,t} + \beta X_{c,t} + \sum_{r=1}^{6} \alpha_{-r}\, Election_{c,t-r} \; + \; \theta_{c,y} \; + \; \theta_t \; + \; e_{c,t} \qquad (2)$$

In this specification, the coefficient estimates of the interaction terms (i.e., β_p for $p = 1, \; ..., 6$ and β_0) capture the role of those factors in the strength of pre-election policy manipulation by governments, whereas β gauges their direct effect on the likelihood of tax reforms.

4. Results

4.1. Main results

Table 1 illustrates the findings on the likelihood of an increase in tax around election periods based on the linear probability model in equation 1. In column 1, we only include the forward value of the election dummy for 6-month period. Column 2 adds the months of elections. Finally, column 3 controls for the lagged values of the election dummy as well.

Figure 3 represents the coefficient estimates (in pp) and the 90 percent confidence intervals based on the results represented in column 3 in Table 1. It shows that the probability of a tax reform announcement declines during the months running up to elections. For instance, the coefficient of $Election_{t+6}$ suggests that 6 months before elections the likelihood of a tax reform announcement decreases by 4.7 pp. During the 6-month window before elections, we observe similar and statistically significant effect on the probability of tax reforms

---

[10] It is worthwhile to note that the vast majority of election dates in our sample is pre-determined, and also the results are robust if we drop a few early elections from the sample, as noted later on.

for the majority of months. Focusing on the statistically significant coefficient estimates, the cumulative decline in the probability of reform announcements during the 6-month period before the election becomes 16.9 pp (corresponding to a 2.8 pp lower probability per month over that period on average). In addition, election months also turn-out to be important for electoral cycles: The probability also declines by 4.7 pp in the months of elections. These effects are economically large, considering that the average monthly probability of tax reform announcements in the sample is 5.2 percent.

Table 1: Main results

Variable	(1)	(2)	(3)
$Election_{t+6}$	-0.046***	-0.050***	-0.047***
	(0.011)	(0.011)	(0.012)
$Election_{t+5}$	-0.017	-0.022	-0.015
	(0.018)	(0.019)	(0.019)
$Election_{t+4}$	-0.037***	-0.043***	-0.036**
	(0.013)	(0.014)	(0.015)
$Election_{t+3}$	-0.035***	-0.042***	-0.038***
	(0.014)	(0.014)	(0.014)
$Election_{t+2}$	-0.010	-0.016	-0.006
	(0.019)	(0.020)	(0.021)
$Election_{t+1}$	-0.052***	-0.059***	-0.047***
	(0.013)	(0.014)	(0.015)
$Election_t$		-0.051***	-0.047***
		(0.016)	(0.016)
$Election_{t-1}$			-0.009
			(0.021)
$Election_{t-2}$			0.028
			(0.024)
$Election_{t-3}$			0.087***
			(0.029)
$Election_{t-4}$			0.018
			(0.022)
$Election_{t-5}$			0.025
			(0.024)
$Election_{t-6}$			-0.025
			(0.018)
Country-year and month F.E.	Yes	Yes	Yes
R-squared	0.138	0.139	0.143
Observations	7524	7524	7392

Notes: The results are based on equation 1. The dependent variable is a dummy which takes one for the months in which there is a tax increase announcement, and 0 otherwise. The election variable takes 1 for the months of elections, and 0 otherwise. Standard errors in parentheses are clustered at the country-year level. *** p<0.01, ** p<0.05, * p<0.1.

Looking at the coefficient estimates for the lagged values of the election dummy, such dampening effect is specific to the periods before and during the elections. During the months following elections, it seems to be the case that governments become more likely to announce increases in taxes, this effect being particularly pronounced for the third month in the aftermath of elections. This suggests that there is a window of opportunity after elections, during which governments have sufficient "political capital" to announce reforms, even if they entail short-term pain. This is also consistent with the "political opportunity" explanation which suggests that politicians consider tax increases when electoral risks are minimal, e.g., when the next election is as far away as possible, since their unpopular actions are discounted by the electorate over time (Fair 1978,

Berry and Berry 1992, 1994). Nevertheless, it seems that the window is typically perceived to be quite narrow, with the positive effect being statistically significant only at the third month in the aftermath of elections.

Considering the shift in the direction of tax policy reforms before and after elections, and the short-lived nature of the post-election window of opportunity, it appears that the use of high frequency data in the examination of electoral cycles in tax reforms is important, as previously mentioned. We conclude that tax reforms tend to exhibit electoral cycles. While governments concerned about re-election tend to avoid tax reform announcements during the periods running up to elections, they become more likely to take such actions in the post-election months, particularly within a short time window.

We also explore whether the findings remain similar if the window of the analysis is extended to 12-months before and after elections. Figure 4 reports the results and illustrates that electoral cycles in tax reforms stay similar to the baseline specification. Governments tend to avoid tax reforms during months in the year before elections, while this pattern disappears during the 12 months following elections. The short duration of the window of opportunity for government to reform the tax system in the post-election period is also confirmed. For these reasons, in the remainder of the paper we present the analysis using 6-month windows.

Figure 3: Change in the probability of tax reforms (in pp) around elections

Notes: The figure documents the results based on equation 1. The figure illustrates the coefficient estimates (bars, in percentage points) and the 90 percent confidence intervals (vertical lines) based on equation 1, as shown in the third column in Table 1.

