Political Budget Cycles: Manipulation from Leaders or Manipulation from Researchers? Evidence from a Meta-Regression Analysis

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Abstract

Despite a long history of research on political budget cycles, their existence and magnitude are still in question. By conducting a systematic analysis of the existing literature we intend to clarify the debate. Based on data collected from over 1.700 regressions and 57 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 differs 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

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1. Introduction

Whether elected leaders use their incumbency advantage to distort policy-making to serve their own interests is a central concern in political economics. In particular, leaders may adopt strategic-timing decisions in a way that help them to hold office. Since Nordhaus (1975), a large -and still increasing- number of studies have scrutinized 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's (1979) 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).[1]

Since cycles are not different from shocks affecting budget, they are likely to hurt the economy (Fatás and Mihov, 2003). Smoothing the policy-making over the representatives' terms should offer more economic stability and benefit the broader interest (Alesina and Perotti, 1994). 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 build institutions that benefit the populations' will.

Considering the substantial size of the so-called political budget cycles (PbCs) literature, researchers have regularly offered some literature reviews (Shi and Svensson, 2003; Eslava, 2011; de Haan and Klomp, 2013).[2] 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 from 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 in-

[1] Cazals and Sauquet (2015), or Dubois (2016) argue that cycles are more likely to occur on instruments, such as fiscal policy, rather than on outcomes of economic policy, such as employment, growth or inflation. Their suggestion would explain why the literature has been increasingly focused on political budget cycles rather than political business cycles.

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

sight. In other words, classic narrative reviews are not able to resolve nor explain apparent divergences among scholars' empirical findings in a rigorous way.

In an attempt to offer a clearer picture of the vast amount of research on the PbCs literature, we provide a meta-regression analysis (MRA). MRA allows going beyond the two limits of literature 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 as 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 into the considered issue, 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 57 papers. All these papers share specific inclusion characteristics.[3] In particular, they all provide a cross-country analysis of how the level of any budget variable (share or total of public revenue, public expenditures, or fiscal surplus) is modified around elections.[4] We then coded the 1,726 regressions extracted from our collection and built our own original dataset summarizing the empirical findings from the PbCs literature . This paper presents the results of their statistical analysis. The MRA developed suggests that in average leaders do manipulate the 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 provides preliminary evidence. Section 5 discusses our approach and the methodology. Sections 6 to 10 present the MRA results and the last section concludes.

2. Theoretical predictions

A meta-analysis offers a toolkit allowing in the best case to solve conflicting theoretical and/or empirical findings on a specific research question.[5] Concerning PbCs, the theory is

[3] see Section 3.1.

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

[5] One of the most famous example refers to the labor market effects of the 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).

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).[6] Empirical findings are however more contrasting. In particular, the existence and magnitude of such predicted cycles seem to vary according to factors such as geography, or institutional settings. In other words, the manifestation of PbCs 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. Another concern is the ability to properly observe such manipulation, which is by nature a hidden phenomenon, through the use of "too much" aggregated data across time and space (Labonne, 2016).

In addition, some scholars question the effectiveness of such strategic manipulation. Several country-specific studies show that incumbents that resort to PbCs 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.[7] If voters punish rather than reward incumbents running fiscal expansions before elections, then there should be no point adopting 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, PbCs are more pronounced in developing countries (Shi and Svensson, 2006), regimes with low levels of democracy (Gonzalez, 2002)[8] 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 set of predefined objective criteria. The second and most time-intensive step is the coding of the estimates encompassed in theses studies resulting in a dataset ready to be

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

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

[8] Gonzalez (2002) finds higher PbCs during the democratization process of Mexico, i.e. when Mexico had a low-level democratic framework.

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 does not rely on primary data such as GDP or household income. Rather we must dedicate much care and effort to build our own dataset, which is critical to ensure its quality and the consistency of our results.

3.1. How did we select the studies?

The present meta-analysis relies on 1,331 estimates (selected over a total of 1,726) with available partial correlations, extracted from 57 papers (see Table A.8). This selection is the result of an extensive search and the adoption of restrictive requirements.

First, we implement 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 fall through the cracks, we undertake a manual complementary search. First, we look for additional studies in the references listed in the papers already selected. Second, we check the publications and working papers of the authors identified in the first round.[9] Finally, we attempt to be as exhaustive as possible, with alternative web engines. 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, as described hereafter.

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[10] that are based on at least two countries. Single-country regressions often dig 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 are likely to yield estimates that are too country-specific to be comparable with estimates stemming from

[9] This manual search only reveals few supplementary papers, hence accrediting the effectiveness of the electronic round of the selection.

