Political Budget Cycles: Manipulation of Leaders or Bias from Research? A Meta-Regression Analysis

Antoine Cazals, Pierre Mandon

CENTRE D'ETUDES
ET DE RECHERCHES
SUR LE DEVELOPPEMENT
INTERNATIONAL

SÉRIE ÉTUDES ET DOCUMENTS

Political Budget Cycles: Manipulation of Leaders or Bias from Research? A Meta-Regression Analysis

Antoine Cazals

Pierre Mandon

Études et Documents n° 33

December 2015

To cite this document:

Cazals A., Mandon P. (2015) "Political Budget Cycles : Manipulation of Leaders or Bias from Research ? A Meta-Regression Analysis ", *Études et Documents,* n° 33, CERDI.
http://cerdi.org/production/show/id/1766/type_production_id/1

CERDI
65 BD. F. MITTERRAND
63000 CLERMONT FERRAND – FRANCE
TEL. + 33 4 73 17 74 00
FAX + 33 4 73 17 74 28
www.cerdi.org

The authors

Antoine Cazals
PhD Student in Economics
CERDI – Clermont Université, Université d'Auvergne, UMR CNRS 6587, 63009 Clermont-Ferrand, France
Email : antoine.cazals@gmail.fr

Pierre Mandon
PhD Student in Economics
CERDI – Clermont Université, Université d'Auvergne, UMR CNRS 6587, 63009 Clermont-Ferrand, France
Email : pierre.mandon@udamail.fr

This work was supported by the LABEX IDGM+ (ANR-10-LABX-14-01) within the program "Investissements d'Avenir" operated by the French National Research Agency (ANR).

Études et Documents are available online at: http://www.cerdi.org/ed

Director of Publication: Vianney Dequiedt
Editor: Catherine Araujo Bonjean
Publisher: Mariannick Cornec
ISSN: 2114 - 7957

Disclaimer:

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.

JEL codes

C82, D72, D78, E62, H0

Acknowledgment

We are very grateful towards Chris Doucouliagos, T.D. Stanley, Martin Paldam, Thomáš Havránek, Patrice Laroche, and conference participants to the 9th MAER-Net Colloquium in Prague, for valuable comments and suggestions.

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 studies 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 built 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 4 provide preliminary evidences. Section 5 discusses our approach and the methodology. Sections 6 to 9 presents the MRA results 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 however more contrasting. In particular, the existence and magnitude of such predicted cycles

[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.

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)[7] 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.

[7] Gonzalez (2002) find higher PbC during the democratization process of Mexico, i.e. when Mexico had a weak democratic framework.

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.[8] 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[9] 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.

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

[9] 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.[10]

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.[11] 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 minimal 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 in 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

[10] 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)

[11] 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) among others. Some papers are using cyclically adjusted measures (Golinelli and Momigliano, 2006; Stanova, 2009; Mourão, 2011, 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 agriculture 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 assesses 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 manipulated 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 A.11.

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.[12] 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).[13] Another class of refinements is pioneered by Franzese Jr. (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

[12] 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, 2003a; Alt and Lassen, 2006; Ebeke and Ölçer, 2013, among others).

[13] 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).[14] 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 governments and executive elections for presidential governments.

4. A first glance

Concerning the magnitude of the leaders' manipulation, a chronological ordering of the mean estimate from each paper of our collection provided in Figure 1 reveals a clear declining trend in time. Earlier papers report stronger budget manipulation than newer studies. The decline is strong since mean estimate is divided by more than two during the last 25 years. This result may have two main sources, or combination of both. First, the budget manipulation may have declined over time, and newer papers, using more recent data logically report weaker economic effects. Second, researchers may have been less prone to inflate the magnitude of the effects they report. In other words, we know the reported effects of pre-electoral budget manipulation have declined over time, but we are not sure who deserves the credit. Does this translate more trustworthiness of leaders or researchers? Before turning to a more rigorous statistical and quantified analysis, graphical tools can still offer us additional information on these two insights.

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

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

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. Computed skewness suggest in Figure 5 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:

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

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.[15]

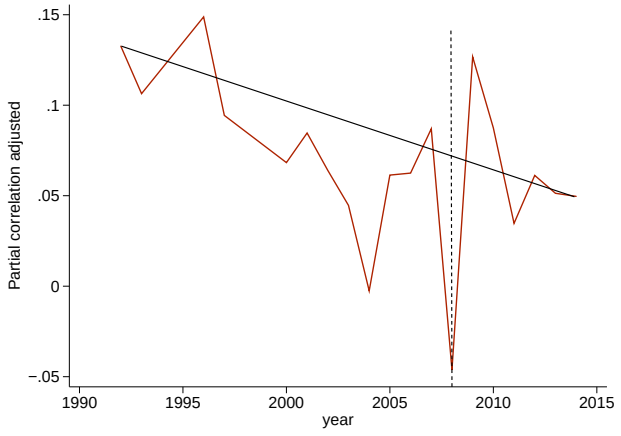
Figure 2 reports the partial correlations on the right panel and Fisher's partial correlations on the left panel of the effect of election on public spending from the 535 regressions coded. Consistently with the theory, the "true" value of the manipulation regarding public expenditures, suggested by the top of the distribution is positive yet close to zero. Moreover, if most estimates reveal a positive manipulation, this is partly explained by a clear skewed right distribution. Similar conclusions can be drawn from the funnel graph of correlations between elections and public revenues (Figure 3). 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

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

sensibility of voters regarding revenues cuts. Since leaders tend to increase spending and slightly reduce revenues when elections get close, Figure 4 reveals a degradation of the budget surplus preceding ballots. Selection bias appears once again distinctly. Figure 5 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. In short, funnel graphs show us that leaders manipulate budget before elections and that researchers manipulate their results in order to make the leaders' manipulation bigger.

Finally, we look at the distribution of the t-statistics of the estimates reported in the literature. Figure 6 show that t-statistics are concentrated around 2 (or -2) according to the expected direction of the association between elections and budget components. That is, t-statistics are concentrated around the standard threshold of statistical significance at 5%. It is then hard to believe this concentration around this specific value that we found in the four panels of Figure 6 to be a pure coincidence. Rather, we suspect this reveals a tendency of researchers to select the results they report, that is statistically significant effects. These first pieces of evidence suggest that the PbC literature is made of both manipulation of political leaders and researchers. However a graphical analysis is not sufficient to state it with certainty. At this stage, we then turn to a more rigorous statistical analysis that allow to dig deeper the findings, dissect the effects and quantify them more precisely.

Figure 1: Average partial correlation (adjusted) per year



Notes: We compute averaged partial correlation for each publication year of the 58 studies. Detailed figures are provided in Table A.12.

Figure 2: Funnel plot of election-on-spending partial correlation ($n = 535$)

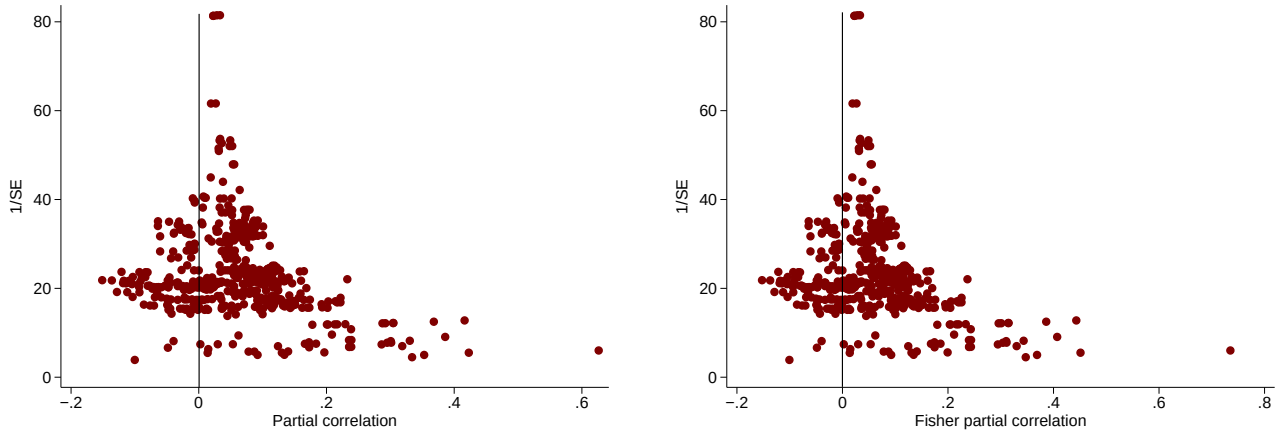


Figure 3: Funnel plot of election-on-revenues partial correlation ($n = 354$)

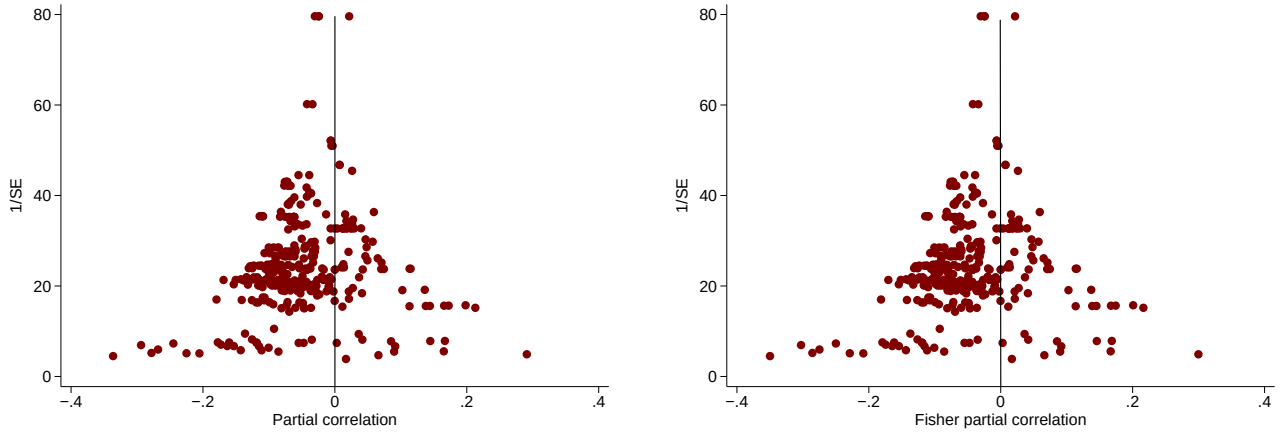


Figure 4: Funnel plot of election-on-fiscal surplus partial correlation ($n = 442$)

