40 years of political budget cycles: What do we know? A Meta-Regression Analysis

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Abstract

Despite a long history of research on political budget cycles, their existence and magnitude are still in question. By conducting a systematic analysis of the existing literature we intend to clarify the debate. Based on data collected from over 1,700 regressions and 58 studies, our meta-analysis suggests that leaders do manipulate fiscal tools in order to be re-elected but to an extent that is significantly exaggerated by scholars. However, we show the incumbents' strategy differ depending on which tools they leverage. Finally, we discuss in further details how authors' methodological choices and country institutions affect political budget cycles.

Keywords: Political cycles, Budget manipulation, Meta-analysis

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1. Introduction

Whether elected leaders use their incumbency advantage to distort policy-making to serve their own interests is a central concern in political economics. In particular, leaders may adopt strategic-timing decisions in a way that help them to hold office. Since Nordhaus (1975), a large -and still increasing- number of papers have scrutinized if and how leaders behave when elections get closer. Despite significant heterogeneity, many studies have shown that incumbents "try to make the year before an election a "happy one" in order to be re-elected" consistently with Paldam (1979)'s expectations. In the present paper we concentrate on fiscal tools' manipulation as most of the political cycles literature has progressively turned to (Shi and Svensson, 2003).

Since cycles are not different from shocks affecting budget, they are likely to hurt the economy. Smoothing the policy-making over the representatives' terms should offer more economic stability and benefit the broader interest. Furthermore, cycles reflect imperfections of institutions and democracy. For these reasons, it is critical to better understand the patterns and mechanisms of electorally-driven manipulation of public accounts. We believe this paper to contribute to this aim and eventually helping to build better institutions fitting the populations' will.

Considering the substantial size of the so-called political budget cycles literature (PbC), researchers have regularly offered some literature reviews (Shi and Svensson, 2003; Eslava, 2011; de Haan and Klomp, 2013).[1] These reviews provide updated overviews and try to draw general conclusions from the various and fragmented pieces of work they put together. They constitute significant milestones of the research on the area, help to synthesize it and finally offer or suggest further developments for scientists. However, literature reviews only provide partial panoramas of the existing literature, that are likely to be biased and distorted towards the authors' ideological positions (Stanley, 2001). Studies not conform to the authors' opinion may be "unintentionally" sidelined or purely disregarded. A second limit of literature reviews resulting of the latter point is the limited information-added they deliver. Literature reviews present and organize researches that have been undertaken so far but do not reveal any additional insight. In other words, classic narrative reviews are not able to resolve nor explain apparent divergences among scholars' empirical findings in a rigorous way.

In a attempt to offer a clearer picture of the vast amount of research on the PbC literature, we provide a meta-regression analysis (MRA). MRA allows to go beyond the two limits of literature

[1] Similar reviews on political business cycles can be found in the literature (Drazen, 2001, among others)

reviews above-mentioned. First, MRA relies on a systematic review of the existing literature. As a consequence, it encompasses all studies produced to date on a specific issue. No matter how well a study matches with the MRA author's ideology, it receives the same weight than the other studies collected. Second, MRA consists in carrying out a statistical analysis of the findings from the collected studies. The regression-based analysis is expected to produce some new insights on the issue in question, especially regarding heterogeneities and discrepancies observed in the literature.

To implement the MRA, we first performed a broad and meticulous investigation of the literature resulting in a selection of 58 papers. All these papers share specific inclusion characteristics.[2] In particular, they all provide a cross-country analysis of how the level of any budget variable (component or total of public revenues, public expenditures, or fiscal surplus) is modified around elections.[3] We then coded the 1,726 regressions extracted from our collection and build our own original dataset summarizing the PbC literature empirical findings. This paper presents the results of their statistical analysis.

The MRA developed suggests that in average leaders do manipulate budget before elections, though at a moderate rate. Our results also reveal this rate is fairly exaggerated in the literature.

The paper is organized as follows. Section 2 provides a brief overview of the literature and its most debated issues. Section 3 describes the data we use for conducting the statistical analysis. Section 5 discusses our approach and the methodology. Section 6 presents the genuine manipulation of political leaders, section 7 display strategic manipulation of researchers and the last section concludes.

2. Theoretical predictions

Meta-analysis offers a toolkit allowing in the best case to solve conflicting theoretical and/or empirical findings on a specific research question.[4] Concerning PbCs, the theory is rather unambiguous. It states that incumbents have an incentive to distort policy-making before elections in order to please and incite voters to renew them at office (Rogoff, 1990).[5] Empirical findings are

[2] see Section 3.1

[3] We refer indifferently to "budget" or "fiscal" variable. So we do for the terms "expenditure" and "spending".

[4] One of the most famous example is on labor market effects of minimum-wage. Contrary to the neo-classic predictions and conventional belief, meta-analyses reveal no significant negative association between minimum-wage and employment (Card and Krueger, 1995; Doucouliagos and Stanley, 2009, for instance).

[5] We do not discuss here the underlying mechanisms, such as competency signaling process, trickery of short-sighted voters or targeting of swing voter groups.

however more contrasting. In particular, the existence and magnitude of such predicted cycles vary according to factors, such as geography and especially institutional settings. In other words, the manifestation of PbC is heterogeneous and conditional (Wittman et al., 2006). de Haan and Klomp (2013) provide an excellent and updated review of conditioning variables examined in the literature, such as the level of development, the quality of institutions, democracy characteristics, and constitutional features.

In addition, some scholars question the effectiveness of such strategic manipulation. Several country-specific studies show that incumbents that resort to PbC have a lower probability to be re-elected. This phenomenon is evidenced by Peltzman (1992) for the United States, Brender (2003) for Israël, Drazen and Eslava (2010) for Colombian mayors, and Brender and Drazen (2007) in a worldwide cross-country study.[6] If voters punish rather than reward incumbents running fiscal expansions before elections, then there should be no point to adopt such strategies.

Arvate et al. (2009) dissect this paradox and explain that strategic manipulation of fiscal tools is more rewarding when voters are less sophisticated and informed. Unsurprisingly PbC are more pronounced in developing countries (Shi and Svensson, 2006), less democratic regimes (Gonzalez, 2002) and new democracies (Brender and Drazen, 2005), where voters are usually less informed and experienced, and where thus manipulation is expected to be more effective.

3. The meta-data

Basically a MRA breaks down in three stages. The first one is the collection of all the relevant studies, that meet objective criteria we define. The second and most time-intensive step is the coding of the estimates encompassed in theses studies resulting in a dataset ready to be exploited. The third and last part is the statistical analysis of this dataset. We discuss the first two steps in this section. Unlike standard empirical studies, MRA do not rely on primary data such as GDP or household income. Rather we must deserve much care and effort to build our own dataset, that is critical to ensure its quality and the consistence of our results.

[6] On the other hand, literature is far from unanimity on the detrimental effects of PbCs on incumbents chances of reelection. Instead, Jones et al. (2012) and Sakurai and Menezes-Filho (2008), among others, find beneficial effects for Argentinean governors and Brazilian mayors respectively.

3.1. How did we select the studies?

The present meta-analysis relies on 1,331 estimates over 1,726 which have partial correlation available, from 58 papers. This selection is the result of an extensive search and the adoption of restrictive requirements.

First of all, we have implemented the search on the most comprehensive electronic search engines, that is: EconLit, Science Direct, Ideas Repec, Springer, Wiley and Google Scholar, by entering the keywords "political budget cycle", "political business cycle", and "electoral cycle" in these bibliographic databases. As some relevant studies may have fallen through the cracks, we undertook a manual complementary search. First, we looked for additional studies in the references listed in the papers already selected. Second, we checked the publications and working papers of the authors identified in the first round.[7] Finally, we made our best to be as exhaustive as possible. If any relevant study was to remain, we believe its omission is not likely to affect our analysis since it relies on a substantial number of estimates and the potential "missed" ones would be randomly omitted. We then refined our selection and only keep studies that are both relevant and allow a consistent statistical analysis.

We only retain empirical cross-sectional papers written in English. A study with no regression-based estimate is discarded *de facto*. This is the case of most theoretical papers and literature reviews. In the empirical papers thus selected, we only retain original estimates[8] that are based on at least two countries. Single-country regressions dig often deeper in the theoretical mechanisms supporting political cycles. In particular, the analysis of economic and political institutions can be much finer, as it is often a real challenge to compare and collect data for comparable variables for different countries. As a result, single-country regressions is likely to bring too much specific estimates to our sample to be consistently compared with other estimates. Moreover, this would have implied to dissect all studies whatever the language in which they are written (Spanish, French, Chinese, Hindi, Russian, and so on). If quantifiable, the amount of work would have been much more considerable than it has already been (and than all MRA require), and risks related to omissions, such as hidden literature on a specific language, significantly higher.

[7] This manual search only revealed few supplementary papers that accredits the effectiveness of the electronic round of the selection.

[8] We do not keep estimates reported or replicated from another source. Multiple-counting of the same regression would artificially inflate its weight, that is, it would bias our results. Actually we did collect estimates reported multiple times. Such cases are scarce, and unsurprisingly do not affect our results. For a matter of relevance, we only present results after having removed multiple-counting.

This methodological choice limits our sample of estimates, which is not a major concern in our case since we rely on 1,726 estimates. To sum up, we believe the potential benefits of incorporating country-specific estimates in our analysis not worth the risks of omission and biases, and the costs in terms of time that it incurs.

As most of the economic science is released in English, we are not likely to omit much relevant studies. For similar reason we only retain estimates from scientific paper-formatted study, whether published or not. We thus omit estimates from books, reports or even theses. Indeed, the latter are less frequently numerically released and accessible, and often results from books are also spread in papers, so that we eventually catch the relevant data they may contain.[9]

Additionally, we only consider papers which focus on how electoral periods affect the level of either national deficits, revenues or expenditures, or a subdivision of one of these three broad fiscal variables. Consequently, estimates whose the dependent variable is some sort of budget composition change index are not considered.[10] As the theory essentially predicts the behavior of leaders before elections and not after, almost all regressions found in the literature focus on pre-electoral cycles. Due to scarcity of both theoretical and empirical research on post-electoral patterns (de Haan and Klomp, 2013), we restrict our attention to estimates of fiscal manipulation during the run-up to elections.