[10] 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.

cross-sectional papers. Moreover, they would have implied dissecting 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. Unsurprisingly however, we met a limited number of such estimates during our data collection. A consistent estimate of PbCs requires a large number of elections, and thus in the case of a single-country estimate a large temporal dimension. In turn, this leads to a problem of data availability and reliability. This methodological choice slightly 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 are 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.[11]

Additionally, we only consider papers which focus on how electoral periods affect the level of either national deficits, revenue or expenditure, 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.[12] 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 strictly before January 1^{st} 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

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

[12] This is typically the case of Brender and Drazen's (2013) 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 analysis.

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 featuring a fiscal output variable as the 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 changes in budget composition (Ashworth and Heyndels, 2002; Brender and Drazen, 2013, for instance). Therefore, we also discard cases where the dependent variable is a ratio of a sub-component of a budget variable over this budget variable. For instance, in some regressions, Katsimi and Sarantides (2012) use the ratio "current (or capital) spending / total spending". In such cases, we observe the variation of the ratio but we are not able to identify 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) expenditure is affected by the closeness of elections. Similar cases of composition-related regressions that were excluded from our sample may be found in Chang (2008), Vergne (2009), or Klomp and de Haan (2013a), among others. Some papers use cyclically adjusted measures (Golinelli and Momigliano, 2006; Stanova, 2009; Mourão, 2011, for instance), but interestingly none relies on a discretionary measure of fiscal output, with the exception of Buti and Van Den Noord (2004).

The literature splits budget variables into three, even if numerous studies compare (successively) the effect of elections on all three of them. A first set of estimates we code focuses on expenditure patterns. Most of them rely on the level of public expenditure divided by GDP (275 estimates over 1,331). However, we find eight other finer measures based on sub-components of expenditure, in particular current, capital, broad, and local spending. All these four 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 expenditure respectively. Voters are expected to be more sensitive to current rather than capital expenditure as their effects are more tangible in the short term. Therefore leaders should be more prompt in increasing current spending in pre-electoral period. According to the prevailing electoral system, 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 revenue according to electoral periods. Once again, authors' favorite variable is aggregate public revenue over GDP (196

estimates out of 1,331). We include estimates using 13 other measures that are sub-components of overall revenue, normalized by 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 Tables A.9 and A.10.[13]

The third set contains estimates of how elections impact the national budget surplus, which is obtained by subtracting public expenditures from public revenue. Numbers of studies focus on deficits rather than surpluses. In this case, we multiply estimate values by minus one (-1) so that this last set contains only estimates of the effect of elections on budget surplus over GDP.

Finally, 913 estimates over 1,331 use one of the three main variables, that is the nominal value of either surplus, expenditure or revenue on GDP. The number of papers focusing on one category of fiscal ouptut is relatively limited (except when considering fiscal surplus, see Tables A.9 and A.10 for more details). Unsurprisingly, the analysis based on the whole sample (1,331 estimates) the one based on the reduced sample (913 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 capture 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 the value of one in years during which an election occurs, or alternatively the year before it takes place.[14] 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 year rather than several 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 the period of the year during which they occur (Brender and Drazen, 2005; Mink and de Haan, 2006).[15] Another class of refinements is pioneered by Franzese (2000). With this method, the electoral variable is intended to measure how much of a given year may actually been considered as pre-electoral. Considering an election taking place

[13] A list of each considered sub-component of spending and revenue is available upon request.

[14] Even though most authors are interested in pre-electoral 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).

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

during the m^{th} 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 ones 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 predetermined 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 parliamentary and presidential ones.[16] Facing the arbitrariness of the choice, some authors such as Fatás and Mihov (2003) do not distinguish between the two types and pay attention to 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).[17] Yet, according to constitutional design of countries, one kind of elections or the other may exert greater forces on the policy-making and thus may be more relevant regarding the issue of political cycles. A last group of authors thus chose to use what is considered as the highest election according to the country (Shi and Svensson, 2006, for instance): parliamentary elections for parliamentary systems and presidential elections for presidential systems.

4. At first glance

Because they offer a first answer to the economic question raised at a glance, funnel graphs have become very popular in MRA. In our framework, a funnel graph consists in plotting a measure of the precision of the estimates of election effect on fiscal aggregates (vertical axis) against these estimates collected from the literature (horizontal axis). Most of the time, precision is measured by the inverse of the standard error of the estimate ($\frac{1}{SE}$). 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, more precise estimates appear to be concentrated around a precise value. This value is supposed to reflect the "true" genuine effect of elections on fiscal

[16] In semi-presidential systems, parliamentary elections are called legislative elections. Interestingly semi-presidential systems are generally treated as parliamentary systems in the literature, due to the vote of confidence for governments (Persson and Tabellini, 2003b, for instance).