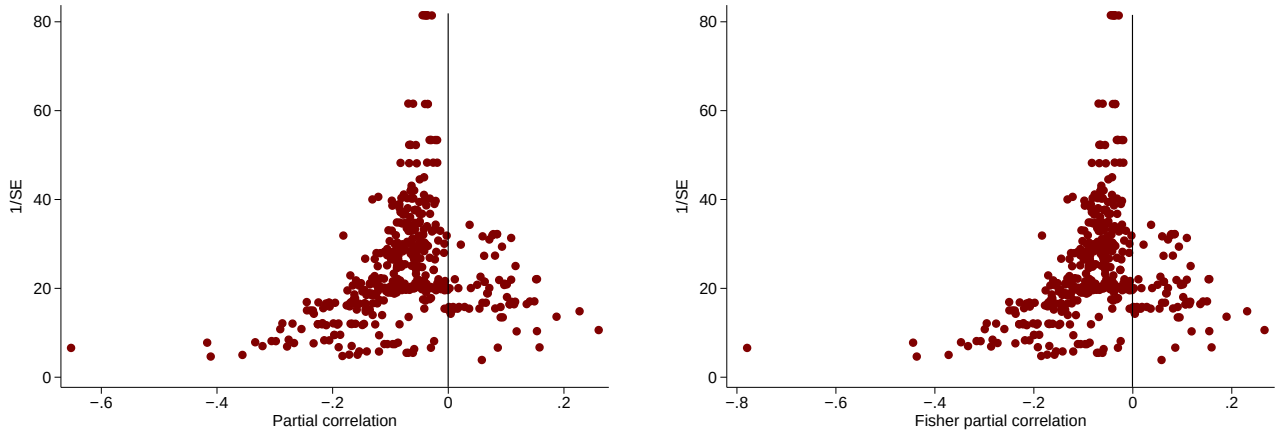


Figure 5: Funnel plot of election-on-fiscal output partial correlation ($n = 1,331$; $skewness = 0.31$)

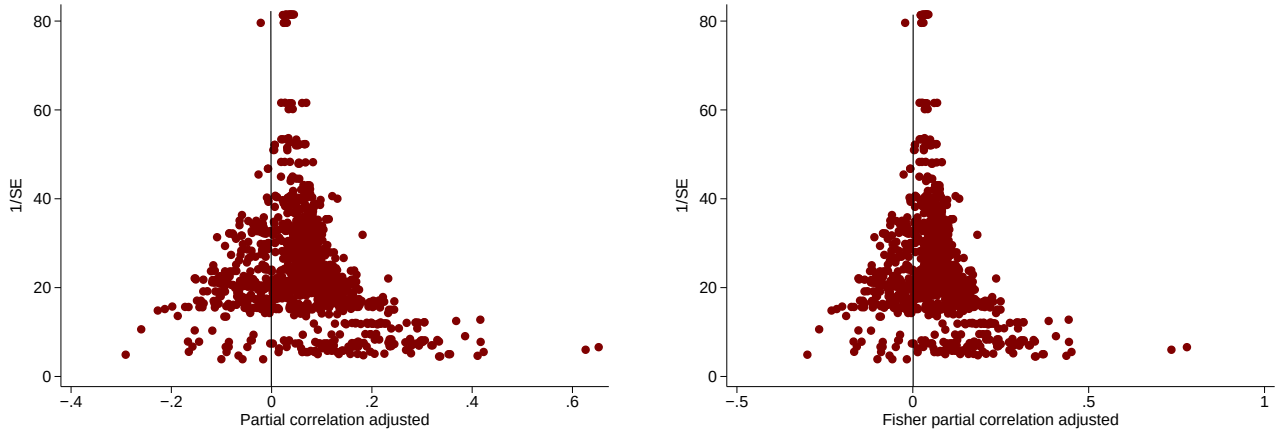
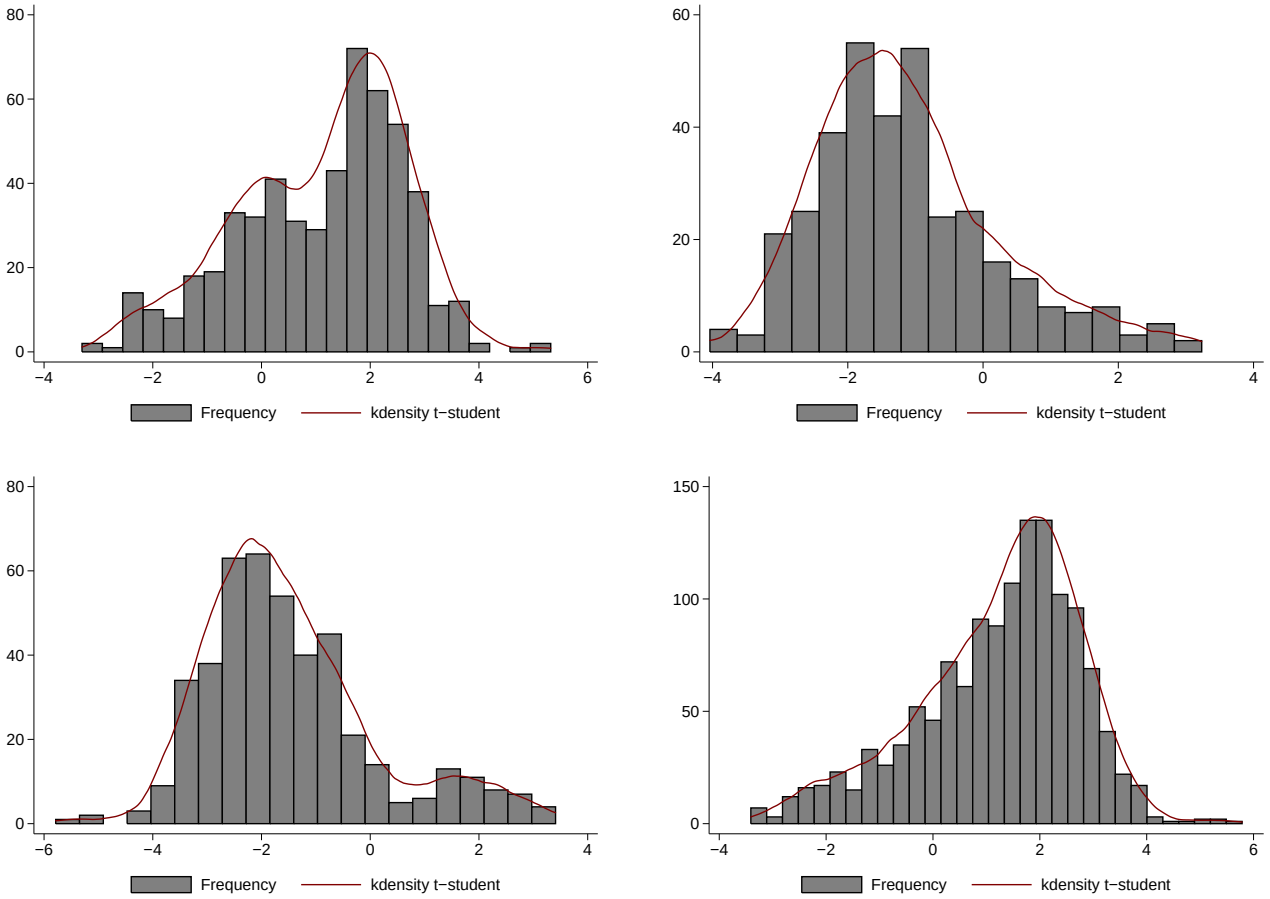


Figure 6: Distribution of t-statistics



Notes: Upper-left quadrant: Distribution of t-statistics on spending. Upper-right quadrant: distribution of t-statistics on revenues. Lower-left quadrant: Distribution of t-statistics on fiscal surplus. Lower-right quadrant: distribution of t-statistics on all output (inverse t-student for revenues and fiscal surplus).

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.[16] 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 Figure 5.[17] 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

[16] 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.

[17] 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:

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

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 B.13. We organize them in nine categories: measure of fiscal output; measure of the election variable; adjustments on the election variable; methodology employed; sample composition; model structure; decades and regions included; publication outlet and covariates included.

6. Basic 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.[18] 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.[19]

[18] "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.

[19] This last estimator is also acknowledged as the precision-effect estimate with standard error (PEESE) and is shown to be the best option when a genuine effect exists beyond selection bias (Stanley and Doucouliagos, 2012).

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, according to Cohen (1988), 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 act 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. On the other hand, the first column of 1 report the value of β_1 and its associated standard error, that indicates the strength of selection bias if any. This coefficient is strongly significant both statistically and economically. Ranging from 0.619 to 0.796, it is considered as large effect considering either Cohen (1988) (β_1>0.50) or Doucouliagos (2011) (β_1>0.33) guidelines. This suggests that results reported by researchers in this literature are strongly inflated. The manipulation of results is strong and widespread.

Finally, based on the heterogeneous existing estimations to date and once removed any selection bias, it appears that on average there is still a small but 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.

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 subsamples (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 4 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. $*p < 0.10, * * p < 0.05, * * *p < 0.01$. 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 2: Estimates of the FAT-PET MRA [By 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. $*p < 0.10, * * p < 0.05, * * *p < 0.01$.

7. 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 towards the direction expected by the theory. 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.[20]

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.

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

[20] These results echo back the work of Barberia and Avelino (2011) on Latin America countries.

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 Alesina et al. (1989), political leaders may prefer avoiding tax reforms before election to keep social order away from demonstrations and strikes.

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 punished 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 the expenditure side - 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.

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 the last two columns of Table 3 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 limited, we remain cautious on interpretation of results; notice however a 10% significant positive shift in spending composition in pre-electoral period, whereas the reverse is true for revenues, which is consistent with the idea of manipulating spending, instead of revenues, before elections.

Finally, 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 constituted by infrastructure spending.[21] 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.

[21] The dichotomy is no longer relevant with other examples, such as public agriculture 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).

Table 3: MRA [Patterns 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 effects	No	Yes	No	Yes	No	No
N	354	354	535	535	26	26

Notes: See Table 1. Dependent variable: non adjusted partial correlation. All columns are estimated with WLS (precision squared 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.