Finally, based on all these criteria of inclusion, we updated and limited our search to studies released before 1^{st} January 2015. On the 1,726 regressions coded, we remove estimates that do not offer the statistical information required by MRA, that is partial correlations and standard errors or t-statistics. We finally end up with a sample of 1,331 estimates.

3.2. Measures of the dependent variable

As stated above, we only retain estimates having a fiscal output as dependent variable, indifferently expressed as level, as a nominal value, as a fraction (of GDP most of the time), as a variation or growth rate. We thus exclude regressions based on budget composition change (Ashworth and Heyndels, 2002; Brender and Drazen, 2013, for instance). Therefore, we neither consider cases where the dependent variable is a ratio of a sub-component of a budget variable

[9] A notable example is the Persson and Tabellini (2003b)'s book, whose the results may be found in companion papers (Persson, 2002; Persson and Tabellini, 2003a)

[10] This is typically the case of Brender and Drazen (2013)'s paper. The information delivered by such a study is meaningful, but we are not able to put together level and composition change indices in a consistent way in our analyses.

on this budget variable. For instance, in some regressions, Katsimi and Sarantides (2012) use the ratio "current (or capital) spending / total spending". In these cases, we observe the variation of the ratio but we are not able to know if this variation results from an electoral manipulation on the numerator, the denominator, or both. As a consequence we cannot know how the level of current (or capital) expenditures is affected by the closeness with elections. Similar cases of composition-related regressions that finally did not enter our sample may be found in Chang (2008), Vergne (2009), or Klomp and de Haan (2013a) for instance. Some papers are using cyclically adjusted measures (Golinelli and Momigliano, 2006; **?**, for instance), but interestingly not any considered study use a discretionary measure of fiscal output, with the exeption of Buti and Van Den Noord (2004).

The literature splits into three budget variables, even if numerous studies compare successively the effect of elections on the three of them. A first set of estimates we code focuses on expenditure patterns. Most of them take the level of public expenditures divided by GDP (276 estimates on 1,331). However, we find seven other finer measures based on sub-components of expenditures, namely current, capital, broad, local and final consumption expenditures. All these five measures are generally expressed as a share of GDP. Exceptions are Potrafke (2010) and Klomp and de Haan (2013a) that use *per capita* for health and agricultural expenditures respectively. Voters are supposed to be more sensitive to current rather capital expenditures as their effects are more tangible in the short-term. Therefore leaders should be more prompt to increase current spending in pre-election period. According to the electoral system prevailing, leaders may also privilege manipulating broad/welfare spending or finer/local expenditures targeting specific groups such as swing voters as a strategic tool.

A second set of estimates assess how leaders manipulate public revenues according to electoral periods. Once again, the authors' favorite variable is the aggregate of all public revenues on GDP (196 estimates on 1,331). We include estimates using 13 other measures, that are sub-components of overall revenues, and are adjusted to the GDP. These alternative variables are essentially specific kind of taxes, that are likely to be more easily or effectively manipulate by leaders. We do not intend to be exhaustive and provide overwhelming details on these measures and the related studies here, but some descriptive statistics are summarized in Table 9 and 10.

The third set contains estimates of how elections impact national budget surpluses, which are obtained by subtracting public expenditures to public revenues. Numbers of studies focus on deficit rather than surplus. In this case, we multiply estimate values by -1 so that the last

set contains only estimates of elections effect on budget surplus on GDP.

Finally, 914 estimates on 1,331 use one of the three main variables, that is nominal value of either surplus, expenditures or revenues on GDP. The number of paper focusing on one category of fiscal ouptut is relatively limited (except when considering fiscal surpluses, see Table 9 for more details). Unsurprisingly, analysis from the whole sample (1,331 estimates) or a reduced sample (914 estimates) yield similar results.

3.3. Measures of election variables

Authors have multiplied the ways of taking account of electoral manipulations. In particular, their challenge is to catch electoral periods in an accurate and relevant manner (Akhmedov and Zhuravskaya, 2004). To do so they develop and compute electoral period variables of various forms. The most common is a dummy taking one in years during which an election occurs, or alternatively the year before it takes place.[11] In order to better capture leaders' behavior during the year preceding elections, scholars have offered various adjustments to this "electoral year dummy", such as coding one pre-electoral years rather than electoral years when the ballot occurs in the first x months of the civil year (Shi and Svensson, 2006), or by distinguishing elections according to which period of the year they occur (Brender and Drazen, 2005; Mink and de Haan, 2006).[12] Another class of refinements is pioneered by Franzese (2000). With this method the electoral variable is intended to measure how much of a given year may actually been considered as pre-electoral. Considering an election taking place during the mth month of year, the electoral variable equals $\frac{m}{12}$ the electoral year, and $\frac{12-m}{12}$ the year before. Alternative measures derived from the generic presented presented here may be found in the literature. But beyond the measure, scholars also question the nature of elections.

For instance, Klomp and de Haan (2013a,b,d) remove anticipated elections and focus explicitly on pre-determined ones in order to avoid endogeneity issues related to the timing of elections. Another concern is which elections to consider. Usually two kinds of elections are of national importance, namely legislative and executive ones. Facing the arbitrariness of the choice, some authors such as Fatás and Mihov (2003) do not distinguish and pay attention to

[11] Even though most authors are interested by pre-election periods, some studies focus on post-electoral years and may use interest variable in the form of a binary variable equaling one in years following a civil year during which a ballot has occured (Block, 2002; Persson and Tabellini, 2003b; Alt and Lassen, 2006; Ebeke and Ölçer, 2013, among others).

[12] Most of the time, these techniques are employed as robustness or sensitivity tests.

all elections, with the risk of a high frequency of elections and the lack of relevant focus. Other papers focus on one given kind of elections considered by the authors as more meaningful for all the countries of their sample (Hagen, 2007, for instance).[13] Yet, according to the constitutional design of countries, one kind or the other may exert greater forces on the policy-making and thus may be more relevant to the issue of political cycles. A last group of authors chose to use what is considered as the highest election according to the country (Shi and Svensson, 2006, for instance): legislative elections for parliamentary countries and executive elections for presidential regimes.

4. A first glance

Because they offer a first answer at the economic question raised at a glance, funnel graphs have become very popular in MRA. In our framework, a funnel graph consists in plotting the estimates of election effect on fiscal aggregates collected in the literature (horizontal axis) against a measure of the precision of these estimates (vertical axis). Most of the time, precision is measured by the inverse of the estimate standard errors (1/SE). In other words, funnel plots provide an illustration of how the estimates are distributed. Most of the estimates lie at the bottom of the graph. They are by definition not precise and they vary across a wide range of estimate values. Moving to the top, the more precise, the more concentrated around a value are the estimates. This value is supposed to reflect the "true" genuine effect of election on fiscal manipulation. If the distribution is centered on zero, we should conclude that elections have no effect on such manipulation. A second information we may infer from such graphs is potential selection bias in the literature. In the absence of such a bias, points should be symmetrically distributed around this "true" effect. Any skewness suggests selection bias towards the direction where inclines the distribution.

For the funnel we design, partial correlations are preferred to regression coefficients that are sensitive to measures and scales of election and fiscal variables. To ensure comparability across the estimates, we convert the coefficients collected into partial correlations. We compute partial correlations such as:

$$r = \frac{t}{\sqrt{t + df}} \tag{1}$$

[13] Others adopt mixed strategies by retaining only presidential regimes and focusing on executive elections (Block, 2002; Hanusch and Vaaler, 2013), or parliamentary regimes and consider legislative elections (Bayar and Smeets, 2009).

where t is the t-statistic and df the degrees of freedom of each estimate collected. If the sample size is almost always reported by authors, it is rarely the case for degrees of freedom. Fortunately, partial correlation are weakly sensitive to imprecise degrees of freedom calculations (Stanley and Doucouliagos, 2012); this uncertainty is especially marginal as sample sizes in the PbC literature state in hundreds even thousands of observations. This standardization removes the economic meaning of effects but still informs on the magnitude and direction of associations between election and fiscal manipulation and makes them quantitatively comparable. By way of robustness, we also compute the widely used Fisher's partial correlation transformations.[14]

Figure 1 reports the partial correlations on the right panel and Fisher's partial correlations on the left panel of the effect of election on public spending from the 535 regressions coded. Consistently with the theory, the "true" value of the manipulation regarding public expenditures, suggested by the top of the distribution is positive yet close to zero. Moreover, if most estimates reveal a positive manipulation, this is partly explain by a clear skewed right distribution. Similar conclusions can be drawn from the funnel graph of correlations between elections and public revenues (Figure 2). The distribution is not symmetrical and inclines to the left suggesting a selection bias in favor of results reporting a reduction of public revenues in pre-electoral periods. However, the "true" value of manipulation on this aggregate is here less unambiguously on the left of the 0-line. This may translate a lesser ability of leaders to modify tax rates and/or a weak sensibility of voters regarding revenues cuts. Since leaders tend to increase spending and slightly reduce revenues when elections get close, figure 3 reveals a degradation of the budget surplus preceding ballots. Selection bias appears once again distinctly. Figures 4 offer a global view on leaders' manipulation over fiscal tools. In this figure we recode the correlations of revenues and surplus by multiplying them by -1, so that we can combine the three previous graphs into one combined funnel graph in a consistent manner. In this graph, any positive correlation suggests a strategic vote-catching manipulation of fiscal tools by leaders. However, a simple graphical analysis may be misleading and a more rigorous statistical analysis must be implemented for more robust and quantified conclusions. We achieve this in the next two sections. The first one is dedicated to the budget manipulation by political leaders, and the second one focuses on how researchers manipulate their results. Each section begins by estimating manipulation and then examines its sources.

[14] We apply the formula: $z = \frac{1}{2} ln \frac{1+r}{1-r}$. See Stanley and Doucouliagos (2012) for a discussion.