[17] Others adopt mixed strategies by retaining only presidential systems and focusing on presidential elections (Block, 2002; Hanusch and Vaaler, 2013), or parliamentary systems and considering parliamentary elections (Bayar and Smeets, 2009).

aggregates. If the distribution is centered on zero, we should conclude that elections have no effect on such aggregates. A second information we may infer from such graphs is the potential selection bias from the literature. In the absence of such a bias, points should be symmetrically distributed around this "true" effect. In short, the funnel graphs displayed in Figures 1 to 4 show us that leaders manipulate budget before elections and that researchers make this effect bigger than it actually is.

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

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

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 correlations are weakly sensitive to imprecise degrees of freedom calculations (Stanley and Doucouliagos, 2012); this uncertainty is especially marginal as sample sizes in the PbCs literature consist of hundreds even thousands of observations. This standardization removes the economic meaning of the effects but is still informative on the magnitude and sign of associations between election and fiscal aggregates and makes them quantitatively comparable. By way of robustness, we also implement the widely used Fisher's partial correlation transformations.[18]

Figure 1 reports the distribution of partial correlations (left panel) and Fisher's partial correlations (right panel) between elections on public spending based on 535 regressions coded. Consistent with the theory, the "true" value of the manipulation of 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 clearly right-skewed distribution. Not so similar conclusions can be drawn from the funnel graph of correlations between elections and public revenue (Figure 2). The distribution is not skewed to the left suggesting a selection bias in favor of results reporting a reduction in public revenue in pre-electoral periods. However, the "true" value of manipulation of this aggregate is here less clearly on the left of the zero-line. This may translate a lesser ability of leaders to modify tax rates and/or a weak sensitivity of

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

voters to revenue cuts. Since leaders tend to increase spending and slightly reduce revenue when elections get close, Figure 3 unsurprisingly reveals a degradation of the budget surplus preceding ballots. Selection bias appears, once again, distinctly. Figure 4 offers a global view on leaders' manipulation of fiscal tools. In this figure we recode the correlations involving revenue and surplus by multiplying them by minus one (-1), so that we can consistently combine the three previous graphs into one funnel graph. It provides further evidence regarding a "true" manipulation from leaders, together with a positive skweness of the distribution (0.31), translating selection bias from scholars.

Then, we look at the distribution of the t-statistics of the estimates reported in the literature. Figure 5 shows that t-statistics are concentrated around two (2) in absolute value consistent with the expected sign 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 5 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.

Before turning to a more rigorous statistical and quantified analysis, graphical tools can (still) offer us additional information on these two insights. Concerning the magnitude of leaders' manipulation, a chronological ordering of the mean estimate of partial correlations -from each paper of our collection- is provided in Figure 6 and reveals a clear declining trend in time. Earlier papers report stronger budget manipulation than more recent studies. The decline is strong since the mean estimate is more than halved over $1992 - 2014$.[19] This result may have two main sources, or combination of both. First, budget manipulation may have declined over time, and more recent papers, 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.

[19] Interestingly, the 2008 trough is actually driven by the estimates from one paper whose electoral variable capturing PbCs is not the variable of interest but control variable. At this stage however, we cannot know if this translates a lesser attempt to find a large PbCs effect or a model specification not suitable to properly identify that precise effect.

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

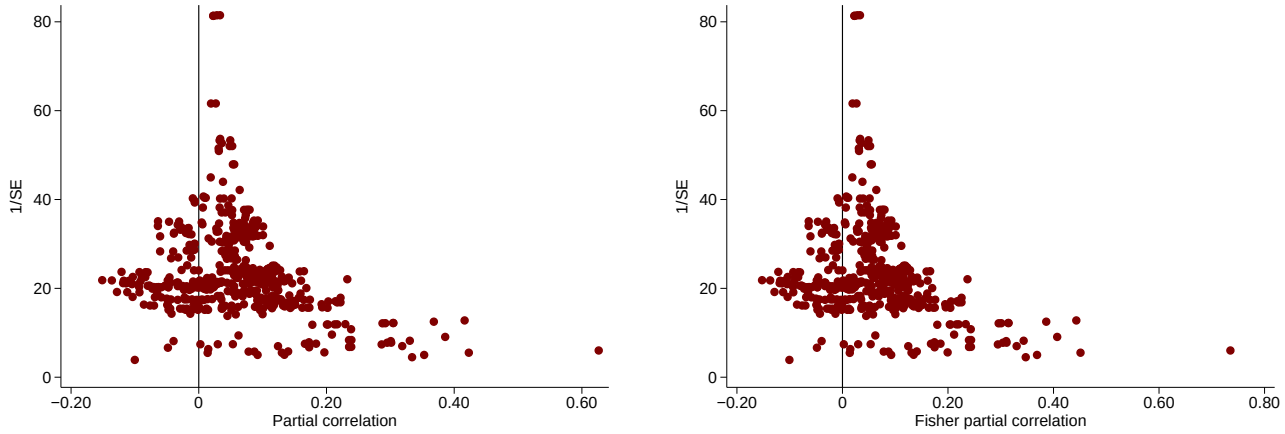


Figure 2: Funnel plot of election-on-revenue partial correlations ($n = 354$)

