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The political business cycle of tax reforms

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Abstract

A political business cycle (PBC), with governments adjusting and timing economic policy for electoral gains, has long been hypothesized. A lack of data has so far limited testing of this phenomenon for government policies as opposed to fiscal outcomes such as tax revenue or government deficit, especially at the national level. We use new monthly data on tax reform announcements for a set of 22 democracies, 1988–2014, to test the PBC hypothesis for taxation. In addition to the traditional electoral strategy formulation of the PBC, we also put forward and test a capacity version of the PBC. We find evidence for the capacity version but not the traditional version of the PBC: tax reforms are less likely to be announced before elections and more likely after elections, independently of whether they are increases or decreases. Our evidence suggests that while a PBC exists, it may be less driven by strategic electioneering and more innocuous than previously assumed.

Keywords Political business cycle · Political budget cycle · Tax reform · Taxation · Political economy · Elections

JEL Classification D72 · P16 · H20

1 Introduction

There is an anonymous adage that states, "*One way to reduce taxes is to hold elections every year, because there never seem to be tax increases in an election year*." Tax policy is at the heart of the political process due to its redistributive nature. In the words of Holcombe (1998), "Tax policy is a product of politics, so a complete understanding of tax policy requires an explicit recognition of the political environment within which tax policy is made." Hence, a natural hypothesis is that tax policy is influenced by the timing of elections. Nordhaus (1975) coined the idea of a political business cycle (PBC). Studying the PBC is highly relevant. In a Keynesian economic framework, the main role of the government is to smooth out the real business cycle. Hence, it seems undesirable for

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electoral politics to introduce a PBC instead by cutting taxes and increasing spending prior to elections.[1]

Elections are a crucial event: voters either punish or reward the incumbent government, and decide the direction of the country for the next cycle. Whether it is to promote reforms or to simply stay in power, politicians need to win elections. As a result, the literature has long proposed that politicians have an incentive to announce reforms that win them elections (e.g., Downs, 1957; Buchanan & Tullock, 1975; Buchanan, 1989; Nordhaus et al., 1989). The basic premise of the PBC is that incumbents stimulate aggregate demand before an election in order to win votes, which results in higher growth and lower unemployment. The stimulus produces inflation, which is then eliminated by post-electoral austerity measures that result in contractions and an increase in unemployment (Alesina et al., 1992).

Given the importance of taxation, and the fact that taxes and transfers can be manipulated more quickly and easily than unemployment (Dubois, 2016), the strategic use of tax reform in order to achieve electoral gains has garnered academic attention. This analysis was originally focused on fiscal deficits. For example, Alesina et al. (1992) found that after controlling for economic determinants, government fragmentation and elections have a statistically significant effect on budget deficits in selected countries of the Organisation for Economic Co-operation and Development (OECD). The main limitation of their work is the lack of data. Yearly budget deficit data were available for only a small sample period. Given that elections do not happen every year, there were no more than four elections per country. Recent research has tackled this by focusing on either municipal elections or specific regional patterns (see, e.g., Aidt et al., 2011; Alesina & Paradisi, 2017; Foremny & Riedel, 2014; Hallerberg & Scartascini, 2017; Vegh & Vuletin, 2015).

A key issue in the literature on the PBC is the use of tax revenues as a proxy for tax policy (Prichard, 2018). Since tax revenues also fluctuate based on the state of the economy, they are a noisy proxy for politicians' tax policies. In this paper we test the PBC by analyzing actual tax policy reform announcements, using the Tax Policy Reform Database (Amaglobeli et al., 2018), a novel dataset from the International Monetary Fund (IMF) that tracks tax reforms across 23 developed and developing countries between 1975 and 2014. This is the most comprehensive international database to date. If the database is extended beyond 2014, future research could cover more recent elections in the analysis.

This paper contributes to the PBC literature in three main ways. First, we use actual tax policy reform announcements instead of macroeconomic or fiscal outcomes. Implementation of policies takes time and is therefore difficult to pinpoint, especially in annual data. The announcement of a reform is thus a more precise measure for the timing of a policy. Second, we contribute to the issue of data scarcity by analyzing tax policy reform in a more granular manner (distinguishing types of taxes and directions of change) for over 30 years for 22 countries. Third, we contribute to prior knowledge on the length of the policy cycle by using monthly data. Some past research using yearly data omitted all months in the electoral calendar year. A more sophisticated approach calculates electoral years as fractional variables depending on how many months precede the election in the calendar year of the election (Franzese, 2000). This still does not allow the use of information on whether reforms in the electoral calendar year preceded

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[1] Like most of the literature, we use the term "political business cycle" even though we are studying policies and not their macroeconomic outcomes. In the literature studying policy cycles, the term "political budget cycle" is sometimes used instead.

the election or not. We use monthly data and the election date to decompose the "electoral year" in pre-electoral months, an election month, and post-electoral months.

We test two lines of theoretical expectations: the traditional PBC hypothesis and another related to capacity constraints. First we test whether strategic decreases of salient taxes are more likely before elections since tax cuts act as signals of competence to the electorate (Hallerberg & Hagen, 2017; Rogoff & Sibert, 1988; Aidt et al., 2011; Murtinu et al., 2022). We also test the inverse, whether strategic increases of salient taxes are more likely after an election. Second, we evaluate a set of alternative hypotheses that are related to governments having the capacity or mandate to actually push reforms, in which case reforms would be less likely before an election and more likely after, independently of their direction and salience.

This paper contributes to the ongoing quest to better understand what determines tax policy by providing empirical evidence on the length and nature of the PBC. Our findings are to some extent in line with prior research. We confirm that tax reform is less likely in the time preceding an election. However, surprisingly, we do not find evidence for politicians strategically using tax reform in order to garner electoral support. Tax reforms are less likely during pre-electoral times independently of the direction or salience of the reform. Also, the presence of a right- or left-leaning incumbent does not influence the likelihood of value-added tax (VAT), personal income tax (PIT), or corporate income tax (CIT) reforms prior to an election. The pattern of pre-electoral tax reform seems to suggest a decrease in executive and legislative productivity or an overall halt in reforms regarding tax matters. These results are robust when controlling for political factors such as control of cabinets or change of party in charge and economic variables such as the presence of a crisis or a reduction in tax revenue in the previous year. In addition, we find that in the first 6 to 12 months after an election, the likelihood of tax reforms is significantly higher.

In terms of the length of the PBC, we find evidence for a relatively short duration. Our main specifications test for 6- and 12-month windows, but we run additional analyses with 3-month increments from 0 to 18 months. Before elections, the 1- to 3-month window is most significant; after elections, the 4- to 6-month window is most significant. This suggests either that newly elected governments use their electoral mandate to push for quick reform and fulfill campaign promises, or that, alternatively, given the polarizing nature of tax reform, politicians rather push for tax reform at the beginning of their mandates to give the electorate time to forget about this. Our results are robust to alternative definitions or measurements of tax reform announcements (our dependent variable), and several tests related to political systems and election types.