Figure 1: Funnel plot of election-on-spending partial correlation ($n = 535$)

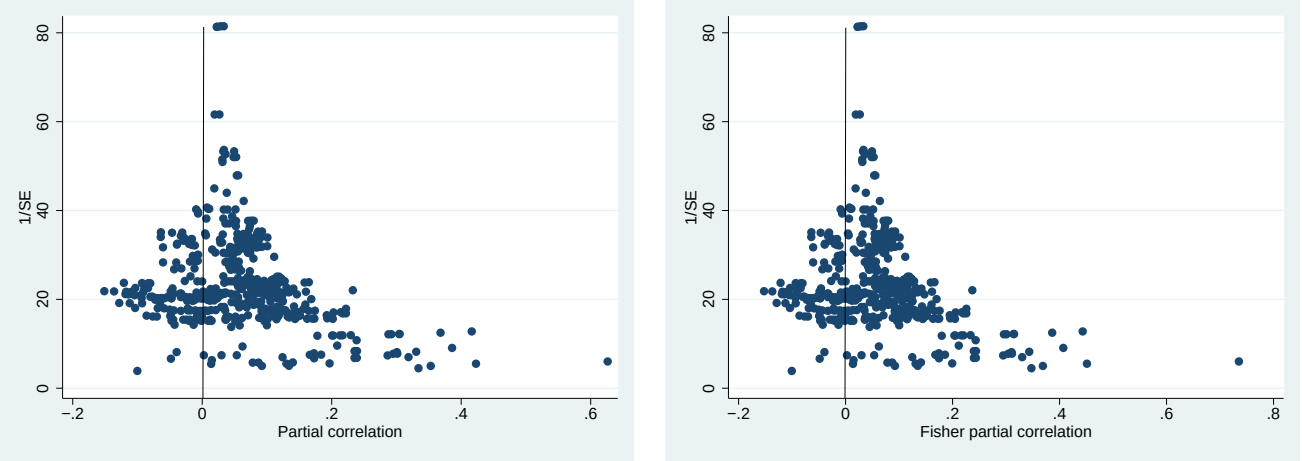


Figure 2: Funnel plot of election-on-revenues partial correlation ($n = 354$)

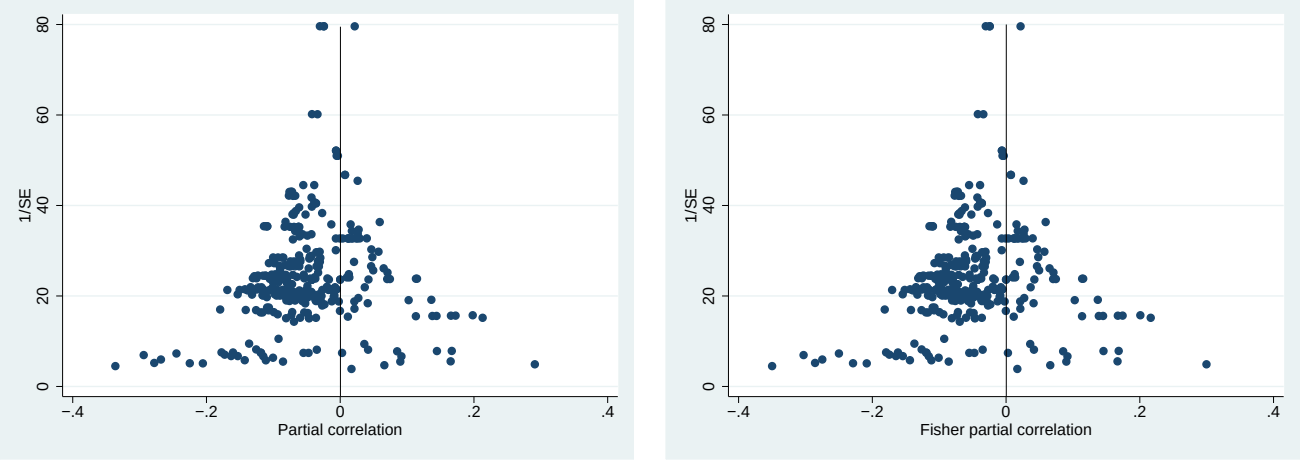


Figure 3: Funnel plot of election-on-fiscal surplus partial correlation ($n = 442$)

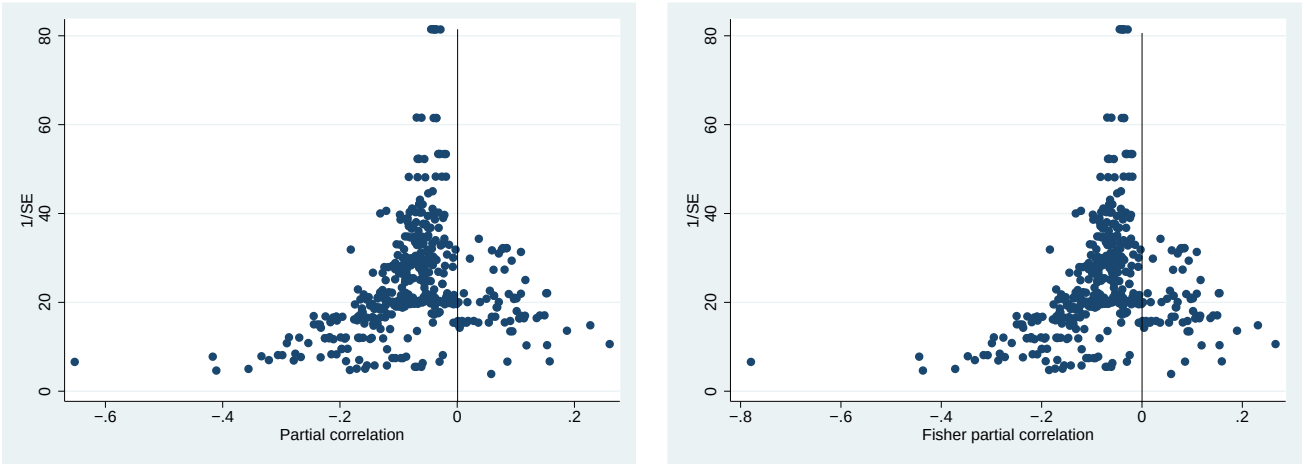
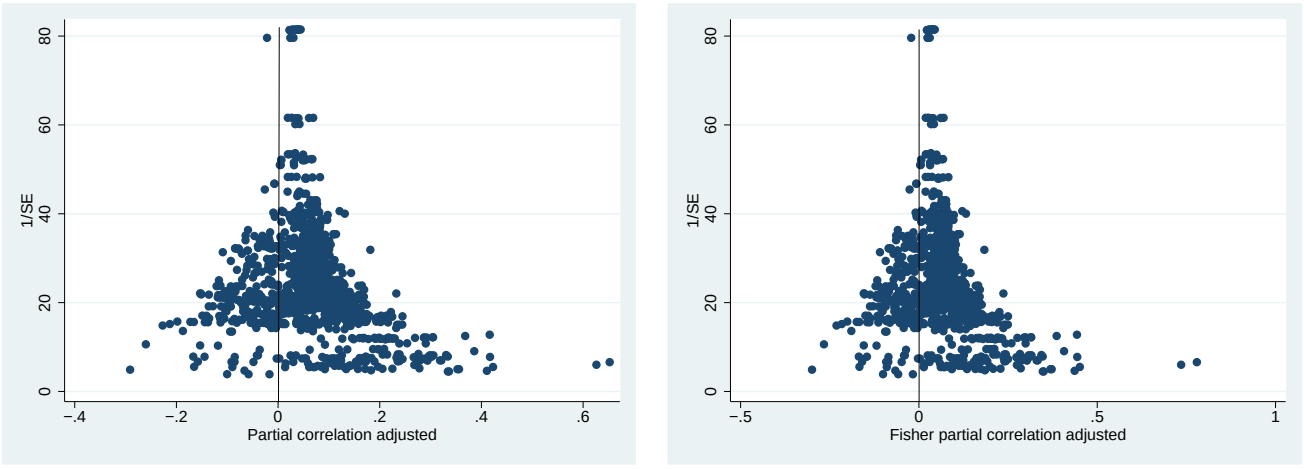


Figure 4: Funnel plot of election-on-fiscal output partial correlation ($n = 1,331$; $skewness = 0.31$)



5. MRA methodology

To obtain more rigorous insights from the dataset we built, we let the data speak by turning to a standard model of simple meta-regression. Basically, it consists in regressing the partial correlations between elections and budget variables on a constant and its standard errors:

$$r_{ij} = \beta_0 + \beta_1 SE_{ij} + \varepsilon_{ij}, \tag{2}$$

where r and SE denote the i^{th} computed partial correlation and standard error from study j and ε are the residuals. Through the estimation of β_1 and β_0, such a model allows to test respectively for funnel-asymmetry and precision-effect. Conventional statistic theory assumes independence between the magnitude of estimated effects and its standard errors. Any significant association, reflected by a β_1 statistically different from 0, would reveal a tendency to favor estimates with a certain t-statistic, likely exceeding the standard threshold of statistical significance.[15] Such a tendency is acknowledged as publication selection. Its causes are numerous and deeply anchored to academic incentives for scholars (Stanley and Doucouliagos, 2012), that proceed to sampling and specification searching and eventually select results that best fit their ideology or conventional belief and offer greater opportunities of publication. In the case of PbCs, we suspect that authors may favor results reporting incumbents' strategic behavior and thus we expect some positive and significant association between the magnitude of manipulation and its standard error, as suggested by the right-skewed funnel graph in 4.[16] If so, the literature would be biased and the effect of elections on budget distortions overestimated.

Since equation (2) captures and thus controls for potential publication selection, its constant, β_0 reveals the genuine effect of elections if any. This effect is not anymore inflated or distorted by researchers' selection. Determining this genuine effect is a critical task of MRA as conflicting results in a literature fail to do so. In our case, it should reveal if and how much leaders are likely to manipulate fiscal tools to boost their reelection prospects. However, a simple meta-regression may not be fully satisfactory. As reviewed by de Haan and Klomp (2013), scholars' disagreement does not lie in the existence of PbCs anymore but rather in the conditions of its

[15] Card and Krueger (1995) show that the t-statistics of studies assessing the effect of minimum-wage on employment gravitate around 2, approximation of the statistical significance at the usual 5% confidence level.

[16] When strategic behavior is supposed to lead to a deterioration of an aggregate, such as budget balance or public revenues, the funnel is likely to be left-skewed and β_1 negative.

existence and magnitude. To examine the conditional nature of PbCs inherent to all socio-economic phenomenon, we then turn to multiple meta-regressions. We increase equation (2) by adding a vector Z of k covariates:

$$r_{ij} = \beta_0 + \sum_{k=1}^{K} \beta_k \mathbf{Z}_{ki} + \beta_1 SE_{ij} + \varepsilon_{ij}, \tag{3}$$

These additional covariates allow to assess how PbCs differ across countries and over time, and how authors' methodological choices affect them. The list of explanatory variables used in the study is provided in Table 10. We organize them in nine categories: measure of fiscal output; measure of the election variable; adjustments on the election variable; estimator applied; sample composition; decades and regions included; publication outlet and data specification.