Figure 4: Change in the probability of tax reforms (in pp) around elections, 12-month windows

Notes: The figure documents the results based on equation 1 spanning 12-month windows. The figure illustrates the coefficient estimates (bars, in percentage points) and the 90 percent confidence intervals (vertical lines).

4.2. Some robustness checks

In this section, we investigate the robustness of the previous findings. Table 2 illustrates the results. We first focus on the dependent variable. In column 1, we adopt the net changes in tax policy in a country during a month, by summing up the directions of all announcements within each month . If the number of increases is larger than that of decreases, we assign the dummy variable for tax reforms 1, and 0 otherwise. In column 2, instead of the dummy variable approach, we use the number of tax increase announcements in a month as the dependent variable. In the third column, we assign the dummy variable for tax reforms by accounting for announcements for minor tax increases as identified by the database, as well.

Next, we focus on an alternative econometric model. Column 4 runs Probit model instead of linear probability model to address potential concerns about using a linear model with the binary outcomes in the dependent variable. We apply analytical bias correction for the well-known incidental parameter problem for Probit models with fixed effects (Lancaster 2000, Cruz-Gonzalez et al 2017).

Finally, we consider specifications that add several control variables. Columns 5 and 6 control for the lagged values of tax increase and decrease announcements, respectively. These robustness checks are important to capture the fact that the recent history of tax policy actions may affect the government's decision. For example, if the government already announced reforms in the tax system in the past few months, they will obviously have less of an incentive and room to do so again. Alternatively, if they announced decreases in taxes recently, a reversal of that action may signal the voters its incompetency, and therefore the governments may avoid reversals within such a short period. Column 7 controls for the lagged values of the percentage of the countries in the same region that announced reforms in the tax system, (excluding the country itself) to account for spill-overs or "imitation effects" on domestic tax policy changes. Column 8 includes lagged values of the industrial production index as a proxy for economic activity. The inclusion of this variable can be important to check, since economic conditions are found to influence the likelihood of tax reforms in some

countries (e.g., Vegh and Vuletin 2015). Finally, the last column controls for inflation, since it may affect fiscal policy choices in general. The results across those tests remain similar to the previous findings pointing to electoral cycles in tax reforms.

Table 2: Robustness

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Election_{t+6}$	-0.028***	-0.049***	-0.056***	-0.147***	-0.056***	-0.049***	-0.047***	-0.052***	-0.047***
	(0.010)	(0.017)	(0.013)	(0.021)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
$Election_{t+5}$	-0.007	-0.047*	-0.013	-0.027	-0.029	-0.018	-0.015	-0.030*	-0.017
	(0.015)	(0.025)	(0.021)	(0.056)	(0.020)	(0.019)	(0.019)	(0.018)	(0.019)
$Election_{t+4}$	-0.021*	-0.048*	-0.049***	-0.113***	-0.048***	-0.038**	-0.036**	-0.036**	-0.035**
	(0.012)	(0.025)	(0.016)	(0.034)	(0.016)	(0.015)	(0.015)	(0.016)	(0.016)
$Election_{t+3}$	-0.024**	-0.044	-0.049***	-0.118***	-0.051***	-0.039***	-0.038***	-0.039**	-0.036**
	(0.012)	(0.027)	(0.015)	(0.032)	(0.015)	(0.014)	(0.014)	(0.015)	(0.015)
$Election_{t+2}$	-0.010	-0.043	-0.015	-0.010	-0.021	-0.007	-0.005	-0.014	-0.014
	(0.016)	(0.032)	(0.021)	(0.057)	(0.022)	(0.021)	(0.021)	(0.021)	(0.020)
$Election_{t+1}$	-0.027**	-0.082***	-0.056***	-0.134***	-0.060***	-0.049***	-0.047***	-0.043***	-0.043***
	(0.011)	(0.024)	(0.016)	(0.026)	(0.017)	(0.015)	(0.015)	(0.016)	(0.016)
$Election_t$	-0.030**	-0.080***	-0.054***	-0.132***	-0.060***	-0.049***	-0.046***	-0.042**	-0.045***
	(0.013)	(0.026)	(0.018)	(0.025)	(0.017)	(0.016)	(0.016)	(0.017)	(0.017)
$Election_{t-1}$	-0.004	-0.022	-0.013	-0.023	-0.024	-0.012	-0.008	-0.005	-0.004
	(0.017)	(0.032)	(0.022)	(0.052)	(0.022)	(0.021)	(0.021)	(0.022)	(0.022)
$Election_{t-2}$	0.020	0.030	0.017	0.096	0.016	0.023	0.029	0.035	0.034
	(0.020)	(0.042)	(0.025)	(0.070)	(0.025)	(0.025)	(0.024)	(0.026)	(0.026)
$Election_{t-3}$	0.059**	0.250**	0.094***	0.236***	0.081***	0.083***	0.087***	0.095***	0.097***
	(0.024)	(0.100)	(0.030)	(0.074)	(0.029)	(0.028)	(0.029)	(0.030)	(0.030)
$Election_{t-4}$	0.020	0.076	0.004	0.042	0.022	0.018	0.018	0.022	0.024
	(0.019)	(0.065)	(0.022)	(0.058)	(0.022)	(0.022)	(0.022)	(0.023)	(0.023)
$Election_{t-5}$	0.015	0.040	0.024	0.062	0.032	0.026	0.025	0.005	0.017
	(0.020)	(0.052)	(0.026)	(0.056)	(0.025)	(0.024)	(0.024)	(0.023)	(0.024)
$Election_{t-6}$	-0.014	-0.041	-0.030	-0.069	-0.016	-0.026	-0.029	-0.023	-0.026
	(0.016)	(0.029)	(0.020)	(0.042)	(0.018)	(0.018)	(0.018)	(0.019)	(0.020)
X_{t-1}					-0.108***	-0.035***	0.001*	-0.001*	-0.000
					(0.014)	(0.013)	(0.000)	(0.001)	(0.000)
X_{t-2}					-0.107***	-0.048***	0.001*	-0.000	0.000
					(0.015)	(0.012)	(0.000)	(0.001)	(0.000)
X_{t-3}					-0.105***	-0.033***	0.001*	0.000	0.000
					(0.014)	(0.012)	(0.000)	(0.000)	(0.000)
X_{t-4}					-0.073***	-0.017	0.000	-0.001	0.000
					(0.015)	(0.011)	(0.000)	(0.001)	(0.000)
X_{t-5}					-0.094***	-0.055***	0.000	0.001*	-0.000
					(0.012)	(0.009)	(0.000)	(0.000)	(0.000)
X_{t-6}					-0.074***	-0.034***	0.001	-0.000	-0.000
					(0.014)	(0.011)	(0.000)	(0.000)	(0.000)
Country-year and month F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.141	0.140	0.150	0.125	0.173	0.150	0.144	0.148	0.148
Observations	7392	7392	7392	2294	7392	7392	7392	6936	7032