In a recent working paper, Fuest et al. (2021) use the same IMF Tax Policy Reform Database (Amaglobeli et al., 2018) data, and combine it with OECD data to build a new dataset with annual indices of tax reforms. They also use this dataset to study the PBC of tax reforms. One main difference is that we use a monthly approach, shedding more detailed light on the length of the PBC. In addition, we do not combine increasing and decreasing reforms into an index, avoiding the arbitrary coding choices (such as how to add up reforms coded as major or minor in the IMF data) that are required to end up with one index number. In terms of results, like Fuest et al. (2021), we find that tax increases are more likely after elections. However, our approach also shows that before elections, both tax increases and decreases are less likely—consistent with our novel hypothesis of pre-electoral constraints. Finally, both Fuest et al. (2021) and we find evidence for more strategic timing of increases of salient taxes such as PIT and VAT. All in all, our results are consistent with but complementary to Fuest et al. (2021).

The remainder of this paper is organized as follows: Section 2 presents an overview of the literature on the PBC. Section 3 outlines our theoretical predictions and hypotheses. Section 4 describes the data and the methodology. Section 5 shows both descriptive and regression results, and Sect. 6 has robustness checks. Finally, Sect. 7 discusses our findings and conclusions.

2 The political business cycle

In a seminal work for the field of public choice, Downs (1957) postulated that parties choose policies to maximize votes and win elections. About 20 years later, influenced by Frey and Lau (1968), Nordhaus (1975) hypothesized the existence of a "political business cycle." If politicians opportunistically try to maximize votes, they have an incentive to boost the economy before elections through loose monetary policy and increased deficit spending. As a result, pre-electoral years are hypothesized to be expansion years, characterized by high employment. By the time elections happen, inflation has gone up, and as a result austerity measures need to be enacted, resulting in higher unemployment (Nordhaus, 1975; Dubois, 2016; Alesina et al., 1992). Hence, post-electoral years are hypothesized to be recession years. Thus, actions of politicians result in undesirable economic cycles that are dependent on the electoral cycle (Blankart & Koester, 2005).

In 1977, Hibbs adds to the literature by adding partisanship, in connection to the Phillips curve. Proposing the existence of "partisan cycles," where politicians and their parties try to maximize votes from their intended voters or "clientele." The left is "unemployment-averse" and in favor of inflation, while the right is "inflation-averse"—as their clientele of upper middle class suffers more from inflation (Blankart & Koester, 2005; Hibbs, 1977). The electoral and partisan connection was made by Frey and Schneider (1978) by proposing that parties follow partisan lines as long as approval is high. If approval is low before elections, parties will fall back into the expansionary pre-electoral cycle.

Although Nordhaus' seminal work garnered interest, empirical research based on his model yielded mixed results. The main shortcoming of empirical tests of the PBC is the focus on macroeconomic outcome variables over which politicians do not have much control, instead of focusing on policy instruments (Tufte, 1980). By the mid-1990s and early 2000s, the quest to find evidence for a PBC in macroeconomic outcome variables had faded out. What remained was an interest in finding a PBC in fiscal outcomes such as government spending, deficit, and debt (see, e.g., Prichard, 2018; Alt et al., 2010; Rogoff & Sibert, 1988; Bohn & Sturm, 2021; Roubini & Sachs, 1989; Alesina et al., 1992).

One of the main challenges faced by the literature on the PBC regarding policy in general (Strobl et al., 2021), and tax policy specifically, has been a lack of data. Policymakers have control over two main policy instruments, the statutory tax rate and the tax base, but they have less control over actual tax revenues. Because data on the announcement of tax policies were missing, research on tax policy has relied on data such as tax revenues and fiscal balance (see, e.g., Alt and Lassen 2006; Shi & Svensson, 2006). Considering that these outcomes are also affected by the economic cycle, their use as dependent variable gives rise to endogeneity issues (Alesina & Paradisi, 2017; Vegh & Vuletin, 2015).

Recent work has been tackling the lack of data in multiple ways, for example, by looking into local elections and tax policy, rather than on a national or cross-country level. Drazen and Eslava (2010) find pre-electoral spending increases in Colombian municipalities. Foremny and Riedel (2014) find that German municipalities reduced local business taxes

during the election year and the year prior, while they increased these taxes the year after elections. Similarly, Alesina and Paradisi (2017) study real estate taxes in Italian municipalities and find evidence of political cycles on a municipal level. Chang et al. (2020) analyse the behavior of state politicians in the United States regarding gas tax laws and CIT laws. They find evidence that politicians are most likely to enact tax increases right after an election.

Finally, new data sources have led to cross-country research. Katsimi and Sarantides (2012) find lower fiscal revenues prior to elections in OECD countries 1972–1999. Focusing on Latin America between 1990 and 2004, Hallerberg and Hagen (2017) find that the likelihood of tax increases is significantly lower prior to an election. Exploring the economic and political causes for tax policy changes in OECD countries between 1990 and 2001, Hallerberg and Scartascini (2017) surprisingly find no partisan effects in tax policy reform, and find post-electoral effects only for CIT increases.

3 Theoretical predictions

Traditionally, the PBC is hypothesized to be the result of politicians' electoral strategies. In this paper, we focus solely on the revenue side of the PBC: tax reforms. Since voters dislike taxes (Berry & Berry, 1994), incumbent politicians will avoid announcing tax increases prior to elections. Not only will they avoid increases, but as elections approach they have an incentive to announce tax decreases (Hallerberg & Hagen, 2017; König & Wenzelburger, 2017; Rogoff & Sibert, 1988). Tax decreases may be announced prior to elections to influence short-sighted or inattentive voters, but may also act as signals of competence to rational voters lacking information on the quality of government (Franzese, 2002). If the incumbent government deems tax increases necessary or desirable, it will plan to announce them shortly after being re-elected, in the hope that the electorate has forgotten about them by the next elections. If a new government is elected, it can credibly claim at the start of its mandate that unpopular tax increases are necessary to correct the wrongdoings of the previous government (Strobl et al., 2021).

Previous literature indicated the importance of the saliency of taxes both theoretically (see, e.g., Bracco et al., 2019; Golden & Poterba, 1980; Matějka & Tabellini, 2021) and empirically (see, e.g., Chetty et al., 2009; Cabral & Hoxby, 2012; Taubinsky & Rees-Jones, 2018). If voters have limited attention or are rationally uninformed (Downs, 1957), the PBC should be more pronounced for salient taxes like VAT and PIT (Brys, 2011; Chang et al., 2020; Lami & Imami, 2019). Taxes such as CIT, which are less salient for the average voter (Alt et al., 2010), are less likely to show a pronounced PBC. Based on these traditional arguments for the PBC, we hypothesize the following:

- **H1a: strategic decreases.** Tax decreases, especially for taxes salient to voters, are more likely to be announced prior to an election.
- **H1b: strategic increases.** Tax increases, especially for taxes salient to voters, are more likely to be announced after an election.