8. Country characteristics

In the literature, several authors argue that the existence of PbC depends on country characteristics. The MRA allows us to explore which of these factors are really conditioning or favoring the budget manipulation by political leaders. To do so we rely on the conditional factors censused by de Haan and Klomp (2013), namely economic development, quality and age of democracy, and constitutional settings. More precisely, we augment the model given in equation 2 by including a dummy equal to one for estimates from regressions containing only observations for which the conditional factor is present. For instance, the dummy "low-income countries" takes the value one for estimates having been computed on sample of low-income countries only. We present the results in Table 4.[22]

Even if the evidence in the literature is mixed, conventional thinking is that PbCs are more likely to occur or to be stronger in less developed economies (Shi and Svensson, 2006; Streb et al., 2009). The first two columns of Table 4 show that this view is not supported by meta data. The coefficients associated to the dummies go in the expected direction but are virtually null since they are not statistically significant. We do not detect any difference of the level of manipulation between low- and high-income countries. Another debate in the literature concerns of the effect of democracy on the occurrence of PbCs. Some authors such as Gonzalez (2002) and Block et al. (2003) argue that the level of democracy is negatively correlated to the budget manipulation, whereas others (Brender and Drazen, 2005; Klomp and de Haan, 2013b) consider that it is rather the age of democracy that matters most. Then, examine both the effect of age and level of democracy in columns 3 to 6. We see that the average effect of budget manipulation given by the PET coefficient is actually mainly driven by the set of young democracies. Older democracies exhibit significantly less strong effects. Actually the coefficient associated to the "*Established democracies*" offsets the PET coefficient, suggesting that leaders facing experienced voters are not likely to engage in pre-electoral budget distortions. On the other hand, it appears that a high-level of democracy is associated with reduced PbCs. Even if the less distorting effects are both statistically and economically more substantial in established democracies compared to stronger democracies, we cannot conclude clearly which democracy characteristic dominates.

[22] We considering methodologies of the World Bank Atlas to capture economic development, Brender and Drazen (2005) to capture age and level of democracy, but also Cheibub et al. (2010), Blume and Voigt (2011) and Bormann and Golder (2013) for constitutional rules, when authors don't use their own classification rules. Results are robust when applying all previous methodologies for all papers.

And the debate stays open. Quality and age of democracy often go together, that make their respective effects difficult to disentangle. At this stage the most plausible interpretation is that both matters. Even when voters are experienced, a degradation of the democracy level may offer a leader a greater room to manipulation, and conversely. Additionally, we look how the institutional settings result in a greater tendency of leaders to create pre-electoral budget cycles (Persson and Tabellini, 2003b). The evidence on this question is rather limited and results point in opposite direction (Streb et al., 2009; Klomp and de Haan, 2013b). In particular, we assess if leaders have greater incentives to make PbCs in parliamentary relative to presidential governments, and in majoritarian relative to proportional election-systems. Whether for the form of government or the electoral rules, the last four columns of Table 4 show that in average leaders do not behave differently regarding their level of budget manipulation according to the constitutional design.[23]

Finally, level and age democracy appear to affect the level of PbCs. In particular, newer and less strong democratic regimes are characterized by PbCs of greater magnitude. The MRA additionally reveal that other factors discussed in the literature do not affect systematically budget manipulation, so that cannot be qualified as PbC existence condition.

[23] A finer analysis of the manipulation of budget components (revenues vs expenditures for instance), as it had been undertaken by Persson and Tabellini (2003a) might reveal different strategies according to the nature of institutions whose the result is not detectable by just looking at the variation of the overall budget. However, due to the lack of observations, we are not able to proceed to this narrower analysis at meta level.

Table 4: MRA [Heterogeneity of PbC in comparison to benchmark results]

Variables	Income level		Age of democracy		Strength of democracy		Forms of government		Electoral rules	
FAT	0.658***	0.700***	0.631***	0.793***	0.659***	0.665***	0.698***	0.743***	0.678***	0.704***
	(0.245)	(0.241)	(0.241)	(0.237)	(0.232)	(0.233)	(0.237)	(0.261)	(0.234)	(0.237)
PET	0.024***	0.024***	0.024***	0.023***	0.024***	0.024***	0.023***	0.022***	0.024***	0.023***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
Low-income countries	0.001									
	(0.011)									
High-income countries		-0.008								
		(0.010)								
Young democracies			0.010							
			(0.010)							
Established democracies				**-0.030*** **						
				(0.011)						
Low level democracies					-0.008					
					(0.011)					
High level democracies						**-0.018***				
						(0.010)				
Presidential							-0.010			
							(0.024)			
Parliamentary								-0.023		
								(0.032)		
Majoritary									-0.017	
									(0.016)	
Proportional										-0.007
										(0.018)
RMSE	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055	0.055
Adjusted R^2	0.035	0.038	0.037	0.064	0.036	0.037	0.037	0.039	0.036	0.036
Number of cluster	147	147	147	147	147	147	147	147	147	147
N	1,331	1,331	1,331	1,331	1,331	1,331	1,331	1,331	1,331	1,331

Notes: See Table 1. Dependent variable: adjusted partial correlation. All columns are estimated with WLS (precision squared weights) and double clustered standard errors.

9. Sources of heterogeneities

So far, we reached to show two insights. First that PbCs do exist but are fairly overstated by researchers. Second we managed to explore deeper this first insight by decomposing overall budget in its sub-components and by assessing how country characteristics may affect the occurrence of PbCs. We offer now to turn to other potential sources of the estimate heterogeneity found in the literature in a more exhaustive manner. To do so we conduct a multiple MRA in two parts. The first part focuses on the sample and the model specification whereas the second part deals with the characteristics of the source paper, the methodology used and the choice of the covariates included in the model. It is not an easy task to know what exactly reflects each source of heterogeneity. However, we believe that result searching and selection bias are even more likely to be be captured by the second part of the multiple MRA. That is, when seeking specific results, authors are likely play on leverages such as the choice of the econometric estimator or the list of covariates for instance. As a consequence, we think the first part offers information of what factors may affect the manipulation of leaders, whereas the second part reflects rather the ways how the researchers may manipulate their results. Nonetheless, the border between the two kinds of manipulation is blurred. For each source of heterogeneity we examine, that is each covariate of our multiple MRA, both explanations are plausible and compatible. Then we do not pretend our classification allows us to disentangle precisely the effect from each kind of manipulation. Consequently, we remain cautious in our interpretation. Given that the choice of decomposing the multiple analysis in two parts, that through two distinct tables, is questionable, we finally gather all of the covariates studied in the multiple analysis and assess the strength of their role in explaining heterogeneity of estimates found in the literature through a bayesian MRA. Such a method removes any arbitrariness in the choice of covariates and reveals us which should be included in the "true" model explaining the estimates of pre-electoral budget manipulation.

9.1. Data and model specification

We run 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 related to the sample and model specification condition the existence and magnitude of PbC.

The first four columns of Tables 5 and 6 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.[24] 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.

Table 5 reveals a significant selection bias that disappears when controlling for authors' fixed effects. This encourages us to explore further sources of strategic manipulations for researchers.[25]. It is also worthy to note that the constant, reflecting the genuine effect of elections on budget variations, is several times stronger than the value found in the simple MRA. The coefficient is now above 0.10, that is a medium effect according to Cohen (1988) and a small impact according to Doucouliagos (2011).

Additionally, we do not find strong effects of sample choice. Results simply suggest PbCs are less severe during the 1990's than during the decades 1960 and 1970. The difference is statistically significant and not economically not negligible.[26] Concerning spatial differences, we only notice that Western countries and Japan are less affected by pre-electoral cycles. This result is line with the idea that in older and stronger democracies PbCs are less strong. However, the evidence is not strongly robust.

We then investigates if the model specification affects the strength of PbCs. In particular we focus on how selecting a subsample may matter. Interestingly, we notice that subsamples on established democracies exhibit significantly reduced PbCs, and results are strongly robust to the introduction of authors' dummies. In line with Table 3 and seminal work of Brender and

[24] There are 23 distinct measures of fiscal outputs are reported in the 58 studies included in our analysis.
[25] See Section 9.2
[26] It could reflect the incorporation of theories on hysteresis of unemployment (Blanchard and Summers, 1986) and time inconsistency (Kydland and Prescott, 1977; Barro and Gordon, 1983) to economic policy implications in the 1990's. It's result could also driven by a couple papers reporting weak evidence of budget manipulation.

Drazen (2005), we have suggesting evidences that informed and experienced voters reduce the budget manipulation, due to democratic maturity.

Table 5: Multiple FAT-PET models [Data and model specification]

Variables	All observations Adjusted partial correlation General	Specific	FEML	FEML double cluster
		Publication bias		
Standard error	**0.898** **(0.389)**	**0.884** **(0.265)**	0.323 (0.523)	0.323 (0.407)
		Model structure		
Interactive model	0.006 (0.008)		0.012 (0.009)	**0.012** **(0.007)**
Subsample	-0.004 (0.003)		-0.001 (0.005)	-0.001 (0.004)
ConstitSamp	**-0.027** **(0.016)**	**-0.020** **(0.011)**	0.003 (0.005)	0.003 (0.005)
HighincSamp	-0.011 (0.021)		-0.008 (0.016)	-0.008 (0.013)
EstdemocSamp	**-0.028** **(0.009)**	**-0.036** **(0.006)**	**-0.040** **(0.009)**	**-0.040** **(0.012)**
HighdemocSamp	-0.005 (0.011)		-0.014 (0.011)	**-0.014** **(0.008)**
BadSamp	0.009 (0.013)		-0.001 (0.011)	-0.001 (0.011)
		Time and region		
1980s	0.013 (0.020)		0.021 (0.025)	0.021 (0.025)
1990s	**-0.092** **(0.031)**	**-0.099** **(0.033)**	-0.052 (0.035)	**-0.052** **(0.028)**
Recent	0.005 (0.010)		**0.012** **(0.007)**	0.012 (0.011)
Eeca	0.015 (0.009)		-0.010 (0.012)	-0.010 (0.009)
Lac	0.002 (0.022)		0.003 (0.015)	0.003 (0.013)
Mena	-0.001 (0.011)		-0.009 (0.011)	-0.009 (0.011)
Sap	0.000 (0.010)		0.001 (0.010)	0.001 (0.008)
Ssa	-0.014 (0.013)		0.015 (0.013)	0.015 (0.012)
WeJ	0.001 (0.010)		-0.015 (0.009)	**-0.015** **(0.008)**
Global	-0.010 (0.017)		0.004 (0.015)	0.004 (0.012)
Constant	**0.106** **(0.052)**	**0.121** **(0.035)**	- -	- -
RMSE	0.053	0.054	0.048	0.048
Adjusted R^2	0.102	0.098	0.567	0.567
Number of cluster	57	57	57	147
Number of covariates	19	5	18	18
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 squared weights. Columns 3 and 4 includes authors fixed effects (not reported). Standard errors clustered by studies in parenthesis. Double clustering on studies and fiscal output in column 4. Adjusted R^2 is not strictly comparable across the different models. $*p < 0.10, * * p < 0.05, * * *p < 0.01$.