Notes: The results are based on equation 1. Column 1 runs the analysis using the netted increases in tax for a given month as the dependent variable. Column 2 adopts the number of tax increases as the dependent variable. Column 3 constructs the dependent variable by also accounting for minor tax increases. Column 4 runs Probit model. Columns 5 and 6 control for the lagged values of tax increases and decreases, respectively. Column 7 includes the lagged values of the percentage of countries in the same region (based on three regions, i.e., Europe, Asia, and Americas) that reformed the tax system, excluding the country itself. Columns 8 and 9 add the lagged values of the industrial production index and inflation, respectively. Standard errors in parentheses are clustered at the country-year level. *** p<0.01, ** p<0.05, * p<0.1.

It is worthwhile to note that the findings are also robust to (i) dropping one country at a time and running the regression for the remaining 21 countries to confirm that a specific country is not driving the findings; (ii) excluding the countries that are above 75th or below 25th percentile of the sample in terms of the total number of reforms (as shown in Figure 1) to make sure that countries with fewer or many reforms do not drive the results; (iii) doing the analysis for various periods such as before or after the Global Financial Crisis of 2008, dropping first few years from the sample, or excluding 2018 which has relatively low number of reforms (as illustrated by Figure 1); (iv) different levels of clustering, or when standard errors are not clustered; (v) different combinations, or absence, of fixed effects; (vi) excluding a few snap elections from the sample to address any concerns about reverse causality; (vii) weighted regressions where the weights are GDP, or GDP per capita, averaged over the sample period, or inverse of those variables (to make sure that a few large/small, or rich/poor, economies do not drive the pattern); and (viii) including all the control variables in Table 2 (in columns 5-9) together. Those results are available upon request.
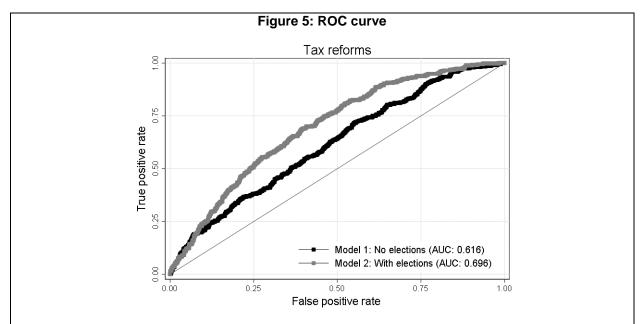
4.3. Explanatory power

In this section, we evaluate the predictive power of our empirical model based on the receiver operating characteristic (ROC) curve, which is a standard tool used for the analyses with binary classification with the Probit model. The ROC curve is a representation of whether a model is able to successfully identify positive cases (reform announcements in our context) and also not to identify negative cases (no reform announcements) across all signal levels.

In Figure 5, the x-axis is the false positive rate, indicating how often there is no tax reform announcement when the model predicts an event. The y-axis represents the true positive rate, showing how often the model predicts announcement when there exists a reform announcement in the data. For example, a point in Figure 5 with true positive rate 0.75 and false positive rate 0.25 represents a threshold which predicts an announcement when there is indeed an announcement around 75 percent of the time, also predicts an announcement when there is no announcement around 25 percent of the time. Hence, a ROC curve that is closer to the upper left corner reflects a greater goodness of fit for the empirical model. Numerically, the predictive power of the model is captured by the area under the curve (AUC). When it is 0.5, the model is uninformative, meaning that it is equivalent to tossing a coin when predicting an event (represented by the 45-degree line in Figure 5). Therefore, a model which is informative needs to lie above the 45-degree line with an AUC value larger than 0.5. A model which perfectly predicts tax reform announcements episodes would have an AUC of 1.