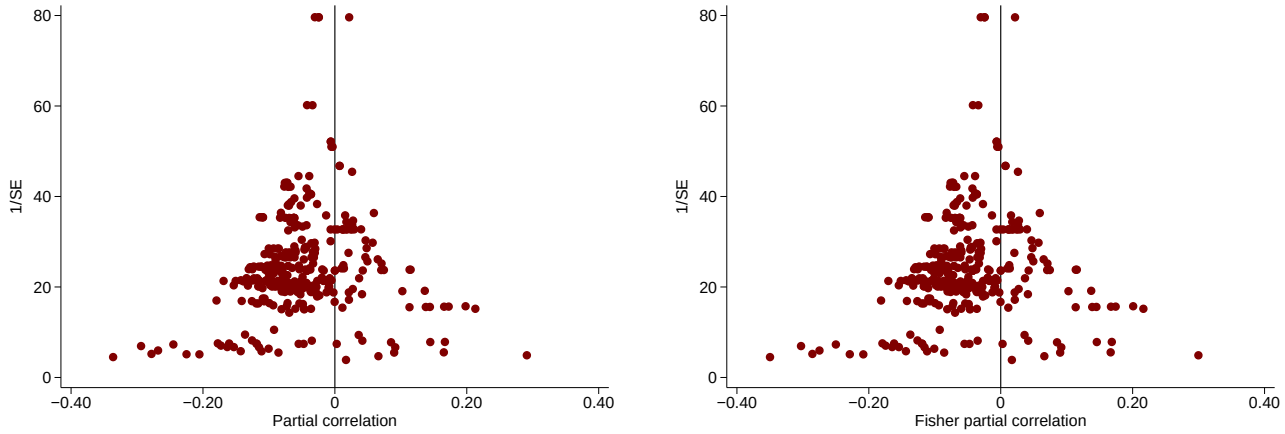


Figure 3: Funnel plot of election-on-fiscal surplus partial correlations ($n = 442$)

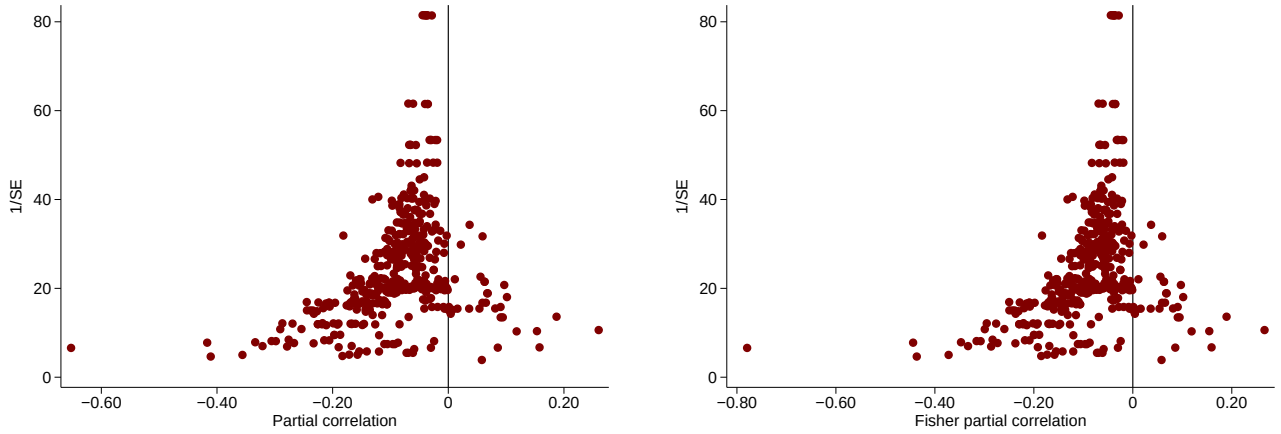


Figure 4: Funnel plot of election-on-all fiscal output (adjusted) partial correlations ($n = 1,331$; $skewness = 0.31$)