9.2. Paper, methodology and covariates

The MRA reported in Table 6 incorporates several key variables related to publication itself, the methodology and the list of covariates retained by authors to explain the heterogeneity of the results. Once again, the publication bias disappears when augmenting the model with author dummies. We then look if the characteristics of papers play a role. A first hypothesis we test is if published papers report greater PbCs. Journals may be more prone to select significant over zero-effect results. The coefficient associated to the variable "Unpublished" suggests this is not the case. Second, the quality of the paper imperfectly proxied through the impact factor of the review or series in which the paper is published, does not affect the level of manipulation neither. We also focus on a specific journal whose political cycles are one of the main topic, namely Public Choice journal. Despite editorial boarding focused on decision-makers' strategic behavior, the journal does not seem to publish disproportionately PbC-friendly articles. Last but not least, we have suggesting evidences of the decreasing trend in PbC, by taking into account the publication timing (see columns 3-4). As the "*Before2008*" coefficient is significant when controlling for authors' fixed effect it could imply a genuine decline in PbC, which may be hidden by researchers. In line with structural break of Figure 1, it could also capture the massive fiscal stimulus, in response to the global recession of 2008-2009 (Lee et al., 2009).

A second block of covariates focuses on the methodology employed by authors. More precisely we look at the measure of the election and budget output selected, and of the estimator selected. First, behavior on electoral manipulation does not differ when the dependent variable refers alternatively to fiscal surpluses, revenues or spending gross categories, as *YSurplus* coefficient underlines (columns 1-4). So, PbC magnitude is related to the nature of fiscal output and to its adjustments.[27] 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 predetermined election, is associated with less magnitude in PbC. Econometric methodology makes a difference. Using dynamic panel estimator leads to more severe PbCs (columns 1 and 2). One possible explanation is the use and abuse of GMM estimators to find convincing insights on PbCs. We may even consider that this insight finds support in the fact that this effect

[27] See Section 3

disappears once we control for authors' fixed effect. Conversely, correction of standard errors for heteroskedasticity and autocorrelation contributes to decrease magnitude of PbCs (columns 1-4). That is, using more requiring estimators leads to smaller PbCs. Finally, the last block of Table 6 shows that the choice of the list of covariates is not neutral and may affect the strength of PbCs authors report. Hence, when determining the way they measure their interest variables, which control variables they include in their econometric model and which tools they use to estimate it, authors may increase their expectancy to obtain specific and significant results in order their theoretical assumptions or ideology to be validated.[28]

[28] As we found suggesting evidence of the damping impact of established democracies on PbCs, we select covariates from Brender and Drazen (2005), but also taking into account for the introduction, or not, of partisan cycles.

Table 6: Multiple FAT-PET models [Paper, methodology and covariates]

Variables	All observations Adjusted partial correlation			
	General	Specific	FEML	FEML double cluster
			Publication bias	
Standard error	**0.954*** **	**0.643*** **	0.187	0.187
	(0.254)	**(0.239)**	(0.466)	(0.404)
			Paper	
Public Choice	0.010		-0.021	-0.021
	(0.012)		(0.016)	(0.013)
Unpublished	0.004		-0.100	-0.100
	(0.011)		(0.095)	(0.092)
Impact factor	0.000		0.002	0.002
	(0.000)		(0.002)	(0.002)
Before 2008	-0.013		**-0.083*** **	**-0.083*** **
	(0.012)		**(0.011)**	**(0.020)**
			Methodology	
Samplesize	0.000		**-0.000***	-0.000
	(0.000)		**(0.000)**	(0.000)
Infrannual	-0.002		-0.011	-0.011
	(0.011)		(0.008)	(0.024)
YSurplus	0.015		0.010	0.010
	(0.012)		(0.016)	(0.014)
YVar	-0.013		0.002	0.002
	(0.014)		(0.011)	(0.007)
YCycl	**-0.036***	**-0.034*** **	-0.001	-0.001
	(0.019)	**(0.009)**	(0.015)	(0.018)
YCentral	-0.008		**0.071*** **	**0.071*** **
	(0.012)		**(0.006)**	**(0.013)**
ElectDum	0.004		0.001	0.001
	(0.009)		(0.006)	(0.005)
ElectRat	-0.010		0.001	0.001
	(0.013)		(0.006)	(0.007)
ACalendar	0.004		-0.003	-0.003
	(0.009)		(0.010)	(0.009)
AHighest	0.007		0.128	0.128
	(0.011)		(0.119)	(0.116)
AExog	**-0.016** **	**-0.018*** **	-0.012	**-0.012***
	(0.007)	**(0.006)**	(0.009)	**(0.007)**
EconDynamic	**0.017*** **	**0.014** **	0.005	0.005
	(0.006)	**(0.007)**	(0.004)	(0.003)
SE Correction	**-0.014***	**-0.004**	**-0.011*** **	**-0.011** **
	(0.008)	**(0.007)**	**(0.003)**	**(0.004)**
			Covariates	
GDPpc.	0.012		-0.014	-0.014
	(0.014)		(0.016)	(0.013)
Trade	-0.003		0.010	0.010
	(0.020)		(0.027)	(0.019)
PopStruct	0.001		0.008	0.008
	(0.020)		(0.015)	(0.012)
OG	-0.005		**-0.022***	**-0.022** **
	(0.014)		**(0.013)**	**(0.011)**
Partisan	**0.037***	**0.031** **	-0.014	-0.014
	(0.019)	**(0.012)**	(0.013)	(0.013)
Time	0.007		0.007	0.007
	(0.008)		(0.004)	(0.005)
Constant	-0.005	**0.024** **	-	-
	(0.026)	**(0.010)**	-	-
RMSE	0.053	0.054	0.048	0.048
Adjusted R^2	0.111	0.073	0.561	0.561
Number of cluster	57	57	57	147
Number of covariates	25	7	24	24
Authors fixed effect	No	No	Yes	Yes
N	1,331	1,331	1,331	1,331

Notes: See Table 5.

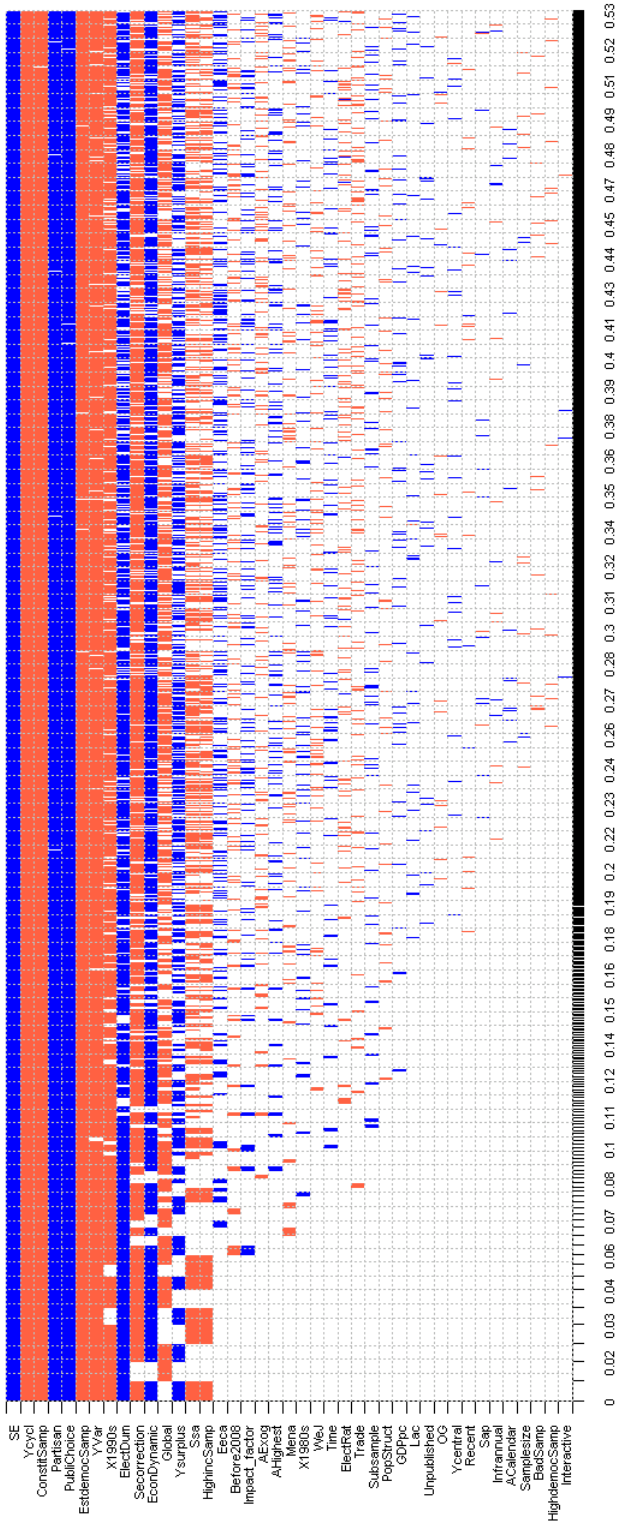
9.3. Bayesian meta-analysis

Lastly, we provide a bayesian MRA. It presents advantages in two respects. First, it offers another rigorous and agnostic approach to determine which factors affect the heterogeneity of the results found in the literature and quantify these effects. Second, as we realize the multiple MRA in two stages, we believe putting together all the covariates examined in Tables 5 and 6 allows to reassess the effect of each covariate without imposing any predetermined structure to our model of estimation.

We condider the uniform prior on model probabilities together with a UIP g-prior, as Havránek (2015).[29] All results are display in Figure 7 and Table 7. We retain all covariates with a posterior inclusion probability (PIP) over 50%, in the frequentist check OLS. Our findings relative to the established democracies and 1990's damping effect, but also to the non neutrality of empirical methodology and covariates inclusion, are corroborates. They are still robust to alternative priors (see Figure 8 and Table 8) and use of precision squared as weights (see Figure 9 and Table 9).

[29] We use the BMS package of Stefan Zeugner. We consider a chain of 200 million recorded draws with 100 million burn-ins, by applying the birth-death sampler. 48, 667731 models are visited and the best 5000 models have a cumulative probability of 54%. Additional details are available upon request.