The gray line in Figure 5 represents the goodness of fit based on the Probit model as shown in the fourth column in Table 2 (the full model with the election dummies and fixed effects), whereas the black line shows that goodness of fit when we run a Probit model by excluding the election variables but only keeping the two sets of fixed effects. The figure shows that the AUC value of the model with fixed effects but without election dummies is 0.616 with a standard error of 0.015. Being statistically different than 0.5 at the 1 percent significance level, it suggests that fixed effects (by controlling for country-year characteristics and month shocks that are common across countries) add some predictive power for tax reforms, with the model performing significantly better than tossing a coin.

When we add the election dummies in line with equation 1, however, the AUC value increases to 0.696 with a standard error of 0.014. This means that the AUC value of the full model it is statistically different

(i) from 0.5, and also (ii) from the model which excludes election dummies at the 1 percent significance level. This indicates that the inclusion of election dummies increases the predictive power of the model significantly. We conclude that although they are still far from being a perfect predictor, electoral cycles are well informative of tax reform announcements in the data.

Figure 5: ROC curve

Notes: The figure illustrates the ROC curve of the models without and with election variables. Grey line represents the goodness of fit based on the Probit model as shown in the fourth column in Table 2. Black line represents the goodness of fit from the Probit model that excludes the election dummies but keeps the two sets of fixed effects. AUC is the area under the curve, ranging between 0 and 1.

4.4. The role of other political and economic factors

In this section, we investigate the role of various country level political and economic factors in the electoral cycles in tax reforms. For this purpose, we include the interaction between those variables and the election dummies for the pre-election period and the election month, as with the specification in equation 2.[11]

Table 3 shows the results. We first examine if institutional quality affects the strength of electoral cycles. In columns 1-2, we adopt the indexes on (i) law and order and (ii) bureaucratic quality form the ICRG database as proxies for institutional quality. In column 3, we instead use the civil liberties index from the Freedom House database. The findings illustrate that the pre-election effect on the probability of tax reforms is weaker in countries with relatively high institutional quality. This suggests that when there are more institutional restrictions on politicians, it becomes harder for them to influence policies in favor of their re-election goal. This result implies that although tax reforms are not immune to political influence from elections, strong institutions can shield them from political interference. It is also consistent with several studies that show evidence that institutions matter for electoral cycles in other economic policies (e.g., Sever and Yücel 2022).

---

[11] Whenever the country level variable ($X_{c,t}$ in Table 3) is available at the annual frequency (as described in Section 2), it is dropped from the regression due to multicollinearity (arising from the inclusion of country-year fixed effects). Otherwise, we add the variable $X_{c,t}$ as well, as mentioned above.

Table 3: The role of other factors

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Election_{t+6}$	-0.103**	-0.103**	-0.091*	-0.039***	-0.041***	-0.060***	-0.044***
	(0.044)	(0.051)	(0.048)	(0.014)	(0.014)	(0.022)	(0.012)
$Election_{t+5}$	-0.030	0.041	0.027	-0.017	-0.011	0.011	-0.024
	(0.100)	(0.013)	(0.015)	(0.020)	(0.023)	(0.051)	(0.018)
$Election_{t+4}$	-0.124**	-0.152**	-0.180***	-0.020	-0.038**	-0.062***	-0.034**
	(0.051)	(0.062)	(0.057)	(0.018)	(0.016)	(0.023)	(0.016)
$Election_{t+3}$	-0.015	-0.074	-0.141**	-0.034**	-0.041**	-0.056**	-0.044***
	(0.073)	(0.087)	(0.062)	(0.016)	(0.016)	(0.023)	(0.014)
$Election_{t+2}$	-0.094	-0.151	-0.180**	0.004	0.001	0.004	-0.017
	(0.107)	(0.113)	(0.085)	(0.003)	(0.024)	(0.043)	(0.020)
$Election_{t+1}$	-0.150***	-0.203***	-0.136**	-0.039**	-0.048***	-0.065***	-0.047***
	(0.053)	(0.063)	(0.057)	(0.018)	(0.016)	(0.024)	(0.016)
$Election_t$	-0.145***	-0.214***	-0.151**	-0.036**	-0.045***	-0.076***	-0.045***
	(0.047)	(0.062)	(0.061)	(0.017)	(0.017)	(0.024)	(0.017)
$Election_{t-1}$	-0.009	-0.009	-0.008	-0.010	-0.009	-0.009	-0.009
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.022)	(0.021)
$Election_{t-2}$	0.029	0.028	0.029	0.027	0.028	0.026	0.029
	(0.024)	(0.025)	(0.025)	(0.024)	(0.024)	(0.025)	(0.024)
$Election_{t-3}$	0.088***	0.088***	0.088***	0.087***	0.087***	0.086***	0.087***
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
$Election_{t-4}$	0.020	0.020	0.019	0.019	0.019	0.019	0.019
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)	(0.022)
$Election_{t-5}$	0.019	0.019	0.019	0.026	0.025	0.027	0.025
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.019)	(0.024)
$Election_{t-6}$	-0.029	-0.028	-0.030	-0.028	-0.028	-0.027	-0.028
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
X_t	-0.000	-0.035					-0.011
	(0.017)	(0.057)					(0.037)
$Election_{t+6} \times X_t$	0.012	0.017	0.008	-0.039	0.027	0.017	-0.029
	(0.009)	(0.017)	(0.008)	(0.025)	(0.025)	(0.026)	(0.058)
$Election_{t+5} \times X_t$	0.003	-0.016	-0.008	0.004	-0.023	-0.035	0.153
	(0.020)	(0.038)	(0.026)	(0.006)	(0.039)	(0.055)	(0.155)
$Election_{t+4} \times X_t$	0.018*	0.035*	0.027**	-0.075***	0.009	0.026	-0.027
	(0.010)	(0.019)	(0.011)	(0.028)	(0.040)	(0.027)	(0.048)
$Election_{t+3} \times X_t$	-0.005	0.011	0.020*	-0.022	0.015	0.024	0.116
	(0.015)	(0.025)	(0.011)	(0.037)	(0.036)	(0.029)	(0.110)
$Election_{t+2} \times X_t$	0.018	0.043	0.033**	-0.049	-0.034	-0.009	0.184
	(0.021)	(0.033)	(0.017)	(0.044)	(0.045)	(0.048)	(0.135)
$Election_{t+1} \times X_t$	0.021*	0.046**	0.017	-0.042	0.004	0.028	-0.000
	(0.011)	(0.020)	(0.011)	(0.028)	(0.004)	(0.028)	(0.054)
$Election_t \times X_t$	0.020**	0.049**	0.020*	-0.052*	-0.008	0.041	-0.023
	(0.010)	(0.020)	(0.011)	(0.028)	(0.040)	(0.029)	(0.057)
Country-year and month F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.144	0.145	0.144	0.144	0.143	0.147	0.145
Observations	7362	7362	7362	7392	7392	7056	7392