6. Manipulation of leaders

6.1. PET results

We estimate equation (2) and present results in Tables 1 and 2. This standard MRA regression is acknowledged as FAT-PET, that stands for Funnel-Asymmetry (β_0) and Precision-Effect (β_1) tests. In Table 1, we combine all fiscal outputs to observe leaders' manipulation in general. Panel (i) reports the results on all observations available. In panel (ii) we exclude observations dealing with subcomponents of revenues and spending. In other words we remove cases of what the literature refers as "pork barrel" (Drazen and Eslava, 2006) to focus on manipulation of the broad fiscal outputs.[17] Finally, panels (iii) and (iv) excludes conditional PbC, captured with interactive models or sub-sampling, from regressions. In the first three rows of each panel we employ weighted least squares (WLS) using precision squared as the weight. Precision squared is the inverse variance, which produces "optimal" weights in meta-analysis (Hedges and Olkin, 1985). By tackling heteroskedasticity issue, WLS are suitable to MRA and routinely employed by researchers (Stanley and Doucouliagos, 2012). WLS do not treat all observations equally and assign more weight to estimates that are reported more precisely. We then replicate each regression: by clustering on studies, by double clustering on studies and fiscal output, and finally employ robust regression to control for the effects of potential outliers.

[17] "Pork barrel" is often assimilated to targeted infrastructure projects, but it can also refers to pre-electoral increase in some current expenditures, such as agriculutural subsidies.

As the introduction of the variable "standard error" (SE) in the econometric model captures potential selection bias, the constant (β_0) reveals the genuine effect of how leaders manipulate budget in the run-up period to elections. Table 1 shows that this coefficient ranges from 0.022 to 0.032. The association is rather limited but strongly significant and impels us to conclude that leaders do use budget tools to increase their popularity before elections, thus creating PbC. This strategy is not illegal *per se* but consists in fooling short-sighted or non-informed voters in the short-run to serve leaders' own self interest at the cost of a smooth and more sustained policy benefiting the broad interest. Such a political strategy thus deviates from the ideal of democracy, and leaders employing it are likely to make as discreetly as possible. As corruption for instance, manipulation of budget is typically a hidden phenomenon. Given the very nature of such phenomenon, finding any evidence of it, even small in magnitude, is to be considered carefully both for economic efficiency and institutional quality reasons. Based on the heterogeneous existing estimations to date and once removed any selection bias, it appears that on average there is still a statistically robust effect of manipulation. This first result answers the debate around the existence of PbC, but is mute when it turns to explain what are their favored sources.

6.2. Where does the PbC come from?

Table 2 displays the FAT-PET results for each group of fiscal output selected as dependent variable. We report results for a broad measure of spending encompassing all types of expenditures (panel (i)), and a narrow measure of spending by excluding subcomponents of total spending (panel (ii)). We apply the same logic to revenues in panels (iii) and (iv). Finally panel (v) reports results when fiscal surpluses are used to capture fiscal cycles. Again, the results show strong evidence of publication selection bias. Confirming theoretical assumptions, the effect of elections is positive for spending and negative for revenues and fiscal surpluses. Interestingly, we do not find strong evidence of pre-electoral manipulation on revenues and spending (panel (i) to panel (iv)), but we do find a statistically significant and robust manipulation on fiscal surplus. This may suggest heterogeneity in the strategies employed by leaders. According to the political easiness and pay-off that leaders face, they may favor to manipulate rather spending or revenues. In different contexts, leaders maximize their re-election prospects by adopting a spending-strategy or a revenue-strategy, or even a mixed strategy by manipulating both aggregates. If the strategy choice is not clear because context-dependent, what is clear is that

the primary balance systematically deteriorates before elections. The pre-electoral deficit rise translate the opportunism of leaders even if the tools it relies on differ.[18]

To assess even more finely the behavior of leaders, we offer to go one step further and look inside each box: expenditures and revenues. Table 3 reports the estimated effect of electoral manipulation by disaggregating the different fiscal tools. Columns (1) and (2) present results when revenues are used, and columns (3) and (4) when spending are used. In column (2) and (4) we use the fixed effects multilevel (FEML) estimator that includes dummy variable for individual authors to take into account unobserved heterogeneity among authors in the PbC literature, with less bias than random weighted average (Stanley, 2008; Stanley and Doucouliagos, 2012). In every case, total revenues and total spending are used as reference categories. For each column we estimate equation 3, by just adding the fiscal tools as covariates.[19]

Few papers study the composition of fiscal manipulation at national level, relative to studies at municipality level. Regarding revenues, seminal contributions of Ashworth and Heyndels (2002), Efthyvoulou (2012), Katsimi and Sarantides (2012), focus on OECD countries, while Block (2002) and Ehrhart (2013) study manipulation of revenues composition in developing countries. We split revenues into direct taxes (income taxes, payroll taxes, property taxes), external taxes (international trade taxes), indirect taxes (value added tax (VAT), general sales tax (GST)) and non-tax revenues (social security revenues, goods and services revenues, government borrowing). The MRA results are unconclusive about the strategic use of specific revenues category by political leaders during electoral race.

So, as recalled by the caricature from the *Miami Herald* (1986), reported by *The Economist* August 1^{st}, 1987 and Alesina et al. (1989), politicians leaders may prefer avoiding tax reforms before election to keep social order away from demonstrations and strikes. In column 2, there is small evidence of non-tax revenues reduction in pre-electoral period. Unfortunately, none of the paper on PbC at national level study the political behavior regarding natural rents revenues around elections, the most strategical non-tax windfall in some countries, and particularly in oil-exporting countries (Arezki and Ismail, 2013). In line with Ehrhart (2013), coefficient of *External Taxes* and *Indirect Taxes* are negative, suggesting strategic manipulation on these specific tax components of revenues, but insignificant at meta level.

[18] These results echo back the work of Barberia and Avelino (2011) on Latin America countries.
[19] Results are fully robust when we reestimate columns (1) to (4) by using the full model of Table 7.

Regarding manipulation of spending composition, some articles focus on the opposition between capital and current spending (Block, 2002; Schuknecht, 2000; Block et al., 2003; Vergne, 2009; Efthyvoulou, 2012; Katsimi and Sarantides, 2012; Combes et al., 2015, among others), while other papers distinguish local public good spending from broad public good spending (Schuknecht, 2000; Persson and Tabellini, 2003a; Chang, 2008; Potrafke, 2010; Enkelmann and Leibrecht, 2013; Klomp and de Haan, 2013a, among others). So, we adopt a similar methodology, by splitting capital spending from current spending and broad public goods from local public goods. The MRA report clear spending shifts towards current spending and away from capital spending. The findings are in accordance with Katsimi and Sarantides (2012) for OECD countries and with Vergne (2009) for developing countries. They also suggest that leaders reduce expenditures where the benefits are not strong enough in the short-run and reallocate the amount thus "saved" to expenditures categories that offer them a greater and immediate political pay-off.

We here rediscover evidence of composition effects acknowledged by Vergne (2009) for instance. Facing a budget constraint, leaders appear to make it softer in pre-electoral periods. As the result, the primary balance deteriorates and we observe PbCs. But the elasticity of budget constraint has its limits. One of these is that making the PbC too important or perceptible is likely to be punish by voters (Brender and Drazen, 2007). A way of bypassing budget constraint is then to manipulate the composition of public spending. Leaders appear thus to manipulate both level and composition - at least of expenditures - of budget. It offers leaders two strategies they may use as complement or substitute according to their power extent over the policy-making and the political reward they expect from each strategy.

Finally, in a way, this result reinforces the care we must deserve to the manipulation observed on the global level of budget aggregates as it is only one way leaders may distort the policy-making of that policy instruments. In columns 5 and 6 we reestimate our model on regressions of Ashworth and Heyndels (2002) and Brender and Drazen (2013) focused on total revenue composition and total spending composition, respectively. As the number of studies and observations is extremely restrain, we remain cautious on interpretation of results; notice however a significant and positive shift in spending composition in pre-electoral period, whereas the reverse is true for revenues, which is consistent with all our previous findings.

In the same line, the MRA results find preferences on manipulation of broad public good spending in pre-electoral period. Some broad public goods, such as welfare spending, have a

large component of current spending, while some local public goods are mainly constitute by infrastructure spending.[20] In addition, political leaders may prefer giving satisfaction for a whole sociological voters' category rather than geographically targeted voters, to ensure strong electoral basis before elections, since we consider broad public good spending as a *"[...] type of expenditure that benefits broad groups in the population and is difficult to target towards narrow geographic constituencies."* (Persson and Tabellini, 2003a, p. 4). To summarize, national political leaders have incentives to allocate the cost of investment in current spending and increase broad public good spending before elections. It is not conflicting with findings in literature on higher capital spending and local public goods in pre-electoral period at municipality level, where voters' preferences are much more targetable (Khemani, 2004; Eslava, 2005, for the case of Indian states and Colombian municipalities, respectively). Also, favoring targeted groups such as swing voters may be less easily detectable in the data.

7. Manipulation of researchers

In this section, we examine whether and how much the estimations of PbC found in the literature are distorted by researchers or the current publication process, and thus are distant from its genuine effect we identify above. In a way, we intend to measure how much researchers do manipulate the level of leaders' manipulation they report.

7.1. FAT results

The first column of Tables 1 and 2 display a coefficient associated to selection bias ranging in absolute value from 0.617 to 0.985. These coefficients are strongly significant and robust in a statistical way. It highlights that the estimations of PbC found in the literature are in average substantially exaggerated. Identifying the precise reasons of this overestimation is a difficult task, may rely on various factors and vary across studies and authors. However we intend to examine some causes of heterogeneity in the results observed in the literature and thus potential trails explaining such a selection bias.

[20] The dichotomy is no longer relevant with other examples, such as public agricultural spending, constituted either by capital and current spending and considered as broad public goods in most developing countries and local public goods in developed countries (de Haan and Klomp, 2013; Klomp and de Haan, 2013a).

7.2. Sources of selection

To do so, we turn to the multivariate MRA model described by equation (3). This model allows to observe the causes of heterogeneous findings on PbC in the empirical literature. In particular, this section intends to improve our understanding of what determinants condition the existence and magnitude of PbC.