Figure 7: Bayesian model averaging (BMA): Model inclusion [UIP g-prior; uniform model prior]



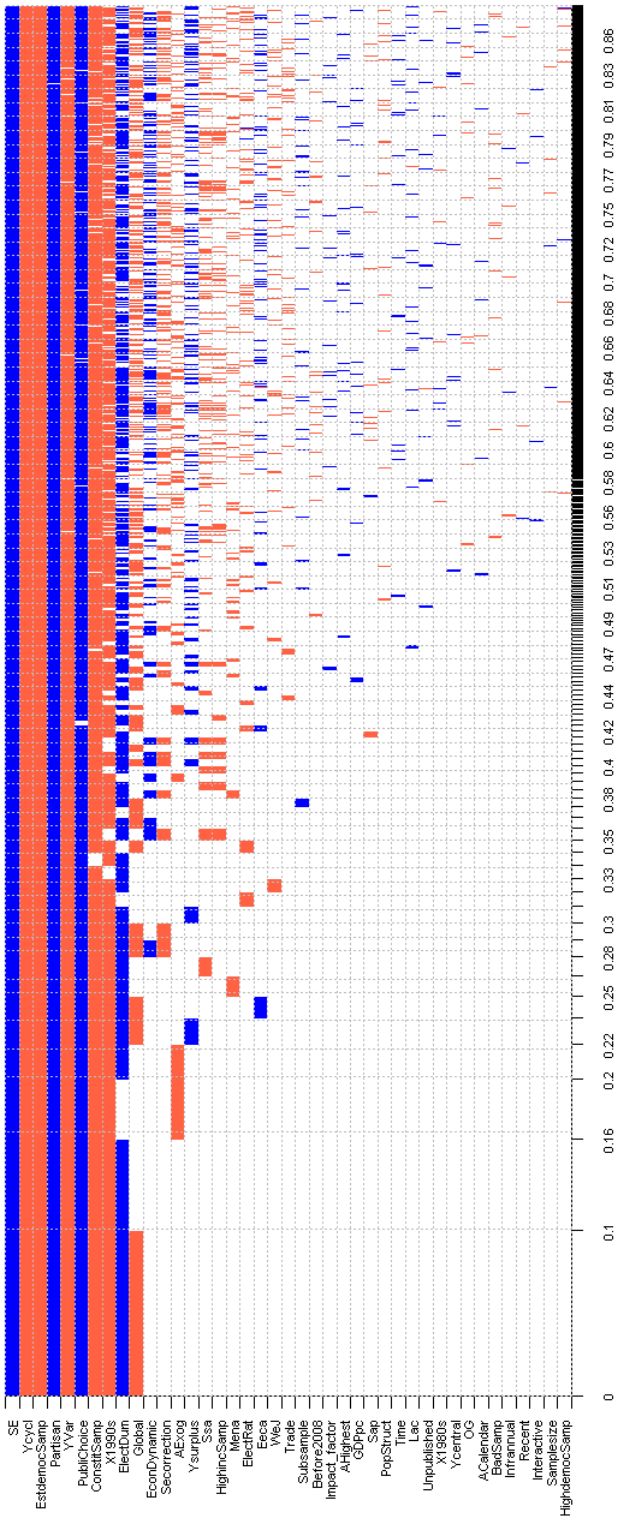
Notes: Dependent variable: adjusted partial correlation. Birth-death sampler used. Columns denote individual models; the variables are sorted by posterior inclusion probability (PIP) in descending order. Blue color (darker in grayscale): the variable is included and the estimated sign is positive. Red color (lighter in grayscale): the variable is included and the estimated sign is negative. No color: the variable is not included in the model. The horizontal axis measures the cumulative probabilities of the best 5, 000 models. Numerical results of the BMA estimation are reported in Table 7. A detailed description of all the variables is available in Table B.13.

Table 7: BMA [UIP g-prior; uniform model prior]

	BMA				Frequentist check (OLS)			
	Model prior: uniform				Cluster: study		Double cluster	
	PIP	Post.mean	Post.s.d.	Cond.pos.sign	Coefficient	Standard error	Coefficient	Standard error
Publication bias								
SE	**1.000**	**0.961**	**0.116**	**1.000**	**0.748*** **	**(0.268)**	**0.748*** **	**(0.242)**
Model structure								
Interactive model	0.008	0.000	0.001	0.697				
Subsample	0.072	0.001	0.003	1.000				
ConstitSamp	**1.000**	**-0.034**	**0.008**	**0.000**	**-0.020***	**(0.012)**	-0.020	(0.013)
HighincSamp	0.431	-0.012	0.016	0.000				
EstdemocSamp	**0.995**	**-0.043**	**0.010**	**0.000**	**-0.037*** **	**(0.006)**	**-0.037*** **	**(0.010)**
HighdemocSamp	0.009	0.000	0.002	0.085				
BadSamp	0.010	0.000	0.001	0.025				
Time and region								
1980s	0.087	0.002	0.007	0.966				
1990s	**0.866**	**-0.042**	**0.022**	**0.000**	**-0.084*** **	**(0.026)**	**-0.084*** **	**(0.021)**
Recent	0.016	0.000	0.002	0.270				
Eeca	0.242	0.004	0.009	1.000				
Lac	0.028	0.000	0.003	0.929				
Mena	0.104	-0.002	0.006	0.000				
Sap	0.016	0.000	0.002	0.751				
Ssa	0.481	-0.015	0.018	0.000				
WeJ	0.085	-0.001	0.005	0.000				
Global	**0.617**	**-0.016**	**0.015**	**0.000**	-0.010	(0.006)	-0.010	(0.010)
Paper								
Public Choice	**0.997**	**0.032**	**0.008**	**1.000**	0.017	(0.011)	0.017	(0.016)
Unpublished	0.027	0.000	0.002	0.989				
Impact factor	0.128	0.000	0.000	1.000				
Before 2008	0.129	-0.002	0.007	0.000				
Methodology								
Samplesize	0.010	0.000	0.000	0.436				
Infrannual	0.013	0.000	0.002	0.260				
YSurplus	**0.533**	**0.007**	**0.007**	**1.000**	0.013	(0.010)	0.013	(0.009)
YVar	**0.983**	**-0.039**	**0.012**	**0.000**	-0.012	(0.012)	-0.012	(0.013)
YCycl	**1.000**	**-0.065**	**0.012**	**0.000**	**-0.031*** **	**(0.010)**	**-0.031** **	**(0.015)**
YCentral	0.018	0.000	0.001	0.978				
ElectDum	**0.828**	**0.017**	**0.010**	**1.000**	0.009	(0.008)	0.009	(0.007)
ElectRat	0.076	-0.001	0.005	0.035				
ACalendar	0.011	0.000	0.001	0.990				
AHighest	0.108	0.001	0.004	1.000				
AExog	0.115	-0.002	0.005	0.000				
EconDynamic	**0.795**	**0.015**	**0.009**	**1.000**	**0.010***	**(0.005)**	**0.010** **	**(0.005)**
Se Correction	**0.808**	**-0.015**	**0.009**	**0.000**	-0.007	(0.006)	-0.007	(0.006)
Covariates								
GDPpc.	0.036	0.000	0.003	0.999				
Trade	0.076	-0.001	0.003	0.000				
PopStruct	0.043	0.000	0.002	0.003				
OG	0.019	0.000	0.001	0.018				
Partisan	**0.998**	**0.039**	**0.009**	**1.000**	**0.022***	**(0.012)**	**0.022***	**(0.011)**
Time	0.081	0.001	0.004	1.000				
Constant	**1.000**	**0.070**	NA	NA	**0.106*** **	**(0.034)**	**0.106*** **	**(0.029)**
RMSE			-		0.052		0.052	
Adjusted R^2			-		0.135		0.135	
Number of studies			57		57		57	
Number of cluster			-		57		147	
N			1.331		1.331		1.331	

Notes: Dependent variable: adjusted partial correlation. Birth-death sampler used. Post.mean: posterior mean conditional on inclusion. Post.s.d.: posterior standard deviation conditional on inclusion. Cond.pos.sign: probability of positive sign conditional on inclusion. In the Frequentist check we only include explanatory variables with PIP > 0.500. The standard errors in the Frequentist check are clustered on studies, or double clustered on studies and fiscal output.

Figure 8: BMA: Model inclusion [BRIC g-prior; beta-binomial model prior]



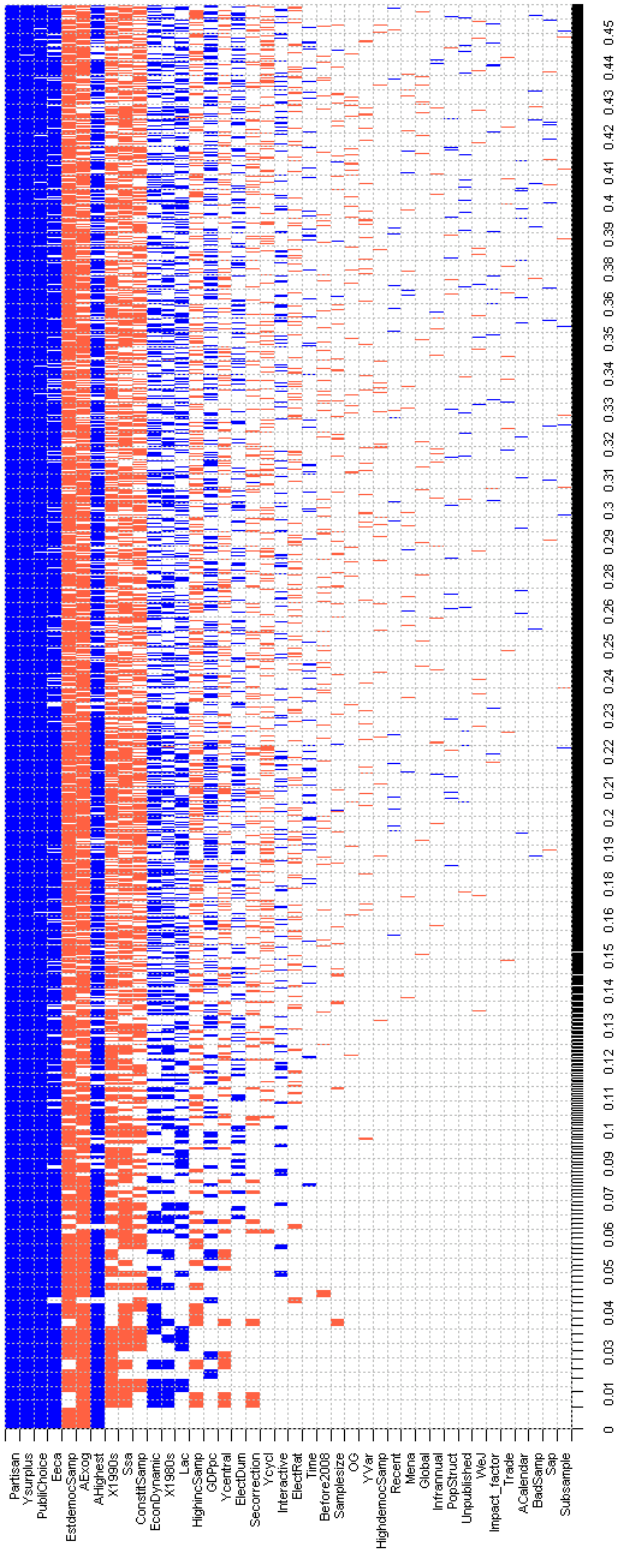
Notes: See Figure 7.