Notes: The results are based on equation 2. Columns 1-3 investigate the role of institutional quality in electoral cycles in tax reforms using the indexes on (i) law and order, (ii) bureaucratic quality, and (iii) civil liberties as proxies for the quality of institutions, respectively. Columns 4 and 5 explore the differential effect of elections on tax reforms in the case of EMs and presidential systems, respectively. Columns 6 and 7 examines the differential effect of elections under the presence of fiscal rules and IMF programs, respectively. Standard errors in parentheses are clustered at the country-year level. *** p<0.01, ** p<0.05, * p<0.1.

In column 4, we investigate whether electoral cycles in tax reforms are stronger in EMs relative to AEs in the sample.[12] The coefficient estimates of the interaction between a dummy variable representing EMs and pre-election months indicate that electoral cycles are somewhat more pronounced in EMs. This is consistent with a few studies, which finds that electoral cycles are generally stronger in countries with relatively lower economic development. For instance, Shi and Svensson (2006) also find such an asymmetry in PBCs in fiscal deficits across developed and developing economies, and propose an explanation for this divergence attributing it to the difference in the strength of institutions across those country groups.[13]

In column 5, we explore if the political system matter by testing whether presidential systems have a differential effect on electoral cycles. This is worthwhile to test, since political institutions may matter in electoral cycles. For instance, presidential systems are often considered to have more checks and balances and more limited flexibility in policy making relative to parliamentary systems, possibly affecting the politicians' ability to shape policies motivated by their re-election concerns (Julio and Yook 2012). However, it seems to be the case that the political system does not have much impact on the strength of electoral cycles in tax reforms.

In the last 2 columns, we focus on the presence of fiscal rules and IMF programs. On the former, it is sensible to examine whether fiscal rules can safeguard tax reforms against electoral pressures, since they likely impose additional limitations on the government's discretion over policy choices. On the latter, there is evidence from the broader literature on structural reforms that IMF programs can be an important driver of structural reforms. Da Silva et al. (2017) show that the presence of an IMF-supported programs or other forms of external conditionality facilitates reforms. David et al. (2020) find that reforms are positively associated with IMF-supported programs in Latin American countries, especially domestic finance reforms.

However, the results on columns 6 and 7 show that neither fiscal rules nor IMF-supported programs seem to have a direct impact on the likelihood of tax reforms in the sample. Moreover, none of these variables appear to have differential effects on electoral cycles in tax reforms. Two points are important to note regarding this set of results. The result on fiscal rules does not change much if we employ the same analysis for different types of fiscal rules (i.e., on debt, expenditure, budget, and revenue) separately. Next, on the role of IMF programs, a caveat of the analysis could be that those programs in the sample are concentrated on a few countries over a limited time span. In particular, the sample encompasses only seven countries that had at least one IMF program during the time period considered, corresponding to around 6 percent of the sample regarding country-month observations (469 observations).

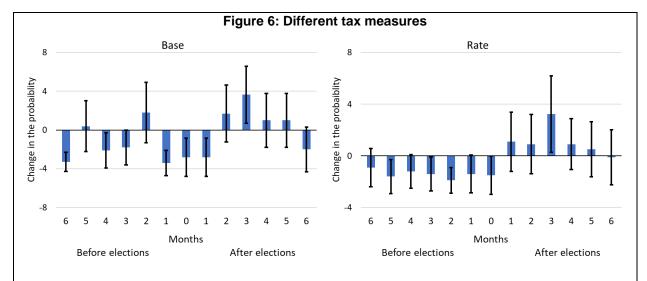## 4.5. Different tax measures and types

Finally, we explore whether electoral cycles in tax reforms are broad-based regarding different tax measures (base and rate changes) and types (CIT, PIT, VAT, SSC, EXE, and PRO).[14] These tests are also useful to examine whether the previous results are driven by electoral cycles in a specific tax measure or type. It is plausible that different types of taxes might have differential effects on distinct constituencies, which might affect the incentives of policy makers, particularly depending on how vocal these constituencies are. Figure 6

---

[12] The list of EMs is adopted from the IMF.