The first four columns of Tables 4, 5, 6, and 7 present the results for the whole sample with the use of the adjusted partial correlation. All the columns but column (2) report the general model with all moderators, following seminal literature on meta-analysis in economics (Askarov and Doucouliagos, 2013; Costa-Font et al., 2014; de Linde Leonard et al., 2014, among others), estimated with WLS weighted by precision and clustered by studies.

Column (3) control for authors' fixed effects with the FEML estimator. As the number of clusters relative to the number of MRA moderators is small (Askarov and Doucouliagos, 2013) we use the FEML estimator by double clustering standard errors on studies and fiscal output, in column (4).[21] In contrast, column (2) employs the general-to-specific methodology, whereby MRA explanatory variables which are not significant at 10% level in column (1) are removed from the estimation in order to have a parsimonious model (Stanley and Doucouliagos, 2012). As FEML estimator is considered as the most exhaustive, it's our benchmark when interpreting results.

In every case our chosen core variables are the *SE*, regional dummies and decade dummies to capture spatial and temporal patterns on PbC, according to the flourished literature. Irrespective to the considered moderator vector introduced, our results reveal a significant selection bias that disappears when controlling for authors' fixed effects. So, we have to explore further sources of strategic manipulations for researchers. Additionally, larger PbC are found in Latin America, in Eastern Europe and Central Asia, and during the 1980's, suggesting spatial and temporal patterns for PbCs but also potential manipulation of researchers in data collection process, to favor significant results.

The MRA models reported in Table 4 incorporate several key variables related to publication itself to explain the heterogeneity of the results. Published papers on the *Public Choice* report smaller PbC, despite editorial boarding focused on decision-makers' strategic behavior.[22]

[21] 23 distinct measures of fiscal output are reported in the 58 studies included in our analysis.

[22] See the definition of "Public Choice", by William F. Shugart II in the Concise Encyclopedia of Economics. Notice that finding is no longer significant when reestimate our model after removing subcomponents of revenues

Unpublished papers tends to find lesser PbC (columns 3 and 4). In a sense it's a publication bias, since papers finding significant PbC have greater chances to be eventually published. In a same line, publications with higher impact factors finds a significantly larger pre-electoral manipulation of fiscal policy (columns 3 and 4).

The MRA models reported in Table 5 displays the variation of results coming from the data heterogeneity among studies. PbC magnitude can be related to the nature of fiscal output and to its adjustments. Indeed, strategic manipulation is found to be significantly reinforced when using data from central government, once taken into account author's unobservable heterogeneity (columns 3 and 4). Adjustments on electoral calendar, executive elections and predetermined elections can be a serious issue in the PbC literature. Among adjustments on elections, relying only on the highest level of national election, is associated with less magnitude in PbC, after controlling for unobserved heterogeneity (columns 3 and 4). Econometric methodology makes a difference. Using dynamic panel estimator leads to more severe PbCs (columns 1-4). One possible explanation is the use and abuse of GMM estimators to find convincing insights on PbCs. Conversely, correction of standard errors for heteroskedasticity and autocorrelation contributes to decrease magnitude of PbCs (columns 1-4). Sample composition also matters. Merging all bad case scenarii for PbC returns a negative and statistically significant coefficient for PbCs when ruling the FEML estimator (columns 3-4).[23] Hence, authors have strong incentives to adjust samples in order to get a "good" result. Finally, behavior on electoral manipulation does not differ when the dependent variable refers alternatively to fiscal surpluses, revenues or spending, as *YSurplus* coefficient underlines (columns 1-4).

In Table 6 we classify interaction models according to the review of de Haan and Klomp (2013). Interactive models may appropriately capture conditional PbC by taking into account different PbC regimes in a same regression. However, meta-analysis study the stability of the effect of electoral manipulation and the use of interaction terms can be viewed as an attempt to impose a structure on the data that ultimately leads to a misspecification bias (Doucouliagos and Paldam, 2013). The MRA coefficients merely quantify that bias. Indeed, the *Elect* ×

and spending, suggesting that it is mainly driven by "pork barrel" manipulation.

[23] Bad case senarii are theoretical conditions under which the existence of PbC is less likely; namely it corresponds to samples isolating: developed countries, established and/or strong democracies, high debt ratio, macroeconomic bad times, strong crowding out effect, high institutional quality, high transparency, high social capital, high trade oriented economies, currency unions, fixed regime rate countries, fiscal ruler countries, low polarized governments or low elections contestability.

Leveldemoc, *Elect* × *Agedemoc*, *Elect* × *Constit*, *Elect* × *Tranparency*, *Elect* × *Dvpt*, *Elect* × *Fiscalrule* and *Elect* × *Checks* terms are strictly variables that capture specification differences, rather than representing a genuine empirical effect. All interactive terms have positive coefficient, except the *Elect* × *Age democracy* term, which contribute to reduce the PET by 0.013, and the *Elect* × *Dvpt* term which is unsignificant at meta level (columns 1-4). Regarding other model specifications, we report MRA results of the basic set of controls defined by Shi and Svensson (2006), partisan measure and time dummies. Lagged dependent fiscal variable have a positive and statistically significant coefficient with FEML estimator (columns 3-4). Again, dynamic panel structure leads to larger PbC. Same conclusions can be drawn when adding time dummies in empirical specifications (columns 3-4).

Finally, while selection bias in PbC literature is an interesting finding, the fundamental issue is whether there exists heterogenous patterns in genuine empirical effect, which can be eventually used by researchers in data collection process. Region dummies and decade dummies are the only variables in the MRA that can be interpreted as estimating heterogeneity in the size of the genunine empirical effect (Doucouliagos and Paldam, 2013).

Location is an important factor of the electoral manipulation. Irrespective to the moderator vector introduced or when the full specification is displayed (Table 7), Latin America, and Eastern Europe and Central Asia countries experiment larger budget manipulation in pre-electoral period. Yet small according to Cohen (1988) guidelines, it should not be neglected, in particular in those countries, which have a long-lasting history of banking crises, sovereign debt crises, and hyperinflation episodes (Reinhart and Rogoff, 2009; Reinhart, 2010). In addition, in the ideal of democracy, strategic manipulation of citizens by elected leaders is expected to be absolutely zero. Moreover, there is a consensus about the detrimental effect of electoral manipulation on social welfare, contrary to other fields such as the public development aid (Askarov and Doucouliagos, 2013), and as recalled by Nelson and Kang (1981) and Nelson and Plosser (1982) short-run shocks may have long-run consequences on economic output.

Similarly to Western Europe, "Neo Europes" and Japan, these two regions have an established practice of electoral race; but their political history explains their lack of fiscal discipline.[24] Contrary to the Western Europe, "Neo Europes", or Japan, Eastern Europe and Central Asia

[24]"Neo Europes" refers to Australia, Canada, New Zealand and United States, according to Crosby (1986) and Acemoglu et al. (2001).

countries do not have a balance between the three components of political order designed by Fukuyama (2011) but either a weak state (the Hungarian case), either a strong and capable state, without any subordination to a rule of law (the Russian and Ukrainian case). Latin America and Caribbean countries have inherited from the Spanish and French patrimonialism of the XVI^{th} century, which was detrimental for the fiscal sustainability, until today (Fukuyama, 2011). So, researchers may have incentives to adjust consequentely their sample composition, in order to provide substantial results. It could also explain the significant number of studies focusing on one of these regions (Maurel, 2006; Klašnja, 2008; Parra and Santiso, 2009; Stanova, 2009; Barberia and Avelino, 2011; Efthyvoulou, 2012; **?**, among others).

The MRA results for the decade dummies also provide clear patterns of PbC over time. Electoral manipulation of PbC is significantly larger if data from the 1980's is added to a sample. With a coefficient higher than 0.05 in Table 7 it has a sizable impact on the variation of fiscal stance and fiscal composition in pre-electoral period (columns 3 and 4). This is hardly surprising, since most countries with electoral agenda in 1980s had less transparency on fiscal matters and less fiscal institutions to prevent deficit bias (Alesina and Perotti, 1994), were younger democracies[25] and known a structural rising unemployment after two decades of full employment. As recalled by Blanchard and Summers (1986) p.15:

"[...] Most of Western Europe has since the early 1970s suffered a protracted period of high and rising unemployment. In the United Kingdom unemployment peaked at 3.3 percent over the period 1945-1970, but has risen almost continuously since 1970, and now [in 1986] *stands at over 12 percent. For the Common Market nations as a whole, the unemployment rate more than doubled between 1970 and 1980 and has doubled since again."*

So, political leaders had possibilities and strong incentives to manipulate fiscal and monetary policy for re-election motives, especially since theories on hysteresis of unemployment (Blanchard and Summers, 1986) and time inconsistency (Kydland and Prescott, 1977; Barro and Gordon, 1983) were not yet fully incorporated to policy implications. Consequentely, researchers could also exploit this temporal pattern to their own advantage, when display heterogenous magnitude of PbCs over time.

[25] According to the Polity2 threshold > 0, Greece, Portugal and Spain, three developed European countries, are democratic only until 1974 (Greece) and 1975 (Portugal and Spain).

8. Conclusion

Initiated by Nordhaus (1975), the PbC literature is still flourishing as empirical findings are not unanimous on the existence and magnitude of such cycles. A couple of narrative reviews help to understand how the literature is structured and what are the main conditions affecting the strategic manipulation of budget by political leaders in pre-electoral period. We go one step further by offering a statistical and systematic analysis of all PbC-related academic papers with the intention to identify the main sources of variability observed in the literature and obtain robust and reliable statistical information on the genuine effects of election on fiscal tools' distortion.

We conduct our analysis on the 1,331 estimates of PbC collected from 58 cross-country studies. The MRA reveals a significant selection bias from scholars translating an inclination to exaggerate the magnitude of PbC. However, once controlled for this overestimation, we still find a slight but statistically robust proof of manipulation of budget by leaders. If necessary, this confirms the opportunistic nature of leaders and the need to strengthen political and economic institutions in order to increase accountability and get closer to the ideal of democracy.

Interestingly, we show that the deterioration of primary balance before elections is systematic, but evidence of public revenues and spending manipulation are slightly less robust. We attribute this finding to leaders adopting a rather revenues or spending-led according to the political costs-benefits trade-off they face. By disaggregating public spending, we find that leaders are more prone to manipulate some subcomponents, such as increasing current relative to capital expenditures but also broad public goods in pre-electoral period, consistently with the theory. On the other hand, incumbents do not systematically target specific revenues subcomponents when they adopt a tax cut strategy in order to maximize their reelection prospects.