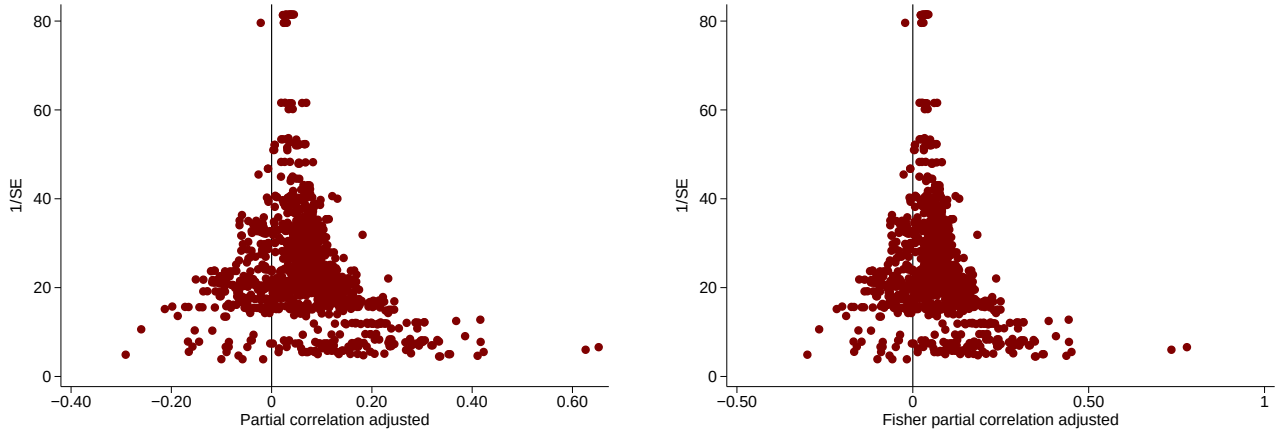
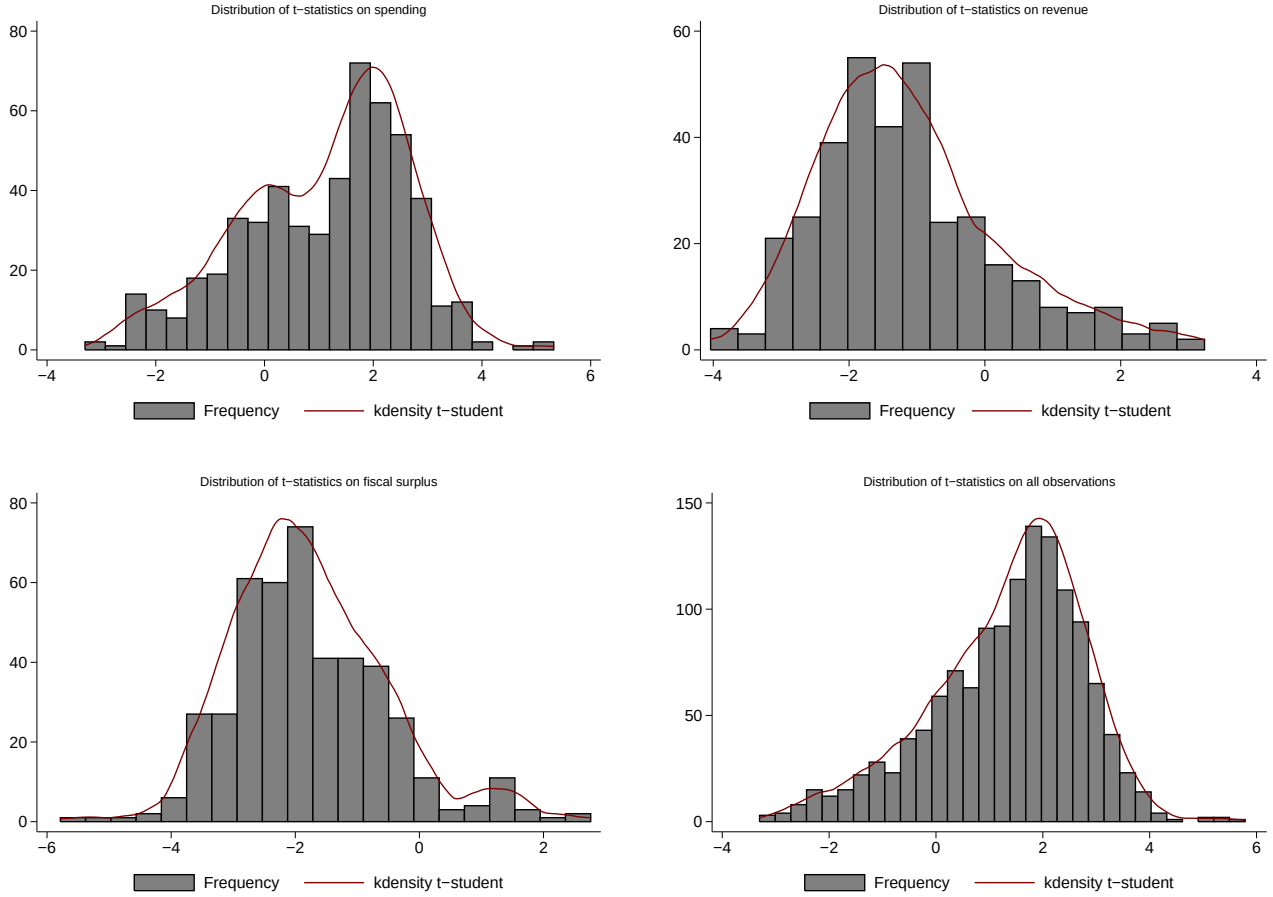


Figure 5: Distribution of t-statistics



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

The graphical analysis conducted in this section seems to suggest evidence of manipulation from both political leaders and researchers. The following section relies on a more rigorous statistical analysis that allow digging deeper the findings, dissect the effects and quantify them more precisely.