In addition to these traditional PBC hypotheses, we also have in mind an alternative PBC related to constraints rather than incentives. In particular, incumbent politicians and bureaucracies may not announce new reforms prior to an election because they know they lack the capacity to implement them so close to an election. After all, implementing

reforms takes time. In addition, governments may lack the legitimacy or mandate to enact reforms just prior to elections. Conversely, newly elected governments have a strong mandate and ample time to implement reforms. These constraint arguments lead to the following hypotheses:

- **H2a: pre-electoral constraints.** Tax reforms, no matter their type or direction, are less likely to be announced prior to an election.
- **H2b: post-electoral rush.** Tax reforms, no matter their type or direction, are more likely to be announced after an election.

Note that H2a has no alternative explanation in terms of electoral strategies. It is in direct contradiction with H1a. In contrast, H2b is compatible with H1b, although it is not observationally equivalent. Note that H2b may also be derived from an electoral strategy, namely a willingness to be seen as hitting the ground running—especially if there are shifts in government composition. Our empirical analysis will control for a new party leading the government after the election. In any case, in contrast to the traditional H1a and H1b, H2a and H2b predict a PBC that is not driven by blatant strategic electioneering.

4 Data and methodology

We combine data on tax reforms with electoral and political data. The tax reform data come from Amaglobeli et al.'s (2018) Tax Policy Reform Database (TPRD). The TPRD is a novel dataset of tax reforms across 23 advanced and emerging market economies from 1988 onwards. Previous datasets focused mainly on statutory tax rates. In contrast, the TPRD contains information on the direction of tax reform (decreasing or increasing) and whether it affects the rate or the base of PIT and CIT, VAT and sale taxes, social security contributions (SSC), excise taxes (EXE), and property taxes (PRO). For our research we focus on PIT, CIT, and VAT, as they are covered more comprehensively (Amaglobeli et al., 2018), i.e., the data are more reliable. While the rate refers to the height of a tax in percentage, the base refers to which amounts are taxable. The date of announcement of measures and implementation is also included; we use this to build a monthly rather than a yearly dataset. Given that we study the interaction between tax reforms and the electoral cycle we use the announcement date as a reference. Our main dependent variables are the likelihood of any reform, the likelihood of a PIT, CIT, and VAT reform (analyzed independently), and the likelihood of increasing and decreasing tax reforms.

Throughout this article, when referring to tax reforms, we refer to tax reform announcements. This decision was based on the fact that in the TPRD, announcements correspond to the day when representatives of the government announced the reform, which is likely more connected to the PBC than implementation, a process that requires intervention from authorities beyond the executive. Moreover, not all reforms that are announced are necessarily implemented, and the implementation lag varies and can depend on factors like the beginning of the fiscal year (Amaglobeli et al., 2018). Nonetheless, the determinants of the implementation of reforms should be studied further.

It is important to distinguish our data on government-announced reforms from party manifestos or campaign promises. The TPRD registers reforms announced by the government. Before elections, this is necessarily the incumbent government. After elections, it can be the previous incumbent or a new government. Since new governments may behave

differently after elections versus incumbents, we will control for party change post-elections. While campaign promises are useful to study, no systematic data exist for it. In addition, the worry behind the idea of a PBC is that government policies induce undesirable macroeconomic fluctuations on top of the real business cycle. Campaign promises are unlikely to have an equally strong effect in this respect as actually announced policy reforms.

The electoral and political data were extracted from the Database of Political Institutions (DPI) (Cruz et al., 2020). The DPI contains institutional and electoral data such as dates of legislative and executive elections and their results, tenure and stability of the government, and party affiliation and ideology from 1975 onward. We take advantage of the comprehensive nature of the DPI to match it to the monthly tax reform data on electoral cycles rather than chronological years. Originally, DPI data capture the state of the country on January 1. Thus, a "1″ is recorded in the year following the election for our election dummy. However, since information on the actual date of the election is provided, we were able to match the corresponding "1″ to the actual month of the election. In doing so, we do not lose the data relevant to the electoral year. Using our monthly approach, only reforms announced during the electoral month cannot be allocated to before or after the election. Section 4.1 summarizes the main characteristics of the sample, including the frequency of reform per country and the most common types and directions of change. Section 4.2 briefly shows the number of elections covered and some basic electoral and political characteristics of the countries in the sample.

4.1 Tax reforms in the sample

As mentioned, our source of data for tax reforms is the TPRD, consisting of an unbalanced panel of 23 countries, 1988–2014, and a total of 8588 months. We exclude China, because it does not have democratic elections. Hence, we have 22 countries in our sample. Table 7 in Online Appendix A shows some main characteristics of the reforms and reform years in the sample. Our data cover 2113 reforms, with 888 country-months having at least one reform. Brazil is the country with the fewest tax reforms, standing at only 32, whereas countries like Australia, Canada, Germany, Denmark, Ireland, and the USA have over 100 reforms. The majority of reforms in our sample are reforms of PIT. All categories of reforms are present in the 22 countries, with the exception of the United States, which does not have a federal VAT. As Fig. 1 shows, CIT reforms are also very common, while VAT reforms are less common.

4.1.1 Personal income taxes

Figure 2 shows how many PIT reforms were decreasing (red) and how many increasing (blue) either tax rate or tax base. Spain has the highest number of PIT reforms, with slightly above 80 reforms in the 27 years between 1988 and 2014.[2] Few countries are relatively balanced in terms of decreases and increases (Turkey and Poland); in many countries, PIT decreases happen nearly twice as often as increases.

Increases and decreases are not a complete picture of tax reform, since an increase in the tax rate can be balanced out with a shrinkage of the tax base. Figure 2 shows more