[13] Instead, Chauvet and Collier (2009) propose that more pronounced political cycles in economic policies in developing economies compared to the developed economies may be explained by that the electorates in the former are more prone to opportunistic government behavior.

[14] The number of country-month observations with a reform announcement each case is illustrated in the Appendix (Table A.2).
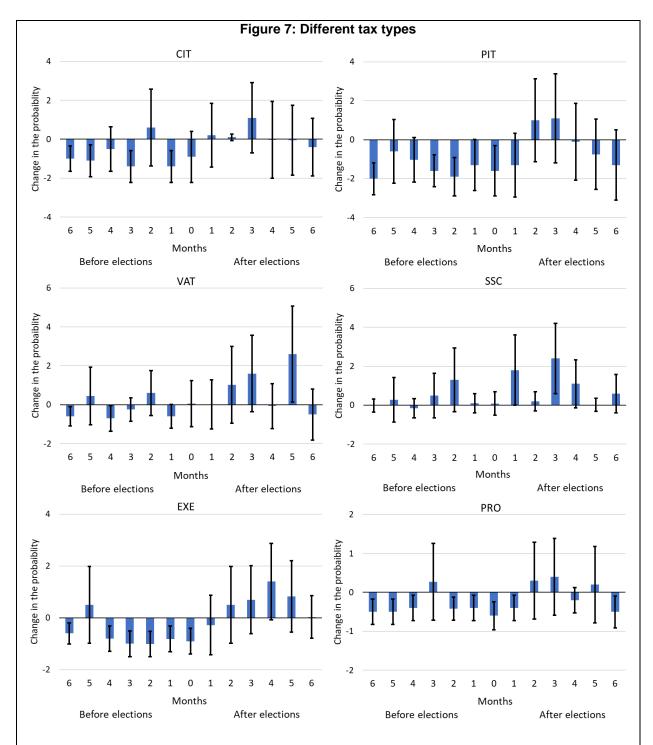
depicts the findings on reforms of tax different tax measures, i.e., tax base and rate. The results show that electoral cycles exist for both base and rate changes in line with the findings from previous sections.

Figure 6: Different tax measures

Notes: The figure documents the results based on equation 1. The dependent variable is a dummy which takes one for the months in which there is a tax increase announcement related to the corresponding measure (base or rate), and 0 otherwise. The figure illustrates the coefficient estimates (bars, in percentage points) and the 90 percent confidence intervals (vertical lines).

In Figure 7, we focus instead on different types of taxes, by replacing the dependent variable with a dummy indicating an increase in CIT, PIT, VAT, SSC, EXE, and PRO, respectively.[15] Given that a large fraction of voters may pay more prominent attention to tax changes that affect them directly (such as the effects of the reforms in personal income taxes and VAT on their income and prices), incumbent politicians may take this into consideration and choose to reform less salient taxes before elections or similarly avoid reforming more salient taxes. In addition, governments may target (or preserve) a specific section of the electorate, such as business owners, or homeowners, by manipulating a specific tax type. Our findings indicate that reforms in most types of taxes considered in the database tend to exhibit electoral cycles. Nevertheless, social security contributions (SSC) are one exception, where the effects of electoral cycles are less pronounced. This could perhaps be related to the fact social security contributions are directly linked to expected future transfers and therefore might be more palatable to taxpayers.

---

[15] It is worth to note that the number of reform announcements in the data is relatively low for SSC, EXE and PRO (as shown in Table A.2), which could be a drawback in interpreting the results regarding those three tax types.

Figure 7: Different tax types

Notes: The figure documents the results based on equation 1. The dependent variable is a dummy which takes one for the months in which there is a tax increase announcement related to the corresponding type (CIT, PIT, VAT, SSC, EXE, or PRO), and 0 otherwise. The figure illustrates the coefficient estimates (bars, in percentage points) and the 90 percent confidence intervals (vertical lines).

5. Conclusions and Policy Implications

The evidence presented in this paper indicates that tax reforms exhibit electoral cycles. Governments tend to avoid announcing reforms that increase tax rates and/or broaden the tax base during the months running up to elections, but become more likely to do so in the first few months following elections. The latter, also suggests that political capital, or perceived political opportunity, plays a role in the timing of post-election reforms. Overall, these findings are robust to a number of checks including alternative definitions of the dependent variable, alternative econometric specifications, and the inclusion of a number of additional control variables.

The analysis also shows that electoral cycles in tax reforms tend to be weaker in countries with stronger institutions, as proxied by variables such as law and order, bureaucratic quality, and civil liberties indexes. It appears that the ability of election-oriented executive branches (who are trying to get re-elected) to influence tax policy in favor of their short-term concerns is higher in countries where institutional constraints cannot restrict governments' discretion.

Nevertheless, the results also indicate that fiscal rules do not have much role in electoral cycles in tax reforms. Moreover, the presence of IMF programs do not appear to affect the strength of electoral cycles in tax reforms for the sample of countries considered. Nonetheless, it is important to note that the later finding should be treated with caution, since our sample only contains a limited number of emerging markets and does not include lower income developing countries due to data restrictions, which limits the prevalence of IMF-supported programs in the analysis. In this regard, an interesting avenue for future research could involve exploring the role of IMF-supported programs in electoral cycles in tax reforms in developing countries as the data on tax reforms become available.