In addition, we realize a sensitivity analysis assessing how model specifications and methodological choices adopted by authors may affect PbC estimates. Finally, the evidence of strategic manipulation we observe on fiscal aggregate levels is very likely to be magnified by composition manipulation (Rogoff, 1990; Ashworth and Heyndels, 2002; Brender and Drazen, 2013). Once again, this impels scientists to carry on with research on political cycles and the way to limit them in order to make democracy more effective.

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Table 1: Estimates of the FAT-PET MRA [Basic Results]

Regression/s.e.	(1) FAT Funnel asymmetry		(2) PET Meta-average		N
	(i) All Observations (Adjusted Partial Correlation)				
	Double publication removed				
Robust s.e.	**0.661*** **	(0.085)	**0.024*** **	(0.003)	1,331
Clustered s.e.	**0.661** **	(0.250)	**0.024*** **	(0.007)	1,331
Double clustered s.e.	**0.661*** **	(0.232)	**0.024*** **	(0.007)	1,331
Robust regression	**0.767*** **	(0.091)	**0.025*** **	(0.003)	1,331
	(ii) Excluding "Pork-barrel" (Adjusted Partial Correlation)				
Robust s.e.	**0.723*** **	(0.091)	**0.022*** **	(0.003)	914
Clustered s.e.	**0.723*** **	(0.229)	**0.022*** **	(0.006)	914
Double clustered s.e.	**0.723*** **	(0.259)	**0.022*** **	(0.008)	914
Robust regression	**0.779*** **	(0.094)	**0.024*** **	(0.003)	914
	(iii) Exluding Interactions (Adjusted Partial Correlation)				
Robust s.e.	**0.666*** **	(0.089)	**0.022*** **	(0.003)	1,037
Clustered s.e.	**0.666*** **	(0.226)	**0.022*** **	(0.005)	1,037
Double clustered s.e.	**0.666*** **	(0.234)	**0.022*** **	(0.008)	1,037
Robust regression	**0.796*** **	(0.097)	**0.024*** **	(0.004)	1,037
	(iv) Exluding Subsample (Adjusted Partial Correlation)				
Robust s.e.	**0.619*** **	(0.157)	**0.029*** **	(0.006)	583
Clustered s.e.	0.619	(0.448)	**0.029** **	(0.013)	583
Double clustered s.e.	**0.619***	(0.350)	**0.029** **	(0.012)	583
Robust regression	**0.699*** **	(0.166)	**0.032*** **	(0.006)	583

Notes: Panel (i) reports all observations. Panel (ii) excludes subcomponents of revenues and spending. Panel (iii) excludes interactive models. Panel (iv) excludes subsamples. The first 3 rows of each panels uses the weighted least squares (WLS), with precision squared (inverse variance) used as weights. Clustering on studies, or double clustering on studies and fiscal output. $*p < 0.10, * * p < 0.05, * * *p < 0.01$.

Table 2: Estimates of the FAT-PET MRA [Fiscal Output]

Regression/s.e.	(1) FAT Funnel asymmetry		(2) PET Meta-average		N
	(i) Spending				
Robust s.e.	**0.776*** **	(0.133)	**0.014*** **	(0.005)	535
Clustered s.e.	**0.776***	(0.401)	0.014	(0.011)	535
Double clustered s.e.	**0.776****	(0.352)	0.014	(0.010)	535
Robust regression	**0.793*** **	(0.166)	**0.016****	(0.006)	535
	(ii) Restrictive Measure of Spending				
Robust s.e.	**0.657*** **	(0.158)	**0.013****	(0.005)	276
Clustered s.e.	0.657	(0.448)	0.013	(0.013)	276
Robust regression	**0.587*** **	(0.180)	**0.016****	(0.007)	276
	(iii) Revenues				
Robust s.e.	**-0.617*** **	(0.178)	**-0.021*** **	(0.007)	354
Clustered s.e.	**-0.617***	(0.347)	-0.021	(0.015)	354
Double clustered s.e.	**-0.617***	(0.356)	-0.021	(0.015)	354
Robust regression	**-0.692*** **	(0.157)	**-0.026*** **	(0.006)	354
	(iv) Restrictive Measure of Revenues				
Robust s.e.	**-0.883*** **	(0.166)	-0.011	(0.007)	196
Clustered s.e.	**-0.883****	(0.307)	-0.011	(0.012)	196
Robust regression	**-0.792*** **	(0.121)	**-0.023*** **	(0.004)	196
	(v) Fiscal surplus				
Robust s.e.	**-0.704*** **	(0.136)	**-0.032*** **	(0.004)	442
Clustered s.e.	**-0.704***	(0.414)	**-0.032*** **	(0.008)	442
Robust regression	**-0.985*** **	(0.139)	**-0.029*** **	(0.005)	442

Notes: See Table 1. The dependent variable is the non adjusted partial correlation between elections and fiscal output. Panel (i) reports observations on spending. Panel (ii) excludes subcomponents of spending. Panel (iii) reports observations on revenues. Panel (iv) excludes subcomponents of revenues. Panel (v) reports observations on fiscal surpluses. $*p < 0.10, * * p < 0.05, * * *p < 0.01$.

Table 3: MRA estimates of PbC [Source of Manipulation]

Variables	Revenues		Spending		Budget Composition	
	WLS	FEML	WLS	FEML	WLS	WLS
Direct Taxes	-0.004	0.007	-	-	-	-
External Taxes	-0.006	-0.019	-	-	-	-
Indirect Taxes	-0.012	-0.003	-	-	-	-
Non Tax Revenues	0.073	**-0.040***	-	-	-	-
Current Spending	-	-	**0.040*****	**0.032*****	-	-
Capital Spending	-	-	**-0.088*****	**-0.097*****	-	-
Broad Public Good	-	-	**0.039****	**0.048*****	-	-
Local Public Good	-	-	**0.040*****	0.016	-	-
Spending Composition	-	-	-	-	**0.102***	-
Revenues Composition	-	-	-	-	-	**-0.102***
Missing Category	Total revenues		Total Spending		Revenues Composition	Spending Composition
RMSE	0.048	0.039	0.049	0.038	0.011	0.011
Adjusted R^2	0.095	0.649	0.317	0.735	0.922	0.922
Number of cluster	42	42	58	58	2	2
SE included	Yes	Yes	Yes	Yes	Yes	Yes
Authors fixed effect	No	Yes	No	Yes	No	Yes
N	354	354	535	535	26	26

Notes: See Table 1. Dependent variable: non adjusted partial correlation. All columns are estimated with WLS (precision weights) and double clustered standard errors. Author's dummies are included in Fixed Effects Multi Level (FEML) estimator. Estimations on Budget composition are ruling using Ashworth and Heyndels (2002) and Brender and Drazen (2013) estimates. Cohen (1988)'s guidelines: small= less than 0.10; medium= 0.30; large= 0.50. $*p < 0.10, **p < 0.05, ***p < 0.01$.

Table 4: Multiple FAT-PET Models [Part I]

Variable	All Observations Adjusted Partial Correlation			
	General	Specific	FEML	FEML double cluster
			Publication bias	
Standard error	**0.556** **(0.254)**	**0.444** **(0.207)**	0.366 (0.485)	0.366 (0.361)
			Publications	
Public Choice	0.016 (0.013)		**-0.021*** **(0.011)**	**-0.021**** **(0.009)**
Unpublished	-0.004 (0.008)		**-0.056***** **(0.008)**	**-0.056***** **(0.009)**
Impact Factor	0.000 (0.000)		**0.001***** **(0.000)**	**0.001***** **(0.000)**
			Time and region	
1980s	0.006 (0.015)		**0.048**** **(0.022)**	**0.048*** **(0.027)**
1990s	**-0.097***** **(0.033)**	**-0.096**** **(0.038)**	-0.048 (0.046)	-0.048 (0.031)
Recent	0.010 (0.013)		0.009 (0.008)	0.009 (0.010)
Eastern Europe & Central Asia	**0.030***** **(0.007)**	**0.023***** **(0.006)**	**0.023***** **(0.007)**	**0.023**** **(0.009)**
Latin America & Caribbean	0.011 (0.011)		**0.011*** **(0.006)**	**0.011*** **(0.007)**
Middle-east and North Africa	0.000 (0.011)		0.004 (0.005)	0.004 (0.007)
South Asia & Pacific	0.008 (0.010)		0.005 (0.011)	0.005 (0.008)
Sub-saharan Africa	**-0.024*** **(0.012)**	**-0.014**** **(0.007)**	-0.012 (0.015)	-0.012 (0.013)
Global	-0.009 (0.014)		0.001 (0.016)	0.001 (0.012)
Constant (Western & Japan)	**0.097**** **(0.039)**	**0.124***** **(0.040)**	**-0.016*** **(0.010)**	**-0.016**** **(0.007)**
RMSE	0.096	0.054	0.048	0.048
Adjusted R^2	0.087	0.076	0.564	0.564
Number of cluster	57	57	57	147
Number of covariates	14	5	14	14
Authors fixed effect	No	No	Yes	Yes
N	1,331	1,331	1,331	1,331

Notes: See Table 1. Estimation using WLS, with precision weights. Columns 3 and 4 includes authors fixed effects (not reported) and dummy for Western and Japan. Standard errors clustered by studies in parenthesis. Column 4 to 6 uses double cluster of standard errors on studies and fiscal output. Adjusted R^2 is not strictly comparable across the different models. $*p < 0.10, **p < 0.05, ***p < 0.01$.