Table 8: BMA [BRIC g-prior; beta-binomial model prior]

	BMA				Frequentist check (OLS)			
	Model prior: beta-binomial				Cluster: study		Double cluster	
	PIP	Post.mean	Post.s.d.	Cond.pos.sign	Coefficient	Standard error	Coefficient	Standard error
Publication bias								
SE	**1.000**	**1.021**	**0.113**	**1.000**	**0.848*** **	**(0.235)**	**0.848*** **	**(0.234)**
Model structure								
Interactive model	0.007	0.000	0.001	0.874				
Subsample	0.031	0.000	0.002	1.000				
ConstitSamp	**0.974**	**-0.029**	**0.009**	**0.000**	-0.017	(0.011)	-0.017	(0.014)
HighincSamp	0.111	-0.003	0.009	0.000				
EstdemocSamp	**1.000**	**-0.045**	**0.009**	**0.000**	**-0.037*** **	**(0.007)**	**-0.037*** **	**(0.010)**
HighdemocSamp	0.006	0.000	0.001	0.087				
BadSamp	0.008	0.000	0.001	0.002				
Time and region								
1980s	0.011	0.000	0.002	0.688				
1990s	**0.886**	**-0.045**	**0.021**	**0.000**	**-0.083*** **	**(0.023)**	**-0.083*** **	**(0.020)**
Recent	0.008	0.000	0.001	0.492				
Eeca	0.077	0.001	0.005	0.992				
Lac	0.014	0.000	0.002	0.966				
Mena	0.084	-0.001	0.005	0.001				
Sap	0.017	0.000	0.002	0.175				
Ssa	0.155	-0.004	0.010	0.000				
WeJ	0.041	-0.001	0.003	0.000				
Global	0.454	-0.010	0.013	0.000				
Paper								
Public Choice	**0.987**	**0.031**	**0.009**	**1.000**	0.016	(0.013)	0.016	(0.018)
Unpublished	0.013	0.000	0.001	0.948				
Impact factor	0.023	0.000	0.000	1.000				
Before 2008	0.023	0.000	0.002	0.000				
Methodology								
Samplesize	0.006	0.000	0.000	0.361				
Infrannual	0.008	0.000	0.001	0.230				
YSurplus	0.176	0.002	0.005	1.000				
YVar	**0.992**	**-0.042**	**0.010**	**0.000**	-0.013	(0.012)	-0.013	(0.013)
YCycl	**1.000**	**-0.062**	**0.012**	**0.000**	**-0.023*** **	**(0.008)**	-0.023	(0.015)
YCentral	0.011	0.000	0.001	0.999				
ElectDum	**0.764**	**0.017**	**0.011**	**1.000**	0.012	(0.007)	**0.012***	**(0.007)**
ElectRat	0.080	-0.002	0.006	0.008				
ACalendar	0.008	0.000	0.001	0.932				
AHighest	0.022	0.000	0.002	1.000				
AExog	0.177	-0.003	0.007	0.000				
EconDynamic	0.225	0.004	0.008	1.000				
Se Correction	0.221	-0.004	0.008	0.000				
Covariates								
GDPpc.	0.018	0.000	0.002	1.000				
Trade	0.036	0.000	0.002	0.000				
PopStruct	0.016	0.000	0.001	0.005				
OG	0.009	0.000	0.001	0.014				
Partisan	**0.998**	**0.042**	**0.009**	**1.000**	**0.028** **	**(0.012)**	**0.028** **	**(0.012)**
Time	0.016	0.000	0.001	1.000				
Constant	**1.000**	**0.062**	NA	NA	**0.095*** **	**(0.027)**	**0.095*** **	**(0.025)**
RMSE					0.053		0.053	
Adjusted R^2					0.1119		0.1119	
Number of studies			57		57		57	
Number of cluster					57		147	
N			1.331		1.331		1.331	

Notes: See Table 7.

Figure 9: Weighted BMA: Model inclusion [UIP g-prior; uniform model prior]



Notes: Precision squared weights. See Figure 7.

Table 9: Weighted BMA [UIP g-prior; uniform model prior]

	BMA				Frequentist check (OLS)			
	Model prior: uniform				Cluster: study		Double cluster	
	PIP	Post.mean	Post.s.d.	Cond.pos.sign	Coefficient	Standard error	Coefficient	Standard error
Model structure								
Interactive model	0.139	0.001	0.004	1.000				
Subsample	0.008	0.000	0.000	0.530				
ConstitSamp	**0.528**	**-0.010**	**0.011**	**0.000**	0.007	(0.010)	0.007	(0.013)
HighincSamp	0.340	-0.006	0.010	0.004				
EstdemocSamp	**0.847**	**-0.020**	**0.011**	**0.000**	**-0.027*** **	**(0.007)**	**-0.027*** **	**(0.010)**
HighdemocSamp	0.021	0.000	0.003	0.000				
BadSamp	0.009	0.000	0.001	0.971				
Time and region								
1980s	0.384	0.009	0.012	0.997				
1990s	**0.641**	**-0.019**	**0.017**	**0.000**	**-0.122*** **	**(0.025)**	**-0.122*** **	**(0.020)**
Recent	0.019	0.000	0.001	0.956				
Eeca	0.897	0.019	0.010	1.000				
Lac	0.365	0.007	0.010	0.996				
Mena	0.019	0.000	0.001	0.394				
Sap	0.009	0.000	0.000	0.383				
Ssa	**0.630**	**-0.014**	**0.012**	**0.000**	0.005	(0.007)	0.005	(0.008)
WeJ	0.015	0.000	0.001	0.129				
Global	0.018	0.000	0.001	0.025				
Paper								
Public Choice	**0.990**	**0.021**	**0.006**	**1.000**	**0.022***	**(0.013)**	0.022	(0.017)
Unpublished	0.015	0.000	0.001	0.832				
Impact factor	0.012	0.000	0.000	0.881				
Before 2008	0.044	0.000	0.002	0.003				
Methodology								
Samplesize	0.044	0.000	0.000	0.088				
Infrannual	0.018	0.000	0.002	0.239				
YSurplus	**0.999**	**0.013**	**0.003**	**1.000**	0.011	(0.010)	0.011	(0.010)
YVar	0.028	0.000	0.002	0.006				
YCycl	0.154	-0.004	0.010	0.000				
YCentral	0.236	-0.003	0.006	0.001				
ElectDum	0.208	0.002	0.005	1.000				
ElectRat	0.133	-0.002	0.006	0.000				
ACalendar	0.010	0.000	0.000	1.000				
AHighest	**0.809**	**0.013**	**0.008**	**1.000**	0.006	(0.009)	0.006	(0.009)
AExog	**0.820**	**-0.015**	**0.009**	**0.000**	-0.008	(0.007)	-0.008	(0.012)
EconDynamic	0.446	0.005	0.007	1.000				
Se Correction	0.157	-0.002	0.004	0.003				
Covariates								
GDPpc.	0.339	0.005	0.008	1.000				
Trade	0.012	0.000	0.001	0.266				
PopStruct	0.016	0.000	0.001	0.783				
OG	0.030	0.000	0.001	0.021				
Partisan	**1.000**	**0.037**	**0.007**	**1.000**	**0.030** **	**(0.014)**	**0.030** **	**(0.013)**
Time	0.060	0.000	0.002	0.998				
Constant	**1.000**	**1.004**	NA	NA	**0.156*** **	**(0.025)**	**0.156*** **	**(0.021)**
RMSE					0.054		0.054	
Adjusted R^2					0.085		0.085	
Number of studies			57		57		57	
Number of cluster					57		147	
N			1.331		1.331		1.331	

Notes: Precision squared weights. See Table 7.

10. 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, concluding that bias from research in this literature is far greater than the manipulation of budget by leaders, 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.

References

Afonso, A., 2008. Ricardian fiscal regimes in the European Union. Empirica 35 (3), 313–334.

Akhmedov, A., Zhuravskaya, E., 2004. Opportunistic political cycles: Test in a young democracy setting. The Quarterly Journal of Economics 119 (4), 1301–1338.

Alesina, A., Ardagna, S., Trebbi, F., 2006. Who adjusts and when? on the political economy of reforms. Working Paper 12049, National Bureau of Economic Research.

Alesina, A., Cohen, G. D., Roubini, N., 1992. Macroeconomic policy and elections in OECD democracies. Economics and Politics 4 (1), 1–31.

Alesina, A., Cohen, G. D., Roubini, N., 1993. Electoral business cycle in industrial democracies. European Journal of Political Economy 9 (1), 1–23.

Alesina, A., Mirless, J., Neumann, M. J. M., 1989. Politics and business cycles in industrial democracies. Economic Policy 4 (8), 55–98.

Alt, J. E., Lassen, D. D., 2006. Transparency, political polarization, and political budget cycles in OECD countries. American Journal of Political Science 50 (3), 530–550.

Arvate, P. R., Avelino, G., Tavares, J., February 2009. Fiscal conservatism in a new democracy: Sophisticated versus naïve voters. Economics Letters 102 (2), 125–127.

Ashworth, J., Heyndels, B., 2002. Tax structure turbulence in OECD countries. Public Choice 111 (3-4), 347–76.

Askarov, Z., Doucouliagos, H., 2013. Does aid improve democracy and governance? A meta-regression analysis. Public Choice 157 (3), 601–628.