The findings in this paper raise the question of how to provide appropriate incentives for election-oriented governments to design the tax policy optimally, particularly given that fiscal rules do not appear to be sufficient to safeguard tax policy against political pressure of upcoming elections. Besides posing a challenge for domestic policy making, electoral cycles could also be a potential obstacle to cross-border coordination in tax policy, as they may increase the complexity of the coordination problem by influencing the timing of reforms.

Appendix

Table A.1. Elections in the sample

Australia	Czechia	Germany	Japan	Poland	UK
3/1990	6/1990	12/1990	2/1990	10/1991	4/1992
3/1993	6/1992	10/1994	7/1993	9/1993	5/1997
3/1996	6/1996	9/1998	10/1996	9/1997	6/2001
10/1998	6/1998	9/2002	6/2000	9/2001	5/2005
11/2001	6/2002	9/2005	11/2003	9/2005	5/2010
10/2004	6/2006	9/2009	9/2005	10/2007	5/2015
11/2007	5/2010	9/2013	8/2009	10/2011	6/2017
8/2010	10/2013	9/2017	12/2012	10/2015	
9/2013	10/2017		12/2014		
7/2016			10/2017		
Austria	**Denmark**	**India**	**Korea**	**Portugal**	**USA**
10/1990	12/1990	5/1991	12/1992	10/1991	11/1992
10/1994	9/1994	4/1996	12/1997	10/1995	11/1996
12/1995	3/1998	2/1998	12/2002	10/1999	11/2000
10/1999	11/2001	9/1999	12/2007	3/2002	11/2004
11/2002	2/2005	4/2004	12/2012	2/2005	11/2008
10/2006	11/2007	4/2009	5/2017	9/2009	11/2012
9/2008	9/2011	4/2014		6/2011	11/2016
9/2013	6/2015			10/2015	
10/2017					
Brazil	**France**	**Ireland**	**Luxembourg**	**Spain**	
10/1994	3/1993	11/1992	6/1994	6/1993	
10/1998	6/1997	6/1997	6/1999	3/1996	
10/2002	6/2002	5/2002	6/2004	3/2000	
10/2006	6/2007	5/2007	6/2009	3/2004	
10/2010	6/2012	2/2011	10/2013	3/2008	
10/2014	6/2017	2/2016	10/2018	11/2011	
10/2018				12/2015	
				6/2016	
Canada	**Greece**	**Italy**	**Mexico**	**Turkey**	
10/1993	4/1990	4/1992	8/1994	10/1991	
6/1997	10/1993	3/1994	7/1997	12/1995	
11/2000	9/1996	4/1996	7/2000	4/1999	
6/2004	4/2000	5/2001	9/2006	11/2002	
1/2006	3/2004	4/2006	7/2012	7/2007	
10/2008	9/2007	4/2008	7/2018	6/2011	
5/2011	10/2009	2/2013		6/2015	
10/2015	6/2012	3/2018		11/2015	
	9/2015			6/2018	

Table A.2. The number of observations with reforms in each tax measure and type

Tax measure		Tax type					
Base	Rate	CIT	PIT	VAT	SSC	EXE	PRO
201	139	91	103	70	28	31	12

References

Aidt, Toke, Zareh Asatryan, Lusine Badalyan, and Friedrich Heinemann. 2020. "Vote buying or (political) business (cycles) as usual?" *Review of Economics and Statistics*, 102: 409-425.

Akhmedov, Akhmed, and Ekaterina Zhuravskaya, 2004. "Opportunistic political cycles: test in a young democracy setting." *The Quarterly Journal of Economics*, 119: 1301-1338.

Alesina, Alberto, Gerald D. Cohen, and Nouriel Roubini., 1992. "Macroeconomic policy and elections in OECD democracies." *Economics & Politics*, 4: 1-30.

Alesina, A., N. Roubini and G.D. Cohen, 1997, "Political Cycles and the Macroeconomy" (Cambridge: MIT Press).

Alesina, Alberto, and Andrea Passalacqua. 2016. "The political economy of government debt." Handbook of Macroeconomics 2: 2599-2651.

Alesina, Alberto, and Matteo Paradisi. 2017. "Political budget cycles: Evidence from Italian cities." *Economics & Politics*, 29: 157-177.

Amaglobeli, David, Valerio Crispolti, Era Dabla-Norris, Pooja Karnane, and Florian Misch. "Tax policy measures in advanced and emerging economies: a novel database". International Monetary Fund Working Paper No 110, 2018.

Amaglobeli, David, Valerio Crispolti, and Xuguang Simon Sheng. "Cross-Country Evidence on the Revenue Impact of Tax Reforms." International Monetary Fund Working Paper No 199, 2022.

Armingeon, Klaus, Sarah Engler and Lucas Leemann. 2022. *Comparative Political Data Set 1960-2020*. Zurich: Department of Political Science, University of Zurich.

Berry, Frances Stokes, and William D. Berry. "Tax innovation in the states: Capitalizing on political opportunity." *American Journal of Political Science* (1992): 715-742.

Berry, Frances Stokes, and William D. Berry. "The politics of tax increases in the states." *American Journal of Political Science* (1994): 855-859.

Biswas, Siddhartha, Indraneel Chakraborty, and Rong Hai. "Income inequality, tax policy, and economic growth." *The Economic Journal* 127, no. 601 (2017): 688-727.

Brender, Adi, and Allan Drazen. "Political budget cycles in new versus established democracies." *Journal of Monetary Economics* 52, no. 7 (2005): 1271-1295.