Table 5: Multiple FAT-PET Models [Part II]

Variable	All Observations Adjusted Partial Correlation			
	General	Specific	FEML	FEML double cluster
			Publication bias	
Standard error	**0.737****	**0.628***	0.250	0.250
	(0.340)	**(0.344)**	(0.529)	(0.446)
			Data heterogeneity	
Samplesize	**-0.000***	**-0.000***	**-0.000*****	-0.000
	(0.000)	**(0.000)**	**(0.000)**	(0.000)
Infrannual	**-0.018**		-0.001	-0.001
	(0.011)		(0.011)	(0.029)
YSurplus	0.012		0.009	0.009
	(0.011)		(0.015)	(0.014)
YVar	-0.015		0.000	0.000
	(0.013)		(0.009)	(0.005)
YCycl	**-0.028***	**-0.025***	-0.002	-0.002
	(0.016)	**(0.013)**	(0.015)	(0.018)
YCentral	**-0.023****	**-0.021*****	**0.088*****	**0.088*****
	(0.009)	**(0.007)**	**(0.011)**	**(0.017)**
ElectDum	0.005		0.003	0.003
	(0.010)		(0.007)	(0.005)
ElectRat	0.004		0.001	0.001
	(0.013)		(0.006)	(0.008)
Adjust. Calendar	0.004		0.001	0.001
	(0.007)		(0.007)	(0.008)
Adjust. Highest	**0.016***	0.011	**-0.066*****	**-0.066*****
	(0.008)	(0.008)	**(0.005)**	**(0.004)**
Adjust. Exog	-0.009		-0.011	-0.011
	(0.008)		(0.009)	(0.007)
EconDynamic	**0.016****	**0.017****	**0.008***	**0.008****
	(0.006)	**(0.006)**	**(0.004)**	**(0.004)**
SE Correction	**-0.015****	**-0.013***	**-0.014*****	**-0.014*****
	(0.007)	**(0.007)**	**(0.004)**	**(0.005)**
ConstitSamp	-0.014		0.002	0.002
	(0.014)		(0.004)	(0.004)
WorstSamp	-0.007		**-0.011****	**-0.011***
	(0.008)		**(0.005)**	**(0.006)**
			Time and region	
1980s	**0.032****	**0.025***	**0.058*****	**0.058****
	(0.013)	**(0.014)**	**(0.021)**	**(0.025)**
1990s	**-0.080***	**-0.087****	-0.053	**-0.053***
	(0.041)	**(0.038)**	(0.048)	**(0.031)**
Recent	0.006		**0.013****	0.013
	(0.012)		**(0.006)**	(0.011)
Eastern Europe & Central Asia	**0.032*****	**0.028*****	**0.026*****	**0.026*****
	(0.006)	**(0.008)**	**(0.005)**	**(0.009)**
Latin America & Caribbean	**0.013***	**0.014***	**0.018*****	**0.018***
	(0.007)	**(0.007)**	**(0.004)**	**(0.010)**
Middle-east and North Africa	-0.011		0.006	0.006
	(0.010)		(0.006)	(0.008)
South Asia & Pacific	0.007		-0.003	-0.003
	(0.011)		(0.010)	(0.007)
Sub-saharan Africa	**-0.029*****	**-0.030*****	-0.021	-0.021
	(0.010)	**(0.006)**	(0.015)	(0.016)
Global	-0.015		0.018	0.018
	(0.013)		(0.015)	(0.012)
Constant (Western & Japan)	0.081	**0.090***	-0.013	**-0.013***
	(0.057)	**(0.052)**	(0.008)	**(0.007)**
RMSE	0.052	0.053	0.048	0.048
Adjusted R^2	0.132	0.107	0.571	0.571
Number of cluster	57	57	57	147
Number of covariates	26	13	26	26
Authors fixed effect	No	No	Yes	Yes
N	1,331	1,331	1,331	1,331

Notes: See Table 4.

Table 6: Multiple FAT-PET Models [Part III]

Variable	All Observations Adjusted Partial Correlation			
	General	Specific	FEML	FEML double cluster
			Publication bias	
Standard error	**0.637****	**0.468****	0.342	0.342
	(0.307)	**(0.225)**	(0.484)	(0.364)
			Specifications	
Election × Leveldemoc	0.007		**-0.027*****	-0.027
	(0.012)		**(0.004)**	(0.061)
Election × Agedemoc	-0.013		**-0.013*****	**-0.013****
	(0.009)		**(0.003)**	**(0.006)**
Election × Constit	**0.042*****	**0.040*****	**0.027*****	**0.027*****
	(0.014)	**(0.011)**	**(0.007)**	**(0.007)**
Election × Transparency	**0.041****	**0.030****	**0.020*****	**0.020*****
	(0.018)	**(0.013)**	**(0.006)**	**(0.005)**
Election × Dvpt	-0.007		-0.016	-0.016
	(0.015)		(0.010)	(0.010)
Election × Fiscalrule	-0.011		**0.048****	**0.048*****
	(0.038)		**(0.023)**	**(0.014)**
Election × Check	0.007		**0.019*****	**0.019*****
	(0.011)		**(0.007)**	**(0.006)**
Lagged Y	0.019		**0.017*****	**0.017*****
	(0.013)		**(0.005)**	**(0.005)**
GDPpc.	0.002		-0.003	-0.003
	(0.011)		(0.010)	(0.010)
Growth	0.006		-0.005	-0.005
	(0.007)		(0.004)	(0.007)
Partisan	0.021		-0.005	-0.005
	(0.015)		(0.008)	(0.008)
Time	0.003		**0.010***	**0.010***
	(0.008)		**(0.005)**	**(0.005)**
			Time and region	
1980s	0.004		**0.049****	**0.049***
	(0.014)		**(0.021)**	**(0.026)**
1990s	**-0.089*****	**-0.104****	-0.040	-0.040
	(0.031)	**(0.041)**	(0.043)	(0.030)
Recent	0.006		0.009	0.009
	(0.010)		(0.009)	(0.011)
Eastern Europe & Central Asia	**0.022*****	**0.012****	**0.024*****	**0.024****
	(0.007)	**(0.005)**	**(0.007)**	**(0.009)**
Latin America & Caribbean	0.012		**0.011***	**0.011***
	(0.008)		**(0.006)**	**(0.007)**
Middle-east and North Africa	0.002		0.002	0.002
	(0.011)		(0.006)	(0.008)
South Asia & Pacific	-0.001		0.006	0.006
	(0.010)		(0.011)	(0.008)
Sub-saharan Africa	-0.018		-0.012	-0.012
	(0.011)		(0.015)	(0.013)
Global	-0.005		0.000	0.000
	(0.011)		(0.015)	(0.011)
Constant (Western & Japan)	**0.070***	**0.127*****	**-0.017***	**-0.017****
	(0.041)	**(0.042)**	**(0.008)**	**(0.007)**
RMSE	0.053	0.054	0.048	0.048
Adjusted R^2	0.101	0.074	0.567	0.567
Number of cluster	57	57	57	147
Number of covariates	23	6	23	23
Authors fixed effect	No	No	Yes	Yes
N	1,331	1,331	1,331	1,331

Notes: See Table 4.

Table 7: Multiple FAT-PET Models [FULL]

Variable	All Observations Adjusted Partial Correlation General	Specific	FEML	FEML double cluster
		Publication bias		
Standard error	**0.865*** **	**0.650*** **	0.232	0.232
	(0.324)	**(0.225)**	(0.555)	(0.466)
		Publications		
Public Choice	**0.022***	**0.023** **	-0.004	-0.004
	(0.012)	**(0.009)**	(0.005)	(0.008)
Unpublished	0.010		**-0.173** **	**-0.173** **
	(0.011)		**(0.069)**	**(0.080)**
Impact Factor	0.000		**0.002***	**0.002***
	(0.000)		**(0.001)**	**(0.001)**
		Data heterogeneity		
Samplesize	**-0.000***	-0.000	**-0.000*** **	-0.000
	(0.000)	(0.000)	**(0.000)**	(0.000)
Infrannual	-0.006		-0.000	-0.000
	(0.009)		(0.012)	(0.029)
YSurplus	0.011		0.010	0.010
	(0.012)		(0.016)	(0.014)
YVar	-0.014		0.002	0.002
	(0.014)		(0.008)	(0.005)
YCycl	-0.031		-0.001	-0.001
	(0.021)		(0.015)	(0.018)
YCentral	-0.010		**0.089*** **	**0.089*** **
	(0.012)		**(0.010)**	**(0.017)**
ElectDum	0.004		0.003	0.003
	(0.009)		(0.005)	(0.005)
ElectRat	-0.009		-0.000	-0.000
	(0.011)		(0.005)	(0.007)
Adjust. Calendar	0.008		-0.002	-0.002
	(0.009)		(0.010)	(0.009)
Adjust. Highest	0.018		0.006	0.006
	(0.012)		(0.072)	(0.081)
Adjust. Exog	**-0.017** **	**-0.025*** **	-0.009	-0.009
	(0.007)	**(0.007)**	(0.009)	(0.006)
EconDynamic	**0.010** **	0.008	0.002	0.002
	(0.005)	(0.007)	(0.004)	(0.004)
SE Correction	-0.012		**-0.009** **	**-0.009***
	(0.008)		**(0.004)**	**(0.005)**
ConstitSamp	-0.009		0.002	0.002
	(0.013)		(0.003)	(0.003)
WorstSamp	-0.009		**-0.011** **	**-0.011***
	(0.009)		**(0.005)**	**(0.006)**
		Specifications		
Election × Leveldemoc	0.016		-0.133	-0.133
	(0.015)		(0.091)	(0.088)
Election × Agedemoc	**-0.012** **	**-0.024*** **	**-0.013*** **	**-0.013** **
	(0.005)	**(0.008)**	**(0.003)**	**(0.007)**
Election × Constit	**0.049** **	**0.048** **	**0.029*** **	**0.029*** **
	(0.021)	**(0.020)**	**(0.009)**	**(0.009)**
Election × Transparency	0.020		**0.018*** **	**0.018*** **
	(0.018)		**(0.006)**	**(0.006)**
Election × Dvpt	-0.008		**-0.018***	**-0.018***
	(0.016)		**(0.010)**	**(0.010)**
Election × Fiscalrule	-0.006		**0.046***	**0.046*** **
	(0.038)		**(0.023)**	**(0.014)**
Election × Check	0.009		**0.018*** **	**0.018** **
	(0.010)		**(0.007)**	**(0.007)**
Lagged Y	**0.026** **	**0.023** **	0.012	0.012
	(0.012)	**(0.011)**	(0.008)	(0.007)
GDPpc.	0.005		-0.011	-0.011
	(0.010)		(0.010)	(0.008)
Growth	-0.002		0.002	0.002
	(0.007)		(0.003)	(0.003)
Partisan	**0.045** **	**0.039*** **	-0.009	-0.009
	(0.017)	**(0.012)**	(0.009)	(0.010)
Time	0.007		0.007+	0.007
	(0.008)		(0.004)	(0.005)
		Time and region		
1980s	**0.023***	0.010	**0.057*** **	**0.057** **
	(0.012)	(0.012)	**(0.019)**	**(0.024)**
1990s	**-0.069*** **	**-0.081*** **	-0.045	-0.045
	(0.025)	**(0.027)**	(0.045)	(0.030)
Recent	0.008		**0.012** **	0.012
	(0.010)		**(0.005)**	(0.010)
Eastern Europe & Central Asia	**0.033*** **	**0.030*** **	**0.027*** **	**0.027*** **
	(0.006)	**(0.006)**	**(0.005)**	**(0.009)**
Latin America & Caribbean	**0.019*** **	**0.022** **	**0.018*** **	**0.018***
	(0.007)	**(0.009)**	**(0.004)**	**(0.010)**
Middle-east and North Africa	-0.005		0.001	0.001
	(0.009)		(0.005)	(0.007)
South Asia & Pacific	0.009		-0.003	-0.003
	(0.010)		(0.009)	(0.007)
Sub-saharan Africa	**-0.038*** **	**-0.029*** **	-0.019	-0.019
	(0.012)	**(0.008)**	(0.014)	(0.015)
Global	-0.012		0.015	0.015
	(0.011)		(0.014)	(0.011)
Constant (Western & Japan)	0.014	**0.057***	-0.010	-0.010
	(0.041)	**(0.034)**	(0.008)	(0.007)
RMSE	0.051	0.052	0.047	0.047
Adjusted R^2	0.163	0.131	0.574	0.574
Number of cluster	57	57	57	147
Number of covariates	41	15	41	41
Authors fixed effect	No	No	Yes	Yes
N	1,331	1,331	1,331	1,331