Barberia, L., Avelino, G., 2011. Do political budget cycles differ in Latin American democracies? Journal of Lacea Economía 11 (2), 101–134.

Barro, R. J., Gordon, D. B., 1983. Rules, discretion and reputation in a model of monetary policy. Journal of Monetary Economics 12 (1), 101–121.

Bayar, A., Smeets, B., 2009. Economic, political and institutional determinants of budget deficits in the European Union. CESifo Working Paper Series 2611, CESifo Group Munich.

Blanchard, O. J., Summers, L. H., 1986. Hysteresis and the European unemployment problem. In: NBER Macroeconomics Annual 1986, Volume 1. NBER Chapters. Cambridge: National Bureau of Economic Research, Inc, pp. 15–90.

Block, S. A., 2002. Political business cycles, democratization, and economic reform: the case of Africa. Journal of Development Economics 67 (1), 205–228.

Block, S. A., Ferree, K. E., Singh, S., 2003. Multiparty competition, founding elections and political business cycles in Africa. Journal of African Economies 12 (3), 444–468.

Blume, L., Voigt, S., 2011. Federalism and decentralization − a critical survey of frequently used indicators. Constitutional Political Economy 22 (3), 238–264.

Bormann, N.-C., Golder, M., 2013. Democratic electoral systems around the world, 1946-2011. Electoral Studies 32 (2), 360–369.

Bove, V., Efthyvoulou, G., Navas, A., 2014. Political cycles in public expenditure: Butter vs guns. Working Papers 2013016, The University of Sheffield, Department of Economics.

Brender, A., 2003. The effect of fiscal performance on local government election results in Israel: 1989-1998. Journal of Public Economics 87 (9-10), 2187–2205.

Brender, A., Drazen, A., 2005. Political budget cycles in new versus established democracies. Journal of Monetary Economics 52 (7), 1271–1295.

Brender, A., Drazen, A., 2007. Why is economic policy different in new democracies? affecting attitudes about democracy. NBER Working Papers 13457, National Bureau of Economic Research, Inc.

Brender, A., Drazen, A., 2013. Elections, leaders, and the composition of government spending. Journal of Public Economics 97 (C), 18–31.

Buti, M., Van Den Noord, P., 2004. Fiscal discretion and elections in the early years of EMU. JCMS: Journal of Common Market Studies 42 (4), 737–756.

Card, D., Krueger, A. B., 1995. Time-series minimum-wage studies: A meta-analysis. American Economic Review 85 (2), 238–43.

Chang, E., 2008. Electoral incentives and budgetary spending: Rethinking the role of political institutions. Journal of Politics 70 (4), 1086–1097.

Cheibub, J., Gandhi, J., Vreeland, J., 2010. Democracy and dictatorship revisited. Public Choice 143 (1), 67–101.

Cohen, J., 1988. Statistical Power Analysis in the Behavorial Sciences (2nd ed.). Hillsdale: Erlbaum.

Combes, J.-L., Maurel, M., Ebeke, C., 2013. The effect of remittances prior to an election. Working Papers 201307, CERDI.

Combes, J.-L., Maurel, M., Ebeke, C., 2015. The effect of remittances prior to an election. Applied Economics 47 (28), 1–16.

Costa-Fernandes, A. L., Mota, P. R., 2013. The present sovereign debt crisis of the euro zone peripheral countries: A case of non-mature democracies and less developed economies. Panaoeconomicus 3 (Special Issue), 291–310.

Costa-Font, J., De-Albuquerque, F., Doucouliagos, H., 2014. Do jurisdictions compete on taxes? A meta-regression analysis. Public Choice 161 (3), 451–470.

de Haan, J., Klomp, J., 2013. Conditional political budget cycles: a review of recent evidence. Public Choice 157 (3), 387–410.

de Linde Leonard, M., Stanley, T. D., Doucouliagos, H., 2014. Does the UK minimum wage reduce employment? A meta-regression analysis. British Journal of Industrial Relations 52 (3), 499–520.

Doucouliagos, H., 2011. How large is large? Preliminary and relative guidelines for interpreting partial correlations in economics. Economics Series 2011_5, Deakin University, Faculty of Business and Law, School of Accounting, Economics and Finance.

Doucouliagos, H., Stanley, T. D., 2009. Publication selection bias in minimum-wage research? a meta-regression analysis. British Journal of Industrial Relations 47 (2), 406–428.

Drazen, A., 2001. The political business cycle after 25 years. In: NBER Macroeconomics Annual 2000, Volume 15. NBER Chapters. National Bureau of Economic Research, Inc, pp. 75–138.

Drazen, A., Eslava, M., 2006. Pork barrel cycles. NBER Working Papers 12190, National Bureau of Economic Research, Inc.

Drazen, A., Eslava, M., 2010. Electoral manipulation via voter-friendly spending: Theory and evidence. Journal of Development Economics 92 (1), 39–52.

Dreher, A., Vaubel, R., 2004. Do IMF and IBRD cause moral hazard and political business cycles? Evidence from panel data. Open Economies Review 15 (1), 5–22.

Ebeke, C., Ölçer, D., 2013. Fiscal Policy over the Election Cycle in Low-Income Countries. IMF Working Papers 13/153, International Monetary Fund.

Efthyvoulou, G., 2012. Political budget cycles in the European Union and the impact of political pressures. Public Choice 153 (3), 295–327.

Ehrhart, H., 2013. Elections and the structure of taxation in developing countries. Public Choice 156 (1), 195–211.

Enkelmann, S., Leibrecht, M., 2013. Political expenditure cycles and election outcomes: Evidence from disaggregation of public expenditures by economic functions. Economics Letters 121 (1), 128–132.

Eslava, M., 2005. Political budget cycles or voters as fiscal conservatives? Evidence from Colombia. Documentos CEDE 003343, Universidad de los Andes-CEDE.

Eslava, M., 2011. The political economy of fiscal deficits: A survey. Journal of Economic Surveys 25 (4), 645–673.

Fatás, A., Mihov, I., 2003. The case for restricting fiscal policy discretion. The Quarterly Journal of Economics 118 (4), 1419–1447.

Franzese Jr., R. J., 2000. Electoral and partisan manipulation of public debt in developed democracies, 1956-90. In: Strauch, R., von Hagen, J. (Eds.), Institutions, Politics and Fiscal Policy. Vol. 2 of ZEI Studies in European Economics and Law. Springer US, pp. 61–83.

Galeotti, A., Salford, G., 2001. Electoral cycles: Do they really fit the data? Tinbergen Institute Discussion Paper 2001/76, Tinbergen Institute.

Golinelli, R., Momigliano, S., 2006. Real-time determinants of fiscal policies in the euro area. Journal of Policy Modeling 28 (9), 943–964.

Gonzalez, M. d. l. A., 2002. Do changes in democracy affect the political budget cycle? Evidence from Mexico. Review of Development Economics 6 (2), 204–24.

Hagen, T., 2007. Estimating the effect of parliamentary elections on primary budget deficits in OECD countries. Economics Bulletin 8 (8), 1–5.

Hallerberg, M., de Souza, L. V., Clark, W. R., 2002. Political business cycles in EU accession countries. European Union Politics 3 (2), 231–250.

Hanusch, M., 2012. Mooted signals: economic disturbances and political budget cycles. Journal of Applied Economics 15 (2), 189–212.

Hanusch, M., Keefer, P., 2014. Younger parties, bigger spenders? Party age and political budget cycles. European Economic Review 72 (C), 1–18.

Hanusch, M., Vaaler, P. M., 2013. Credit rating agencies and elections in emerging democracies: Guardians of fiscal discipline? Economics Letters 119 (3), 251–254.

Havránek, T., 2015. Measuring intertemporal substitution: The importance of method choices and selective reporting. Journal of the European Economic Association forthcoming.

Hedges, L., Olkin, I., 1985. Statistical Methods for Meta-analysis. Orlando: Academic Press.

Jones, M. P., Meloni, O., Tommasi, M., 2012. Voters as fiscal liberals: Incentives and accountability in federal systems. Economics and Politics 24 (2), 135–156.

Jong-A-Pin, R., Sturm, J.-E., de Haan, J., 2012. Using real-time data to test for political budget cycles. KOF Working Papers 313.

Kaplan, S. B., Thomsson, K., 2014. The political economy of sovereign borrowing: Explaining the policy choices of highly indebted governments. Working Papers 2014-10, The George Washington University, Institute for International Economic Policy.

Katsimi, M., Sarantides, V., 2012. Do elections affect the composition of fiscal policy in developed, established democracies? Public Choice 151 (1), 325–362.

Khemani, S., 2004. Political cycles in a developing economy: Effect of elections in the Indian States. Journal of Development Economics 73 (1), 125–154.

Klašnja, M., June 2008. Electoral rules, forms of government, and political budget cycles in transition countries. Panoeconomicus 55 (2), 185–218.

Klomp, J., de Haan, J., 2013a. Conditional election and partisan cycles in government support to the agricultural sector: An empirical analysis. American Journal of Agricultural Economics 95 (4), 793–818.

Klomp, J., de Haan, J., 2013b. Do political budget cycles really exist? Applied Economics 45 (3), 329–341.

Klomp, J., de Haan, J., 2013c. Political budget cycles and election outcomes. Public Choice 157 (1-2), 245–267.

Klomp, J., de Haan, J., 2013d. Popular protest and political budget cycles: A panel data analysis. Economics Letters 120 (3), 516–520.

Kouvavas, O., 2013. Political budget cycles revisited, the case for social capital. MPRA Paper 57504, University Library of Munich, Germany.

Kraemer, M., 1997. Electoral budget cycles in Latin America and the caribbean: Incidence, causes, and political futility. Research Department Publications 4084, Inter-American Development Bank, Research Department.

Kydland, F. E., Prescott, E. C., 1977. Rules rather than discretion: The inconsistency of optimal plans. Journal of Political Economy 85 (3), 473–91.

Lee, J., Laxton, D., Kumhof, M., Freedman, C., 2009. The Case for Global Fiscal Stimulus. IMF Staff Position Notes 2009/03, International Monetary Fund.

Maurel, M., 2006. The Political business cycles in the EU enlarged. Université Paris1 Panthéon-Sorbonne (Post-Print and Working Papers) halshs-00267475, HAL.

Mink, M., de Haan, J., 2006. Are there political budget cycles in the Euro area? European Union Politics 7 (2), 191–211.