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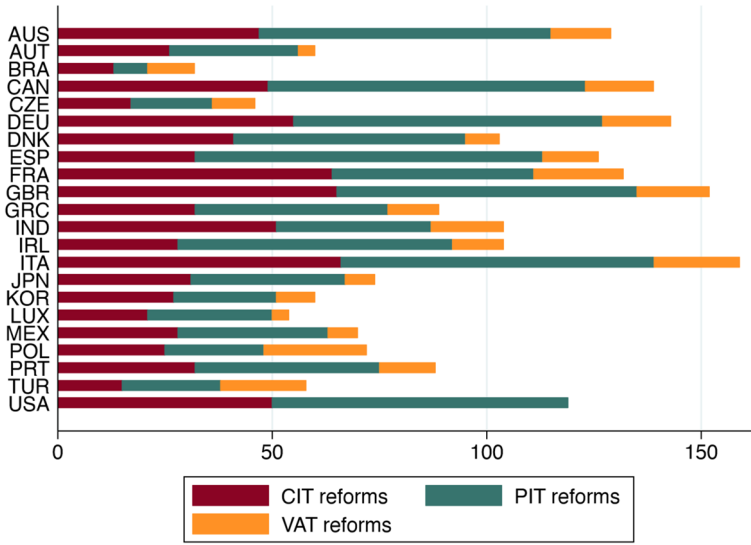[2] A list of the years that each country is covered is available in Table 4.2.
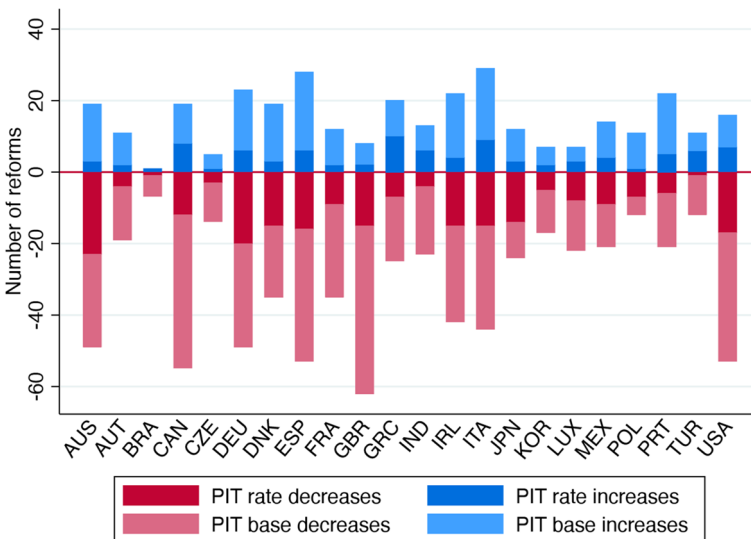
Fig. 1 Overview of reforms

Fig. 2 Overview of all PIT reforms

nuance by illustrating the total number of reforms, affecting the tax rate or the tax base via increases or decreases. The light red parts of the columns refer to PIT base decreases and represent the highest number of reforms for most countries, closely followed by PIT rate decreases in dark red. Overall, rate increases are not as common as base increases. Although these figures illustrate the direction, type, and number of reforms, they do not account for the size or importance of reforms and other relevant aspects. We leave even more detailed analyses for future work.
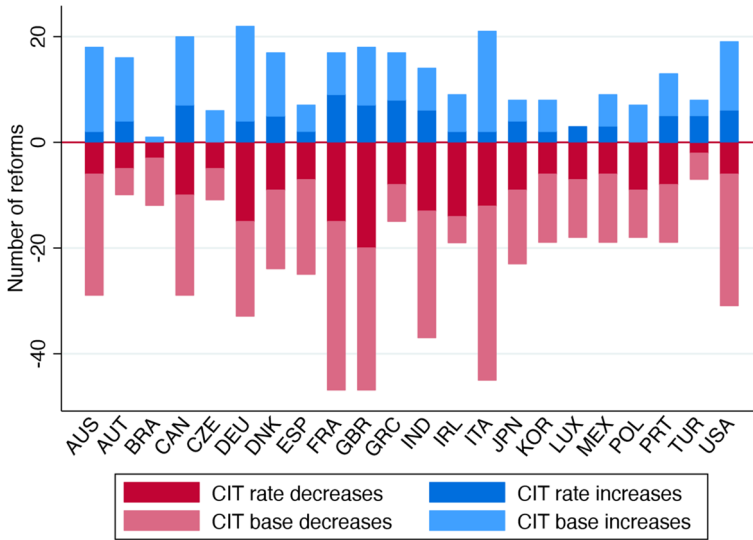
Fig. 3 Overview of all CIT reforms

4.1.2 Corporate income taxes

In Fig. 3 we see a clear dominance of CIT decreases, which is in line with the general finding that CIT rates have been decreasing over time (Hallerberg & Hagen, 2017), probably due to global tax competition. Some countries, such as Luxembourg or Brazil, decreased corporate taxes to the extent that decreases outnumber the number of increases by more than a factor 4. Given that the directions of taxes do not portray a full picture, the figure also shows that CIT increases mainly pertained to the tax base (light blue) rather than the rate. Furthermore, we show that in most countries in the sample the most common type of CIT reform was a reduction of the base, illustrated in light red. Interestingly, at least in our dataset, Brazil has not had a single CIT rate increase announced in the years that it is included.

4.1.3 Value-added taxes

VAT reforms are less common than PIT and CIT reforms. Although VAT is widely used by now—except in the United States at the federal level—its introduction in many countries dates back only to the second half of the twentieth century. Table 1 shows that our dataset covers the introduction of VAT for nearly half of the countries in the sample. Other countries such as France and Ireland introduced VAT only 7 and 3 years before our dataset begins, respectively. Consistent with its growing importance during the period under study, Fig. 4 shows the changes in VAT to be mainly rate increases, in contrast to PIT and CIT. Furthermore, also different from the previous taxes, most changes are related to the rate rather than the base.

Table 1 Introduction of VAT across sample

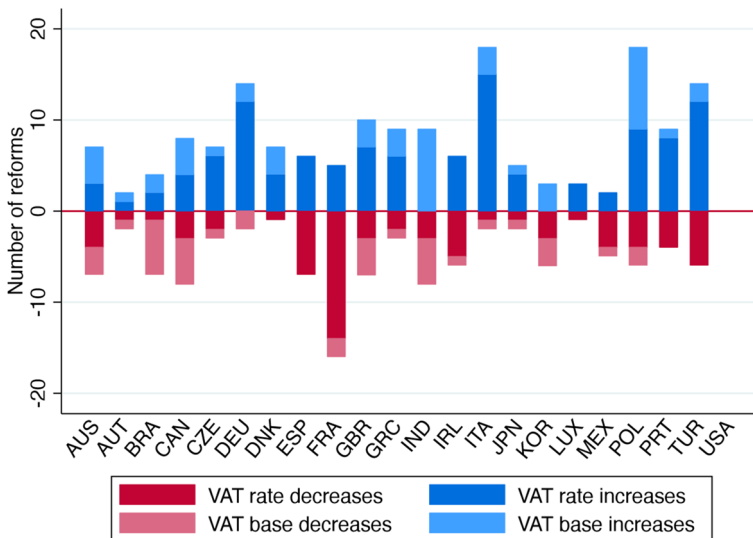| Country	Year of introduction
Australia	2000
Canada	1991
Czech Republic	1993
Greece	1987
India	2005
Japan	1989
Korea	1977
Poland	1993
Portugal	1986
Spain	1986