Chauvet, Lisa, and Paul Collier. "Elections and economic policy in developing countries." *Economic Policy* 24, no. 59 (2009): 509-550.

Clark, William Roberts, and Mark Hallerberg. "Mobile capital, domestic institutions, and electorally induced monetary and fiscal policy." *American Political Science Review* 94, no. 2 (2000): 323-346.

Clausing, Kimberly A. "Corporate tax revenues in OECD countries." *International Tax and Public Finance* 14, no. 2 (2007): 115-133.

Cruz, Cesi, Philip Keefer, and Carlos Scartascini. 2021. "Database of Political Institutions 2020". Washington, DC: Inter-American Development Bank Research Department.

Cruz-Gonzalez, Mario, Iván Fernández-Val, and Martin Weidner. 2017. "Bias corrections for probit and logit models with two-way fixed effects." *The Stata Journal* 17: 517-545.

Da Silva, A., Givone, A., and Sondermann, D., 2017. "When Do Countries Implement Structural Reforms" ECB Working Paper No. 2078, European Central Bank.

David, A. C., Komatsuzaki, T., and Pienknagura, S.. "The Macroeconomic Effects of Structural Reforms in Latin America and the Caribbean" International Monetary Fund Working Paper No 195, 2020.

Davoodi, Hamid R., Paul Elger, Alexandra Fotiou, Daniel Garcia-Macia, Xuehui Han, Andresa Lagerborg, W. Raphael Lam, and Paulo A. Medas. "Fiscal Rules and Fiscal Councils: Recent Trends and Performance during the COVID-19 Pandemic." International Monetary Fund Working Paper No 11, 2022.

Drazen, Allan. "The political business cycle after 25 years." *NBER Macroeconomics Annual* 15 (2000): 75-117.

Dubois, Eric. "Political business cycles 40 years after Nordhaus." *Public Choice* 166, no. 1 (2016): 235-259.

Ehrhart, Helene. "Elections and the structure of taxation in developing countries." *Public Choice* 156, no. 1 (2013): 195-211.

Fair, Ray C. "The effect of economic events on votes for president." *The review of economics and statistics* (1978): 159-173.

Freedom House. Freedom in the World Database.

Fuest, Clemens, Klaus Gründler, Niklas Potrafke, and Fabian Ruthardt. "Read My Lips? Taxes and Elections." CESifo Working Paper (2021).

Gavoille, Nicolas, and Katharina Hofer. "Capital Controls and Electoral Cycles." *IMF Economic Review* 69, no. 2 (2021): 275-324.

Gunter, Samara, Daniel Riera-Crichton, Carlos A. Vegh, and Guillermo Vuletin. "Non-linear effects of tax changes on output: The role of the initial level of taxation." *Journal of International Economics* 131 (2021): 103450

Gupta, Sanjeev, and João Tovar Jalles. "On the political economy determinants of tax reforms: evidence from developing countries." (2020).

International Monetary Fund. International Financial Statistics (IFS) Database.

Julio, Brandon, and Youngsuk Yook. "Political uncertainty and corporate investment cycles." *The Journal of Finance* 67, no. 1 (2012): 45-83.

Lami, Endrit, and Drini Imami., 2019 "Electoral cycles of tax performance in advanced democracies." CESifo Economic Studies, 65: 275-295.

Lancaster, Tony, 2000. "The incidental parameter problem since 1948." *Journal of Econometrics* 95: 391-413.

Leeper, Eric M., Todd B. Walker, and Shu-Chun Susan Yang. "Fiscal foresight and information flows." *Econometrica* 81, no. 3 (2013): 1115-1145.

Mertens, Karel, and Morten O. Ravn. 2012. "Empirical Evidence on the Aggregate Effects of Anticipated and Unanticipated US Tax Policy Shocks." *American Economic Journal: Economic Policy*, 4 (2): 145-81.

Mertens, Karel, and Morten O. Ravn. 2013. "The Dynamic Effects of Personal and Corporate Income Tax Changes in the United States." *American Economic Review*, 103 (4): 1212-47.

Nordhaus, W. D., 1975. "The political business cycle" *Review of Economic Studies*, 42: 169-190.

Persson T., Tabellini G. "Macroeconomic Policy, Credibility and Politics" Hardwood Academics Publishers, Chur, Switzerland (1990)

Political Risk Services Group. The International Country Risk Guide (ICRG) Database.

Rogoff K., Sibert A. "Elections and macroeconomic policy cycles" *Review of Economic Studies*, 55 (1) (1988), pp. 1-16

Rogoff K. "Equilibrium Political Budget Cycles" *American Economic Review*, 80 (1), pp. 21-36 (1990).

Schuknecht, Ludger., 2000 "Fiscal policy cycles and public expenditure in developing countries." *Public Choice* 102: 113-128.

Sever, Can, and Emekcan Yücel, 2022. "The effects of elections on macroprudential policy." *Journal of Comparative Economics*, 50 (2): 507-533.

Shi, Min, and Jakob Svensson. "Political budget cycles: Do they differ across countries and why?" *Journal of Public Economics* 90, no. 8-9 (2006): 1367-1389.

Vegh, Carlos A., and Guillermo Vuletin. 2015. "How Is Tax Policy Conducted over the Business Cycle?" *American Economic Journal: Economic Policy*, 7 (3): 327-70.