Morozumi, A., Veiga, F. J., Veiga, L. G., 2014. Electoral effects on the composition of public spending and revenue: evidence from a large panel of countries. Discussion Papers 2014/16, University of Nottingham, Centre for Finance, Credit and Macroeconomics (CFCM).

Mosley, P., Chiripanhura, B., Jan. 2012. The African political business cycle: Varieties of experience. Working Papers 2012002, The University of Sheffield, Department of Economics.

Mourão, P. R., 2011. Has trade openness already voted? A panel data study. Emerging Markets Finance and Trade 47 (5), 53–71.

Nordhaus, W. D., 1975. The political business cycle. Review of Economic Studies 42 (2), 169–90.

Nyblade, B., O'Mahony, A., 2014. Playing with fire: Pre-electoral fiscal manipulation and the risk of a speculative attack. International Studies Quarterly 58 (4), 828–838.

Paldam, M., 1979. Is there an election cycle? A comparative study of national accounts. Scandinavian Journal of Economics 81 (2), 323–42.

Parra, S. N., Santiso, J., 2009. Revisiting political budget cycles in latin america. OECD Development Centre Working Papers.

Peltzman, S., 1992. Voters as fiscal conservatives. The Quarterly Journal of Economics 107 (2), 327–61.

Persson, T., 2002. Do political institutions shape economic policy? Econometrica 70 (3), 883–905.

Persson, T., Tabellini, G., 2003a. Do electoral cycles differ across political systems. Working Papers 232, IGIER (Innocenzo Gasparini Institute for Economic Research), Bocconi University.

Persson, T., Tabellini, G., 2003b. The Economic Effects of Constitutions. What do the data say? Cambridge MA: The MIT Press.

Potrafke, N., 2007. Social expenditures as a political cue ball? OECD countries under examination. Discussion Papers of DIW Berlin 676, DIW Berlin, German Institute for Economic Research.

Potrafke, N., 2010. The growth of public health expenditures in OECD countries: Do government ideology and electoral motives matter? Journal of Health Economics 29 (6), 797–810.

Rogoff, K., 1990. Equilibrium political budget cycles. American Economic Review 80 (1), pp. 21–36.

Sakurai, S., Menezes-Filho, N., 2008. Fiscal policy and reelection in Brazilian municipalities. Public Choice 137 (1-2), 301–314.

Schuknecht, L., 1996. Political business cycles and fiscal policies in developing countries. Kyklos 49 (2), 155–70.

Schuknecht, L., 2000. Fiscal policy cycles and public expenditure in developing countries. Public Choice 102 (1-2), 115–30.

Shelton, C., 2014. Legislative budget cycles. Public Choice 159 (1), 251–275.

Shi, M., Svensson, J., 2003. Political budget cycles: A review of recent developments. Nordic Journal of Political Economy 29, 67–76.

Shi, M., Svensson, J., 2006. Political budget cycles: Do they differ across countries and why? Journal of Public Economics 90 (8-9), 1367–1389.

Stanley, T. D., 2001. Wheat from chaff: Meta-analysis as quantitative literature review. Journal of Economic Perspectives 15 (3), 131–150.

Stanley, T. D., 2008. Meta-regression methods for detecting and estimating empirical effects in the presence of publication selection. Oxford Bulletin of Economics and Statistics 70 (1), 103–127.

Stanley, T. D., Doucouliagos, H., 2012. Meta-regression Analysis in Economics and Business. London: Routledge.

Stanova, N., 2009. Are there political fiscal cycles in NMS? Working papers, University of Antwerp, Faculty of Applied Economics.

Streb, J. M., Lema, D., Garofalo, P., 2012. Temporal aggregation in political budget cycles. Journal of Lacea Economía 11 (1), 39–78.

Streb, J. M., Lema, D., Torrens, G., 2009. Checks and balances on political budget cycles: Cross-country evidence. Kyklos 62 (3), 426–447.

Troeger, V., Schneider, C. J., 2012. Strategic budgeteering and debt allocation. CAGE Online Working Paper Series 85, Competitive Advantage in the Global Economy (CAGE).

Tujula, M., Wolswijk, G., 2007. Budget balances in OECD countries: What makes them change? Empirica 34 (1), 1–14.

Vergne, C., 2009. Democracy, elections and allocation of public expenditures in developing countries. European Journal of Political Economy 25 (1), 63–77.

Wittman, D. A., Weingast, B. R., Franzese, R. J., Jusko, K. L., 2006. Political Economic Cycles. Oxford: Oxford University Press.

Wright, J., 2011. Electoral Spending Cycles in Dictatorships. Manuscript, Pennsylvania state university.

Appendix A. List of studies and descriptive statistics

Table A.10: List of studies

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

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 effective reference category for elections.

Table A.11: Descriptive statistics [Repartition of fiscal outputs]

	No. of estimates	No. of papers	No. of papers focus
Spending	535	33	9
Total spending	276	24	2
Sub-components	259	20	7
Revenues	354	24	2
Total revenues	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 papers using a given category of fiscal output. Column 3 indicates the number of paper using exclusively a given category of fiscal output.

Table A.12: Descriptive statistics [Timeline]

Year of publication	No. of published articles	No. of working papers	No. of estimates	Average partial correlation	Median partial correlation	Lower bound	Upper bound
1992	1	0	3	0.133	0.130	0.123	0.145
1993	1	0	8	0.106	0.116	0.047	0.158
1996	1	0	3	0.149	0.145	0.069	0.232
1997	0	1	5	0.094	0.093	0	0.178
2000	2	0	7	0.068	0.081	-0.116	0.145
2001	0	1	4	0.085	0.089	-0.074	0.168
2002	3	0	26	0.064	0.050	-0.213	0.386
2003	1	2	102	0.045	0.042	-0.167	0.330
2004	2	0	8	-0.003	-0.080	-0.158	0.319
2005	1	0	45	0.061	0.043	-0.041	0.168
2006	4	1	47	0.063	0.070	-0.227	0.192
2007	2	1	17	0.087	0.097	0.013	0.141
2008	2	0	20	-0.046	-0.068	-0.260	0.212
2009	1	3	159	0.127	0.111	-0.291	0.652
2010	1	0	24	0.087	0.086	0.034	0.127
2011	2	1	158	0.035	0.043	-0.094	0.122
2012	4	3	234	0.060	0.050	-0.198	0.416
2013	7	3	160	0.051	0.057	-0.071	0.222
2014	3	3	301	0.050	0.072	-0.153	0.291
Total/Average	38	19	1331	0.069	0.065	-0.099	0.242

Notes: We compute descriptive statistics on adjusted partial correlation.

Appendix B. Variable definitions

Table B.13: 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 partial 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.	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: Measure of elections					
12	ElectDum	BD if elections are captured by electoral dummies.	1,726	0.77	0.42	0	1
13	ElectRat	BD if Elections are captured by ratio *a la* Franzese.	1,726	0.19	0.39	0	1
14	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 elections					
15	ACalendar	BD if adjustment for electoral or fiscal calendar.	1,726	0.18	0.39	0	1
16	AHighest	BD if adjustment on executive election.	1,726	0.50	0.50	0	1
17	AExog	BD if adjustment on predetermined election.	1,726	0.33	0.47	0	1
		Group 4: Other methodology					
18	Samplesize	Number of observations included in the sample.	1,726	719.20	779.59	15	6631
19	Infra	BD if infra annual data used.	1,726	0.05	0.22	0	1
20	EconDynamic	BD if used dynamic panel estimator.	1,726	0.33	0.47	0	1
21	EconOther	BD if used other estimator (used as the base).	1,726	0.67	0.47	0	1
22	SE correction	BD if used SE correction for heteroskedasticity or autocorrelation.	1,726	0.50	0.50	0	1
		Group 5: Model structure					
23	Interactive	BD if author(s) use interactive models.	1,726	0.38	0.48	0	1
24	Subsample	BD if author(s) use subsample technique.	1,726	0.48	0.50	0	1
25	ConstitSamp	BD if subsample on specific constitutional forms.	1,726	0.26	0.44	0	1
26	HighincSamp	BD if subsample on high-income countries.	1,726	0.24	0.43	0	1
27	EstdemocSamp	BD if subsample on established democracies.	1,726	0.17	0.37	0	1
28	HighdemocSamp	BD if subsample on high level democracies.	1,726	0.03	0.16	0	1
29	BadSamp	BD if subsample on other bad-case senarii for PbC.	1,726	0.05	0.22	0	1
		Group 6: Decades					
30	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
31	1980s	BD if data for the 80's.	1,726	0.79	0.41	0	1
32	1990s	BD if data for the 90's.	1,726	0.97	0.18	0	1
33	Recent	BD if data for the 00's and 10's.	1,726	0.86	0.35	0	1
		Group 7: Region					
34	WeJ	BD if Western Europe, neo Europes & (or) Japan were included in samples.	1,726	0.73	0.44	0	1
35	Eeac	BD if countries from Eastern Europe & Central Asia were included in samples.	1,726	0.55	0.50	0	1
36	Lac	BD if countries from Latin America & Caribbean were included in samples.	1,726	0.62	0.49	0	1
37	Mena	BD if countries from Middle-east & North Africa were included in samples.	1,726	0.49	0.50	0	1
38	Sap	BD if countries from South Asia & Pacific were included in samples (except Japan).	1,726	0.51	0.50	0	1
39	Ssa	BD if countries from Sub-saharan Africa were included in samples.	1,726	0.49	0.50	0	1
40	Global	BD if at least two regions were included in samples.	1,726	0.62	0.48	0	1
		Group 8: Publications outlet					
41	Public Choice	BD if article is published on Public Choice.	1,726	0.11	0.32	0	1
42	Unpublished	BD for unpublished paper.	1,726	0.49	0.50	0	1
43	Impact factor	Google Scholar (5 years) impact factor of publication.	1,726	25.92	27.20	0	168
44	Before2008	BD if paper is released $\leq$ 2008.	1,726	0.28	0.45	0	1
45	After2008	BD if paper is released $>$ 2008 (used as the base).	1,726	0.72	0.45	0	1
		Group 9: Covariates					
46	GDPpc.	BD for per capita GDP as control.	1,726	0.76	0.43	0	1
47	Trade	BD for trade as control.	1,726	0.57	0.50	0	1
48	PopStruct	BD for population structure as control.	1,726	0.61	0.49	0	1
49	OG	BD for output gap as control.	1,726	0.51	0.50	0	1
50	Partisan	BD for partisan measure (such as political ideology) as control.	1,726	0.13	0.33	0	1
51	Time	BD for time dummies or time trend as control.	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.