Made by authors based on data from the OECD (2020) report on VAT

Fig. 4 Overview of all VAT reforms

4.2 Elections in the sample

Table 2 shows the elections in our dataset. In total we have 202 legislative elections and 37 executive elections. The majority of our sample consists of countries with a parliamentary regime, which means that the executive is chosen by parliament. Our main explanatory variable, *elec*, is a composite of legislative elections for parliamentary countries and

Table 2 Elections in dataset

| Country	Years in dataset	Legislative elections	Executive elections
Australia	1975–2014	14	
Austria	1975–2012	8	1
Brazil	1988–2013	5	6
Canada	1975–2013	11	
Czechia	1991–2012	5	
Germany	1975–2011	9	
Denmark	1975–2012	14	1
Spain	1977–2014	7	
France	1975–2014	9	6
Great Britain	1975–2010	8	
Greece	1987–2013	8	1
India	1988–2014	7	
Ireland	1975–2011	6	
Italy	1975–2014	11	
Japan	1975–2014	14	
Korea	1975–2014	6	5
Luxembourg	1975–2007	6	
Mexico	1987–2013	9	6
Poland	1988–2013	8	5
Portugal	1975–2013	13	
Turkey	1985–2014	6	
USA	1975–2011	18	9
Total		202	37
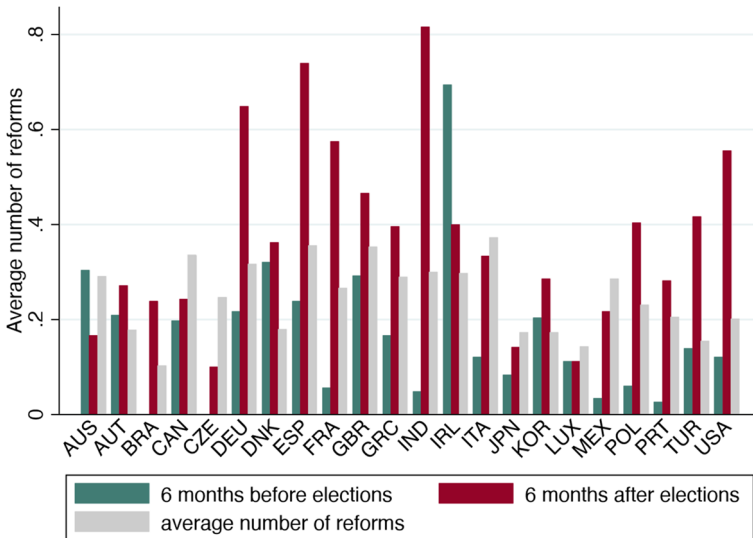
Fig. 5 Average of reforms around an election -6 months-
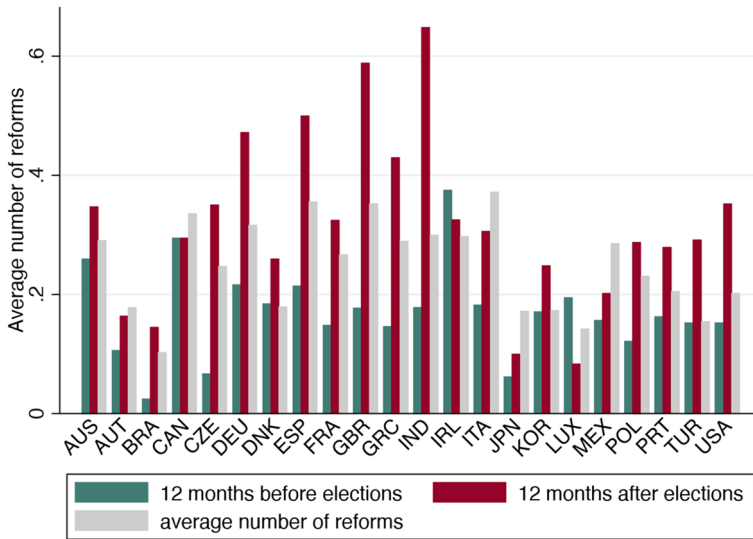
Fig. 6 Average of reforms around an election -12 months-

executive elections for presidential countries.[3] This is in line with recent literature (see, e.g., Bohn & Sturm, 2021; Vergne, 2009; Shi & Svensson, 2006). Nonetheless, we also run the analysis for legislative elections, *legelec*, and executive elections, *exelec*, separately in Sect. 6. This is especially important given that elections in parliamentary regimes are not exogenous, because the government can fall. Therefore, we also run robustness checks for snap elections that happened earlier than expected.

4.3 Descriptive evidence on tax reforms before and after elections

Figures 5 and 6 illustrate the average number of reforms per country 6 and 12 months before and after an election,[4] including the overall average of reforms in light gray for comparison. When observing the average number of reforms 6 months before, with the exception of Australia and Ireland, in all countries the average total number of reforms before elections is lower than that after elections. Australia might be an outlier because the electoral cycle in Australia lasts only 3 years. When analyzing the 12-month period, a similar pattern arises, with Ireland[5] as the sole exception. However, it is necessary to bear in mind that in these figures snap elections are included, which might generate an overlap between pre- and post-electoral periods. For example, Australia had elections in March 1983 and

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[3] Given the nature of Portuguese elections where the president has little executive power, presidential elections for Portugal have been excluded.

[4] We calculate these averages per country by adding the number of reforms in each 6- and 12-month period before elections and per election, and later divide this by the total number of elections

[5] There are two potential explanations as to why Ireland is an outlier. Our dataset captures a critical period in Irish history in which there was considerable political turmoil and change. This includes the signing of the Good Friday Agreement in 1998, a year in which coincidentally we also see a peak in tax reforms. In addition, we also capture the beginning of what would be the Irish "low CIT" strategy that started in 1989 with the arrival of Intel (Noonan 2021); this year also represents a peak in tax reforms for the country.
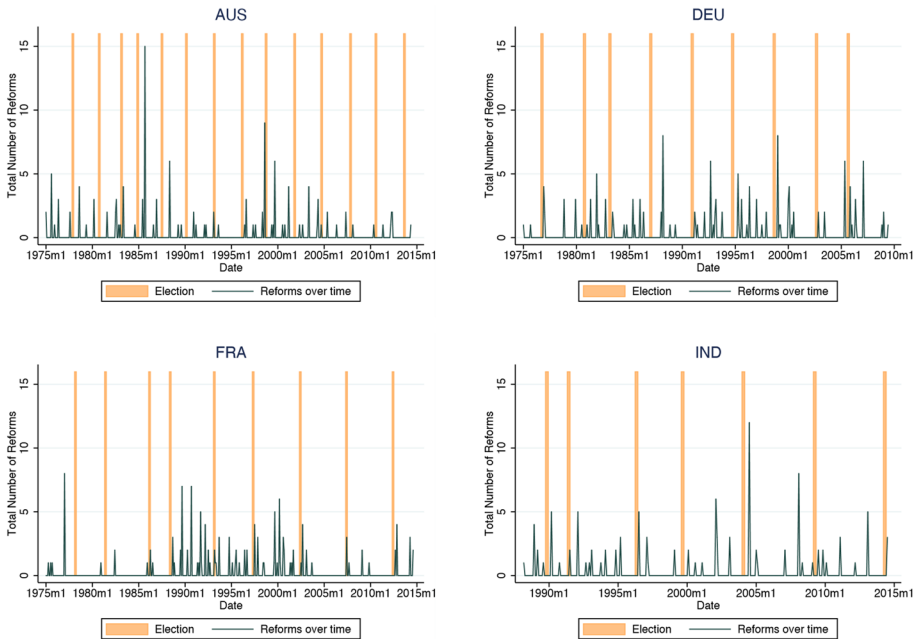
Fig. 7 Total number of reforms and the electoral cycle for selected countries; see Online Appendix B for the remaining countries