5. MRA methodology

To gain more rigorous insights into the manipulation controversy, 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 intercept 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 testing 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 zero, would reveal a tendency to favor estimates with a certain t-statistic, likely exceeding the standard threshold of statistical significance.[20] Such a tendency is acknowledged as publication selection. Its causes are numerous and deeply anchored to academic incentives of scholars (Stanley and Doucouliagos, 2012) who tend to 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 favor results reporting incumbents' strategic behavior and thus we expect some positive and significant association between the magnitude of the manipulation and its standard error, as suggested by the right-skewed funnel graph in Figure 4.[21] If so, the literature would be biased and the budget-distording effect of elections overestimated.

Since Equation (2) controls for potential publication selection, its intercept, β_0 reveals the genuine effect of elections, if any. This effect is not anymore inflated or distorted by researchers'

[20] Card and Krueger (1995) show that the t-statistics of studies assessing the effect of the minimum-wage on employment gravitate around two, approximation of the statistical significance at the usual 5% confidence level. Similar conclusions can be drawn from empirical effects reported by best journals (Brodeur et al., 2016).

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

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 to what extent 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 their existence and magnitude. To examine the conditional nature of PbCs inherent to all socio-economic phenomenon, we then turn to multiple meta-regressions. We augment 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 assessing 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.12. We organize them in nine categories: measure of fiscal output; measure of the election variable; adjustments on the election variable; methodology employed; 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_1) and precision-effect (β_0) tests. In Table 1, we combine all fiscal outputs to observe leaders' manipulation in general. Panel (i) reports the results on all available observations. In panel (ii) we exclude observations dealing with subcomponents of revenue and spending. In other words we remove cases of what the literature refers to as "pork barrel" (Drazen and Eslava, 2006) to solely focus on manipulation of the broad fiscal outputs.[22] Finally, panels (iii) and (iv) exclude conditional PbCs, captured with interactive models or sub-sampling. 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 the heteroskedasticity issue, WLS are suitable for MRA and routinely employed by researchers (Stanley and Doucouliagos, 2012). WLS do not treat all observations equally and assign more

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

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.[23]

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 0.021 to 0.033. The size of 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 PbCs. 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. Similar to corruption, 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 Table 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 statistically significant. Ranging from 0.638 to 0.917, this effect can be considered as substantial based on Doucouliagos and Stanley's (2013) guidelines. This suggests that results reported by researchers in this literature are fairly inflated. We do not claim that scholars always and deliberately manipulate their results. We observe a general tendency to over-report some specific results, that leads us to conclude that the results from the PbCs literature are on average distorted even manipulated by researchers.[24] Somewhat ironically, we also note that the magnitude of the research bias is actually much bigger than the one associated to budget manipulation by leaders.[25]

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

[24] Interestingly, the same logic applies for leaders. We only observe an average tendency that does not imply that all leaders use budget manipulation every time and every where. Furthermore, a budget deterioration before elections may have sound economic grounds in some cases, but this phenomenon occurs (too) frequently to be pure coincidence or driven for or by economic reasons.

[25] Research bias is pervasive and present in most bodies of literature within and beyond economics. To put our findings in perspective, although the publication selection bias is substantial, many MRA present magnitude of bias in other fields of research even much stronger (see Stanley and Doucouliagos (2012)).

Finally, based on the heterogeneous existing estimations to date and once the selection bias removed, it appears that on average there is still a small but statistically robust evidence of manipulation. This first result answers the debate around the existence of PbCs, but is mute when it turns to explain what are their origins.

[insert Table 1]

7. Where do PbCs come from?

Table 2 displays the FAT-PET results for each group of fiscal output selected as the dependent variable. We report results for a broad measure of spending encompassing all types of expenditure (panel (i)), and a narrow measure of spending excluding subcomponents of total spending (panel (ii)). We apply the same logic to revenue in panels (iii) and (iv). Finally, panel (v) reports results when fiscal surplus is used to capture fiscal cycles. Again, the results show strong evidence of publication selection bias consistent with: the effect of elections is positive on spending and negative on revenue and fiscal surplus. Interestingly, we do not find strong evidence of pre-electoral manipulation on revenue and spending (panel (i) to panel (iv)), but we do find a statistically significant and robust manipulation on fiscal surplus. This may suggest possible heterogeneity in the strategies employed by leaders. Depending on the political easiness and pay-off that leaders face in their countries, they may favor manipulation of spending rather than revenue. 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 reflects the opportunism of leaders although the fiscal tools they leverage are different.[26]