Notes: See Table 4.

Table 8: List of studies

	Author(s)		Author(s)		Author(s)
1	Afonso (2008)	21	Franzese (2000)	41	Mosley and Chiripanhura (2012)
2	Alesina et al. (1992)	22	Galeotti and Salford (2001)	42	Morozumi et al. (2014)[b]
3	Alesina et al. (1993)	23	Golinelli and Momigliano (2006)	43	Mourão (2011)
4	Alesina et al. (2006)	24	Hagen (2007)	44	Parra and Santiso (2009)
5	Alt and Lassen (2006)	25	Hallerberg et al. (2002)	45	Nyblade and O'Mahony (2014)
6	Ashworth and Heyndels (2002)	26	Hanusch (2012)	46	Persson and Tabellini (2003a)
7	Barberia and Avelino (2011)	27	Hanusch and Vaaler (2013)	47	Potrafke (2007)
8	Bayar and Smeets (2009)	28	Hanusch and Keefer (2014)	48	Potrafke (2010)
9	Block (2002)	29	Jong-A-Pin et al. (2012)	49	Troeger and Schneider (2012)
10	Block et al. (2003)	30	Kaplan and Thomsson (2014)	50	Schuknecht (1996)
11	Bove et al. (2014)	31	Katsimi and Sarantides (2012)	51	Schuknecht (2000)
12	Brender and Drazen (2003)	32	Klašnja (2008)	52	Shelton (2014)
13	Brender and Drazen (2005)	33	Klomp and de Haan (2013b)	53	Shi and Svensson (2006)
14	Buti and Van Den Noord (2004)	34	Klomp and de Haan (2013a)	54	Stanova (2009)
15	Combes et al. (2015)[a]	35	Klomp and de Haan (2013d)	55	Streb et al. (2009)
16	Costa-Fernandes and Mota (2013)	36	Klomp and de Haan (2013c)	56	Streb et al. (2012)
17	Dreher and Vaubel (2004)	37	Kouvavas (2013)	57	Tujula and Wolswijk (2007)
18	Ebeke and Ölçer (2013)	38	Kraemer (1997)	58	Wright (2011)
19	Efthyvoulou (2012)	39	Maurel (2006)		
20	Ehrhart (2013)	40	Mink and de Haan (2006)		

Notes: [a] As the publication date is after december 31^{th} 2014, we take into account the working paper version (Combes et al., 2013). [b] We don't consider regressions from Table 2 to Table 5 in Morozumi et al. (2014) due to lack of information on reference category for elections.

Table 9: Descriptive Statistics

	Nb of estimates	Nb of papers	Nb of papers focus
Spending	535	33	9
Restrictive	276	24	2
Sub-components	259	20	7
Revenues	354	24	2
Restrictive	196	15	0
Sub-components	158	12	2
Surplus	442	47	22

Notes: Column 1 indicates the number of regressions using a given category of fiscal output. Column 2 indicates the number of paper using a given category of fiscal output. Column 3 indicates the number of paper using exclusively a given category of fiscal output.

Table 10: Variable definitions

No.	Variables	Variable Description (BD for binary dummy)	N	Mean	S.D.	Min	Max
1	Adjustedpartial	Partial correlation (adjusted for revenues and fiscal surplus)	1,331	0.06	0.09	-0.29	0.65
2	Partial	Partial correlation (non adjusted for revenues and fiscal surplus)	1,331	-0.01	0.11	-0.65	0.62
3	SE	Standard error of the correlation	1,331	0.05	0.03	0.01	0.26
		Group 1 : Measures of Cycle					
4	YSur	BD if used fiscal surplus (or inverse of deficit) over GDP	1,726	0.35	0.48	0	1
5	YSpen	BD if used total expenditure over GDP	1,726	0.19	0.39	0	1
6	YRev	BD if used total revenues over GDP	1,726	0.14	0.35	0	1
7	YSpen bis	BD if used total (or subcomponents) expenditure over GDP, in level, or per capita	1,726	0.40	0.49	0	1
8	YRev bis	BD if used total (or subcomponents) revenues over GDP	1,726	0.25	0.44	0	1
9	YVar	BD if dependent variable is in first difference or growth rate	1,726	0.13	0.34	0	1
10	YCycl	BD if dependent variable is cyclically adjusted[a]	1,726	0.06	0.23	0	1
11	YCentral	BD if dependent variable explicitely refers to central government	1,726	0.59	0.49	0	1
		Group 2: Measures of Election					
11	ElectDum	BD if elections are captured by electoral dummies	1,726	0.77	0.42	0	1
12	ElectRat	BD if Elections are captured by ratio a la Franzese	1,726	0.19	0.39	0	1
13	ElectOth	BD if Elections are captured by other methods (used as the base)	1,726	0.04	0.20	0	1
		Group 3: Adjustment on Election					
14	Adjust. Calendar	BD if adjustment for electoral or fiscal calendar	1,726	0.18	0.39	0	1
15	Adjust. Highest	BD if adjustment on executive election	1,726	0.50	0.50	0	1
16	Adjust. Exog	BD if adjustment on predetermined election	1,726	0.33	0.47	0	1
		Group 4: Estimator					
17	EconDynamic	BD if used dynamic panel estimator	1,726	0.33	0.47	0	1
18	EconOther	BD if used other estimator (used as the base)	1,726	0.67	0.47	0	1
19	SE correction	BD if used SE correction for heteroskedasticity or autocorrelation	1,726	0.50	0.50	0	1
		Group 5: Sample					
20	SampConstit	BD if subsample on specific constitutional forms	1,726	0.26	0.44	0	1
21	SampWorst	BD if subsample on all worst-case senarios for PBC	1,726	0.86	0.34	0	1
		Group 6: Decades					
22	Elder	BD if data for the 50's, 60's or 70's (used as the base)	1,726	0.63	0.48	0	1
23	1980s	BD if data for the 80's	1,726	0.79	0.41	0	1
24	1990s	BD if data for the 90's	1,726	0.97	0.18	0	1
25	Recent	BD if data for the 00's and 10's	1,726	0.86	0.35	0	1
		Group 7: Region					
26	Wej	BD if Western Europe, neo Europes and Japan were included in samples (used as the base)	1,726	0.73	0.44	0	1
27	Eeac	BD if countries from Eastern Europe or Central Asia were included in samples	1,726	0.55	0.50	0	1
28	Lac	BD if countries from Latin America & Caribbean were included in samples	1,726	0.62	0.49	0	1
29	Mena	BD if countries from Middle-east & North Africa were included in samples	1,726	0.49	0.50	0	1
30	Sap	BD if countries from South Asia and Pacific were included in samples (Japan is excluded)	1,726	0.51	0.50	0	1
31	Ssa	BD if countries from Sub-saharan Africa were included in samples	1,726	0.49	0.50	0	1
32	Global	BD if at least two regions were included in samples	1,726	0.62	0.48	0	1
		Group 8: Publications outlet					
33	Public Choice	BD if article is published on Public Choice	1,726	0.11	0.32	0	1
34	Unpublished	BD for unpublished paper	1,726	0.49	0.50	0	1
35	Impact Factor	Google Scholar (5 years) impact factor of publication	1,726	25.92	27.20	0	168
		Group 9: Data Specification					
36	Samplesize	Number of observations included in the sample	1,726	719.20	779.59	15	6631
37	Infra	BD if infra annual data used	1,726	0.05	0.22	0	1
38	Election × Leveldemoc	BD for election interacted with level of democracy	1,726	0.04	0.20	0	1
39	Election × Agedemoc	BD for election interacted with age of democracy	1,726	0.06	0.23	0	1
40	Election × Constit	BD for election interacted with constitution	1,726	0.09	0.28	0	1
41	Election × Tansparency	BD for election interacted with transparency (including fiscal transparency and mass-media freedom)	1,726	0.04	0.20	0	1
42	Election × Dvpt	BD for election interacted with the level of development	1,726	0.01	0.07	0	1
43	Election × Fiscalrule	BD for election interacted with fiscal rule	1,726	0.01	0.07	0	1
44	Election × Check	BD for other election interacted terms (mainly checks and balances and institutional quality)	1,726	0.16	0.37	0	1
45	LaggedY	BD for lagged value of dependent variable	1,726	0.91	0.28	0	1
46	GDPpc.	BD for per capita GDP	1,726	0.76	0.43	0	1
47	Growth	BD for economic growth	1,726	0.34	0.47	0	1
48	Partisan	BD for partisan measure (such as political ideology)	1,726	0.13	0.33	0	1
49	Time	BD for time dummies or time trend	1,726	0.56	0.50	0	1

Notes: [a] We include the discretionary measures of Buti and Van Den Noord (2004) in that category.