December 1984. This means that the months from December 1983 until March 1984 are both in the post-electoral period of the March 1983 election and the pre-electoral period of the December 1984 election.

Figure 7 illustrates the total monthly number of reforms, with vertical lines marking elections. Hence, each space between two vertical lines represents an electoral cycle. These figures do not give further information on the type or nature of the reforms, but from a first glance, it seems the peak in total number of announced reforms comes usually after the elections. Furthermore, for each of the countries there does not seem to be a recurring month in which most reforms happen, a hypothesis one might entertain in countries that have a traditional annual political calendar for announcing reforms. In any case, our empirical analysis will include country-calendar month fixed effects to control for country-specific seasonality.

4.4 Political and economic control variables

The selection of control variables is based on the existing literature on PBCs. The controls can be divided into two main categories: political and economic.

Political control variables come from the DPI (Cruz et al., 2020).[6] We include the Herfindahl index of the government, *herfgov,* defined as the share of seats they have relative to

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the total. This serves as a proxy for the relative power of the government to pass reforms. Similar to Hallerberg and Scartascini (2017) and Castanheira et al. (2012), we also control for the ideology of the ruling party by including a dummy, *right*, equal to 1 for right-wing parties and 0 for left-wing parties. In addition we control for the influence veto players can have (Hallerberg & Scartascini, 2017): as governments might find it hard to push for reforms if they lack support, we include the variable *allhouse* that indicates to what extent the executive has control over the houses that have lawmaking powers (see Gunzinger & Sturm, 2016). Finally, we also control for changes in the executive through a self-coded dummy variable called *partychange* that takes the value of 1 if there is a different party governing after an election.[7] As stated before, one may expect new governments of different composition to enact more reforms than re-elected incumbents.

Economic control variables come from different sources. We use banking crises *bankingcrisis* from the Reinhart and Rogoff (2009) dataset[8] and complement it with Laeven and Valencia (2013) for missing data. We also include data from the World Bank and the IMF on the lagged tax to GDP ratio, debt to GDP ratio, and GDP growth, in order to account for the pressure the government can face to introduce a reform.[9]

4.5 Estimation technique

Our dataset has a panel structure with monthly observations. The monthly structure allows us to shed light on the length of the PBC. Consider Austria, where elections are usually between October and December. When working on a yearly basis, reforms passed almost two chronological years before the election are considered to belong to the year before the election. For a concrete case, consider the elections in November 2002. Our monthly dataset allows us to observe reforms announced between November 2001 and the date of the election.

Our main dependent variable, *reform*, is binary: whether the government announced a tax reform in that month or not. As reported in Online Appendix D.5, our results are robust to using the number of reforms as a dependent variable instead. Variations in the dependent variable that we report disaggregate by type of reform and whether they were increases or decreases. Our main explanatory variables are the occurrence of an election 6 months prior, or having an upcoming election in the next 6 months. Given that the sample includes parliamentary, presidential, and semi-presidential regimes, we have generated a variable called *elec*, which captures the occurrence of any type of election. We also replicate the analysis taking into account a 12-month time frame. The 6- versus 12-month specifications allow us to take a peek into the length of the PBC.

Our baseline logit-model specification is

$$\text{logit}(y_{it}) = \beta_0 + \beta \, \text{Elections}_{it} + \gamma X_{it} + \delta Z_{it} + \kappa_{im}, \tag{1}$$

where *y* is coded as 0 or 1 depending on whether the government announced a tax reform during that month. The main explanatory variable are elections (having an election 6 months before/or the period 6 months after the election). *X* and *Z* are vectors of the

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[7] This is coded based on variable *execme* that has the name of the party in power for a set year.

[8] We use the latest version as of April 2020 available on their website.

[9] Our analysis uses monthly data, but not all variables are available on a monthly basis. When we have annual data, we use the value for the year for each month in that year.

Table 3 Likelihood of tax reform 6 months before and after an election

Variables	(1)	(2)	(3)	(4)
before_elec6	−0.354***	−0.386***	−0.445***	−0.450***
	(0.123)	(0.123)	(0.131)	(0.132)
after_elec6	0.283**	0.233**	0.231*	0.228*
	(0.118)	(0.116)	(0.121)	(0.121)
Observations	7407	6984	6691	6691
Country-month FE	Yes	Yes	Yes	Yes
Political	No	Yes	Yes	Yes
Economic	No	No	Yes	Yes
Crises	No	No	No	Yes

Clustered standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4 Likelihood of tax reform 12 months before and after an election

Variables	(1)	(2)	(3)	(4)
before_elec12	−0.307***	−0.331***	−0.354***	−0.357***
	(0.0900)	(0.0932)	(0.0977)	(0.0980)
lag_elec12	0.160*	0.119	0.123	0.121
	(0.0887)	(0.0891)	(0.0932)	(0.0938)
Observations	7407	6984	6691	6691
Country-month FE	Yes	Yes	Yes	Yes
Political	No	Yes	Yes	Yes
Economic	No	No	Yes	Yes
Crises	No	No	No	Yes

Clustered standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

political and economic control variables. We include country-calendar month fixed effects κ_{im} to account for differences between countries and country-specific seasonal variation. In particular, these fixed effects absorb seasonal variation in reforms due to different fiscal years (Brender & Drazen, 2013; Veiga et al., 2017) or traditions of announcing reforms in a certain calendar month. This means that for identification we are exploiting variation in reforms in a given calendar month close to elections versus the same calendar month in non-election years. We also cluster standard errors at the country-calendar-month level, to account for observations not being independent within clusters.

5 Results

We start with testing the simpler hypotheses H2a and H2b, which do not consider the direction or salience of tax reforms. Here the dependent variable is 1 if any reform was announced in the month. Next we address the more granular H1a and H1b.