To assess even more finely the behavior of leaders, we offer to go one step further and look inside each box: expenditure and revenue. Table 3 reports the estimated effect of electoral manipulation by disaggregating the different fiscal tools. Columns 1 and 2 present results when revenue are used, and columns 3 and 4 when spending are used. In even columns we use the fixed effects multilevel (FEML) estimator that includes dummy variable for individual authors

[26] This result echoes back the work of Barberia and Avelino (2011) on Latin American countries.

to take into account unobserved heterogeneity among authors in the PbCs literature, with less bias than random weighted average (Stanley, 2008; Stanley and Doucouliagos, 2012). In every case, total revenue 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 the national level, relative to studies at the municipality level. Regarding revenue, seminal contributions of Ashworth and Heyndels (2002); Efthyvoulou (2012) and Katsimi and Sarantides (2012), focus on OECD countries, while Block (2002) and Ehrhart (2013) study manipulation of revenue composition in developing countries. We split revenue into direct taxes (such as income taxes, payroll taxes, property taxes), external taxes (international trade taxes), indirect taxes (value added tax (VAT), general sales tax (GST)) and non-tax revenue (especially social security revenue, goods and services revenue, government borrowing). The MRA results are unconclusive about the strategic use of specific revenue categories by political leaders during electoral race. So, as recalled by Alesina et al. (1989), political leaders may prefer avoiding tax reforms before election to maintain social order.

Regarding manipulation of spending composition, some articles contrast 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). We adopt a similar methodology, by distinguishing capital spending from current spending and broad public goods from local public goods. The MRA reports 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 when the short-run benefits are not strong enough and reallocate the amount thus "saved" to expenditure categories that offer them a greater and immediate political pay-off.

Our results provide evidence of composition effects in line with Vergne (2009) for instance. Leaders appear to relax the budget constraint 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 PbCs too important or perceptible is likely to be punished by voters (Brender and Drazen, 2007). A way of bypassing the budget constraint is then to manipulate the composition of public spending. Thus leaders appear to manipulate both the level and

composition - at least the expenditure side - of budget. They have faced with two strategies they may use as complement or substitute according to their power extent over the policy-making and the political reward they expect from each strategy.

Finally, the MRA results uncover preferences of manipulation of broad public good spending in pre-electoral periods. 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.[27] In addition, political leaders may prefer giving satisfaction to 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 the literature on higher capital spending and local public goods in pre-electoral period at the 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.

[insert Table 2 and Table 3]

8. The declining magnitude of PbCs

As revealed by Figure 6 studies have continuously reported PbCs of lesser magnitude over $1992-2014$. One may wonder if this declining trend is due to a lesser manipulation from leaders or researchers. In the first case, we may imagine that the development of democracy and political institutions accompanied with a greater voters' information and experience, have led to a lesser ability and willingness of leaders to distort budget before elections. In the second case, we may imagine that the adoption of better practices by researchers, supported by the development of suitable econometric tools, stronger requirements and scrutiny in the publication process or even benefiting form larger sample size datasets with time have led and/or allowed researchers to estimate PbCs in a less biased fashion.

[27] 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).

In order to answer this question, we split our sample in four sub-samples. The first sub-sample encompasses observations collected from papers released in 2000 or before. The second sub-sample gathers studies form 2001 to 2005, the third one from 2006 to 2010 and the last one from 2011 to our search limit that is January 1^{st} 2015. We then run Equation 2 on each of these four sub-samples.

The results presented in Table 4, suggest that both the magnitude of PbCs and research bias have been declining. We remain cautious in our interpretation since column 1 shows no publication selection bias for early studies and column 2 no occurrence of PbCs in studies released between 2001 and 2005. However the overall tendency over the four columns points to enforce that the magnitude of research bias has declined by about one third since 2000, and the magnitude of budget manipulation itself has been divided by a factor of four or five since the pioneer empirical findings on PbCs.

[insert Table 4]

9. Country characteristics

In the literature, several authors argue that the existence of PbCs depends on country characteristics. The MRA allows exploring which factors determine (or promote) 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, the quality and age of democracy, and constitutional settings. More precisely, we augment the model given in Equation 2 by including a dummy equaling one for estimates from regressions containing only observations for which the conditional factor is present. For instance, the "*Low-income countries*" dummy takes the value of one for estimates computed on the sample of low-income countries only. We present the results in Table 5.[28]

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 5 show that this view is not supported by the meta-data.

[28] We consider methodologies from the World Bank, to capture economic development status, using the last year of estimates for each regression, as reference, Brender and Drazen (2005) to capture the 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 do not use their own classification rules. Results are robust to applying a common classification methodology for all papers.