Table 3 presents the first results. Model (4) includes all explanatory and control variables and can be considered our main model (Section C in the Appendix includes the coefficients of all control variables). In line with H2a and H2b, reforms are significantly less

Table 5 Likelihood of increasing and decreasing tax reform 6 and 12 months before and after an election

Variables	(1)	(2)	(3)	(4)
	Tax increase 6	Tax decrease 6	Tax increase 12	Tax decrease 12
before_elec6	−0.991***	−0.278**		
	(0.233)	(0.133)		
after_elec6	0.412***	0.181		
	(0.142)	(0.140)		
before_elec12			−0.821***	−0.245**
			(0.152)	(0.108)
after_elec12			0.265**	−0.001
			(0.112)	(0.113)
Observations	5253	5984	5253	5984
Country-month FE	Yes	Yes	Yes	Yes
Political	Yes	Yes	Yes	Yes
Economic	Yes	Yes	Yes	Yes
Crises	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6 Likelihood of increasing and decreasing tax reform 6 months before and after an election by type of election *CIT, PIT & VAT*

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	CIT−	CIT+	PIT−	PIT+	VAT−	VAT+
before_elec6	−0.698***	−1.101***	−0.001	−0.800***	−0.034	−1.543***
	(0.197)	(0.323)	(0.159)	(0.277)	(0.347)	(0.492)
after_elec6	0.158	0.343*	0.216	0.383**	0.242	0.761***
	(0.169)	(0.193)	(0.166)	(0.176)	(0.288)	(0.243)
Observations	4810	3529	4676	3522	1833	2489
Country-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Political	Yes	Yes	Yes	Yes	Yes	Yes
Economic	Yes	Yes	Yes	Yes	Yes	Yes
Crises	Yes	Yes	Yes	Yes	Yes	Yes

Clustered standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

likely before, and significantly more likely after an election.[10] Table 4 shows the same models but with 12-month windows before and after an election. Consistent with H2a, reforms are also significantly less likely 12 months before an election. However, contrary to H2b, the post-electoral rush is not significant for the 12-month window. This sheds some first light on the length and the lack of symmetry of the PBC of tax reform announcements.

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[10] This is controlling for newly elected governments rather than re-elected incumbents being in charge after elections. As can be seen in the Online Appendix, the coefficient of the control variable *partychange* is positive as expected, but not significant.

In addition, it shows the benefit of working on a monthly basis; a calendar year approach could fail to identify the 6-month post-electoral rush.

However, the likelihood of reform is not the only relevant aspect of the PBC. For the traditional PBC, as formulated in H1a and H1b, the direction of reform is critical. Table 5 separates out increases and decreases. In contrast to H1a and H1b, there is no difference in the direction of the coefficients for increases and decreases.[11] In particular, contrary to H1a but consistent with H2a, tax decreases are *less* likely before elections. Here our results differ somewhat from most earlier empirical findings, revealing a strategic cycle of decreases before elections and increases after (see, e.g., Hallerberg & Hagen, 2017; König & Wenzelburger, 2017; Foremny & Riedel, 2014; Alesina & Paradisi, 2017).

Under the traditional electoral strategy formulation of the PBC, the salience of different kinds of taxes should matter, as per H1a and H1b. We consider PIT and VAT to be more salient to voters (since they pay these taxes) than CIT (which only voters with a firm would know about). However, Table 6 shows that there is no evidence for strategic tax decreases prior to an election, not even for salient PIT and VAT taxes.[12] Consistent with H2b, all reforms appear more likely after elections, not just painful increases in salient taxes. Nonetheless, comparing significance levels, there is some evidence for the importance of salience. While the coefficients for decreases before elections are negative for all taxes, they are not significant for salient PIT and VAT taxes. Similarly, while the coefficients for tax increases after elections are positive for all taxes, they are more significant for salient PIT and VAT taxes. Our empirical findings, therefore, are more or less in line with earlier work that indicates the importance of the salience of policies (see, e.g., Bracco et al., 2019; Taubinsky & Rees-Jones, 2018; Veiga & Veiga, 2007).

6 Robustness

In order to verify the reliability of our results, we run robustness checks that account for potential confounding factors, especially those related to the nature of elections. But first we explore in more depth the length of the PBC.

6.1 Cycle length

This paper studies the likelihood of reforms prior to and after elections. The models in Sect. 5 use 6- and 12-month periods, with results reported in Tables 4 and 5. However, given the monthly nature of our data, we are also able to run our model with different intervals. In particular, we considered the periods 1 to 3 months, 4 to 6 months, 7 to 9 months, 10 to 12 months, 13 to 15 months, and 16 to 18 months before and after elections. Table 12 in D.1 shows that reforms are only significantly less likely 1–3, 4–6, and 7–9 months before an election. The significance is strongest for the 1–3-month period before the election. After elections, the 4–6-month period is most significant.

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[11] Note that coefficient magnitudes cannot be directly compared across logit regressions.

[12] Note that the lack of significance for VAT decreases might be due to the limited number of VAT decreases in the sample. Almost half of the countries in our sample introduced VAT after the starting date of our database.

The more granular 3-month increment approach shows which months drive the results for the 6- and 12-month periods in our main analysis. In particular, the pre-electoral halt is strongest right before elections, but it remains significant for the 7–9-month increment. This explains the significance of both the 6- and 12-month periods for the pre-electoral halt. In contrast, for the post-electoral increase, the 4–6-month increment is most significant, explaining why only the 6-month and not the 12-month period was significant in the main analysis.

Another aspect to consider when looking at the length of the PBC is the length of the electoral cycle of each country. A majority of our dataset has a 4- or 5-year cycle. There are two main outliers: Australia has a very short cycle of 3 years, while Mexico has a very long cycle of 6 years. These different cycles might also result in different post- and pre-electoral rushes and slowdowns as suggested by Nordhaus (1975). As can be seen in Online Appendix D.1, excluding the outliers Australia and Mexico yields coefficients that are more significant than in our baseline model, especially for the post-electoral rush. This increases the confidence in the external validity of our main results for countries with typical 4- or 5-year electoral cycles.

6.2 Snap elections

In the 22 countries of our sample, there are three types of political systems: parliamentary, presidential, and assembly-elected president. These three different systems have different formats of elections and choose their executive in different ways. In presidential systems there are legislative and presidential elections. These happen every 4, 5, or 6 years on a predefined date. Unless there is a coup d'état, this is relatively consistent over time. In parliamentary systems, there are legislative elections, and it is the parliament that chooses the head of government in the form of a prime minister. Although most countries have a fixed term, the government coalition can fall or parliament can withdraw its support for the cabinet. As a result, elections need to be called; these are known in the literature as snap elections (see, e.g., Ginsburgh & Michel, 1983). Since they are unplanned, it can be argued that they "catch" politicians, to some extent, by surprise, thus not giving them enough time to push for reforms that might win them an election. In order to account for these types of elections, we reran our main regressions in a subsample composed of only finished cycles. In other words, we dropped incomplete election cycles from the sample.[13]

The results are available in Section D.2 of the Appendix. The overall conclusion is that our results are robust with respect to incomplete electoral cycles due to snap elections. We even see an increase in significance *(p < 0.01)* for the negative likelihood of tax decreases prior to an election.

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[13] In order to do this we use information from the variable *ycurnt* from DPI that reflects how many years are left in the current term, in order to extract the electoral term of each country. From this we generate the dummies called *finterm* and *postfin* that flag incomplete cycles, the snap election that "broke" said cycle, and the months that preceded it. We exclude these months and their corresponding reforms to create a completed cycle subsample.

6.3 Legislative and executive elections only

Given the differences amongst the systems, as a robustness check we run our models using either only legislative or only executive elections to compute the explanatory variables. The advantage of using only legislative elections is that these are present in all democratic systems and hence can be to some extent compared across countries. Using these alternative definitions of elections we find that our results are overall robust. We even see an increase in significance ($p < 0.05$) for the negative likelihood of tax decreases prior to an election. The complete results are in section D.3.

Next we consider only the main executive elections to code our pre-and post-electoral explanatory variables. We consider legislative elections for parliamentary regimes, and presidential elections only for presidential and semi-presidential regimes. The results for the impact of elections on the type and direction of reform are overall robust. However, as can be seen in D.4 the post-electoral effect is less significant than in our baseline model.

6.4 Number of reforms

Our baseline regressions use the likelihood of reform as the dependent variable. As an additional robustness check, we run our models changing the dependent variable to the total number of reforms in a given month. We use a negative binomial regression with fixed effects since this better suits a count dependent variable model. As can be seen in Online Appendix D.5, there are significantly more reforms during the 6 months after an election ($p < 0.05$) and significantly fewer reforms in the 6 and 12 months before elections ($p < 0.01$), in line with our baseline results.

6.5 Major reforms

As mentioned in Sect. 4.1, we observe a total of 888 reform months, that is, months in which there is at least one reform in the dataset. TPRD distinguishes in their original data between minor and major reforms. Intuitively, it would be rather odd to have a single minor reform in one month. This is exactly what happens in the data: there are only 70 months in which there are only minor reforms. In order to see whether our results are still robust when excluding minor reform months, we ran our regressions on a subsample of the data with only major reforms. The results are available in section D.6, and they are overall robust, with some minor loss in significance in, for example, the post-electoral rush 6 months after an election.

6.6 IMF programs in Ireland, Portugal, and Greece

In the aftermath of the 2008 crisis, the European Union and the IMF intervened via adjustment programs in Ireland, Portugal, and Greece. Given these restrictions, it could be the case that governments lost the ability to use fiscal policy strategically to some extent. We account for this through an IMF program dummy. To account for global economic issues not captured by our other economic control variables, this specification also includes year fixed effects. Our results remain robust although there is some loss in significance.

The complete results are in Online Appendix D.7.

6.7 Coalition governments

Independently of the regime in place, on many occasions more than one party needs to form an alliance in order to govern. Since parties might not necessarily align in their intentions, coalition governments could be less likely to announce reforms. We created a dummy called *coalition* that has a value of 1 when a coalition government is in place.[14] The inclusion of this control does not affect our results and the dummy is not significant in any model. The full output is in D.8.

7 Discussion and conclusions

Our results confirm the existence of a pre-electoral slowdown and a post-electoral rush, with tax reforms significantly less likely before and more likely after an election, independently of the direction of reform. The existence of a traditional PBC based on calculating politicians who intend to influence voting behavior by announcing tax reductions prior to elections seems less clear. In particular, we find that tax reductions are *less* likely before elections. This suggests that (also) different mechanisms are at play than those traditionally assumed to cause a PBC.

We hypothesized that capacity constraints before elections can explain the lack of pre-election tax reforms. Incumbent politicians and bureaucracies lack the time or mandate to plan and announce reforms just prior to elections. As a result, there is less administrative or legislative capacity to come up with tax reforms. Our results confirm that there are fewer tax reforms announced in pre-electoral periods, although the lower likelihood was not significant for decreases in salient PIT and VAT. Conversely, after elections, more reforms are announced—and not just more increases in salient taxes that politicians hope to be forgotten by the next election.

In terms of length, the post-electoral rush seems slightly shorter than the pre-electoral halt. Our main specifications tested for 6- and 12-month windows. Before elections, the 12-month window is more significant, and after elections, the 6-month window. A deeper analysis using 3-month increments confirmed that both the pre- and post-electoral effects are concentrated in months relatively close to elections. Future work using monthly data may shed further light on the length and (a)symmetry of the PBC of tax reforms. However, the election date is generally not equal to the date at which the new government takes up its role; this may indeed take months. This clouds our analysis and future work. More precise data on when the new government takes up its role would be needed for a more careful analysis of the timing of pre- and post-electoral effects.

Data availability may limit the external validity of our results. For instance, the TPRD database does not include any African countries. Developed countries are overrepresented, especially within Europe. This results in parliamentary regimes being overrepresented. Because parliamentary regimes are subject to snap elections, the PBC might be less strong

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[14] The dummy *coalition* is based on the *execme*, *gov1me*, and *gov2me* variables from DPI that have the name of the parties in government. We consider a coalition government as one where there are at least two party names.

there. However, our robustness analysis shows that our results hold in a variety of specifications accounting for differences in regime and election type.

Future versions of the TPRD may be extended not only in space but also in time. If the database is extended beyond 2014, future research could cover more recent elections in the analysis.

Future research could also look into the specifics of reforms. Specifically regarding PIT and CIT, the majority of reforms are related to the tax base. Tax base reforms allow politicians to cater to specific groups of voters, for example, through reduced CIT for specific industries like fishing or mining. This strategic interaction cannot be measured with the current data and would need qualitative analysis.

Announcing a tax reform is only the first step. Reforms still need to be implemented and used and their effects need to play out in practice. Future work could investigate the PBC of implementation and of lags between announcements and implementations. This will require careful attention to the fiscal calendars of each country, which we controlled for in our analysis using country-calendar month fixed effects.

Our paper does not consider other possibly relevant factors, such as media influence (as pointed out by, e.g., Prat, 2004; DellaVigna & Gentzkow, 2010; Prat & Stromberg, 2013; Strömberg, 2015; Veiga et al., 2017) and the rationality and financial literacy of voters (as pointed out by, e.g., Fornero & Lo Prete, 2019; Murtinu et al., 2022; Prato & Wolton, 2018). The dataset used for this paper could enable further progress in these areas as well.

In conclusion, we find evidence for the capacity version but not the traditional version of the PBC: tax reforms are less likely to be announced before elections and more likely after elections, independently of whether they are increases or decreases. Our evidence suggests that while a PBC exists, it may be less driven by strategic electioneering than previously assumed. This means that the PBC may be more innocuous than previously assumed.

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