The coefficients associated with the dummies bear the expected sign but are not statistically significant. We do not detect any difference in the level of manipulation between low- and high-income countries. Another debate in the literature concerns the effect of democracy on the occurrence of PbCs. Some authors such as Gonzalez (2002) and Block et al. (2003) argue that the strength of democracy is negatively correlated with 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. Subsequently, we examine both the effect of the 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 clearly conclude which democracy characteristic dominates. And the debate stays open. The 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 matter. Even when voters are experienced, a degradation of the democracy level may offer a leader a greater room for manipulation, and conversely.[29] Additionally, we investigate 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 generate PbCs in parliamentary systems relative to presidential ones, and in majoritary voting systems relative to proportional representation. Regarding the form of government or the electoral rules, the last four columns of Table 5 show that on average leaders do not behave differently regarding their level of budget manipulation according to the constitutional design.[30]

[29] Additional descriptive statistics on PbCs magnitude, conditional on geographic regions and political regimes are provided in supplementary appendix.

[30] A finer analysis of the manipulation of budget components (revenue vs expenditure 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 the meta-level.

Finally, only the strength and the age democracy appear to significantly affect the level of PbCs. In particular, younger and low-level democratic regimes are characterized by PbCs of greater magnitude. The MRA additionally reveals that other factors discussed in the literature do not systematically affect budget manipulation, so that cannot be qualified as a condition for the existence of PbCs.

[insert Table 5]

10. Sources of heterogeneities

So far, our analysis has evidenced two main results. First we showed that PbCs do exist but are fairly overstated by researchers. Second we further decompose overall budget in its sub-components and assess how country characteristics may affect the occurrence of PbCs. We now turn to an exhaustive analysis of other potential sources of heterogeneity in the estimates found in the literature. 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.[31] It is not an easy task to precisely identify each source of heterogeneity. However, we believe that result searching and selection bias are even more likely to be captured by the second part of the multiple MRA. That is, when seeking specific results, authors are likely to 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 about which factors may affect the manipulation by leaders, whereas the second part reflects how researchers may manipulate their results. Nonetheless, the border between the two kinds of manipulation is blurred. For each source of heterogeneity we examine (i.e. each covariate of our multiple MRA), both explanations are plausible and compatible. Then we do not pretend our classification allows disentangling precisely the effect from each kind of manipulation. Consequently, we remain cautious in our interpretation.

[31] Given that the choice of decomposing the multiple analysis in two parts may impose a structure on the data and condition our results, we also conduct a bayesian MRA on the whole set of covariates. This investigation offers comparable results with the two-part multiple MRA. Results and a brief discussion are provided in supplementary appendix.

10.1. Data and model specification

We run the multivariate MRA model described by Equation (3). This model allows observing the causes of heterogeneous findings on PbCs in the empirical literature. In particular, this section intends to improve our understanding on how determinants related to the sample and model specification would condition the existence and magnitude of PbCs.

The first four columns of Tables 6 and 7 present the results for the whole sample using the adjusted partial correlations. All the columns but column 2 report the general model with all moderators, following the seminal meta-analysis literature 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 controls for author 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.[32] In contrast, column 2 employs the general-to-specific methodology, whereby MRA explanatory variables which are not significant at the 10% level in column 1 are removed from the estimation in order to have a parsimonious model (Stanley and Doucouliagos, 2012). As the FEML estimator is considered as the most exhaustive, it is our benchmark when interpreting results.

Table 6 reveals a substantial selection bias that disappears when controlling for author fixed effects. This encourages us to explore further sources of strategic manipulation by researchers.[33] It is also worthy to note that the constant, reflecting the genuine effect of elections on budget variations, is larger than the value found in the simple MRA. The coefficient is now above 0.10, that is a medium effect according to Cohen (1988).

Additionally, we do not find strong effects of sample choice. Results simply suggest PbCs are less severe with data covering the 1990s than with data encompassing earlier decades. The difference is statistically significant and economically not negligible.[34] As for spatial differences, we only notice that Western countries and Japan are less affected by pre-electoral cycles. This

[32] Recall there are 24 distinct measures of fiscal outputs that are reported in the 57 studies included in our analysis.

[33] See Section 10.2.

[34] It could reflect the implementation of theories on hysteresis of unemployment (Blanchard and Summers, 1986) and time inconsistency (Kydland and Prescott, 1977; Barro and Gordon, 1983) to policies in the 1990s. The result could also be driven by a couple of papers reporting weak evidence of budget manipulation, using data from the 1990s. Recall that covered decades refer to data, not publication timing (see Section 8).

result is in line with the idea that in older and stronger democracies PbCs are less strong. However, the evidence is not very robust.

We then investigate if the model specification affects the strength of PbCs. In particular we focus on how selecting a subsample would matter. Interestingly, we notice that subsamples on established democracies exhibit significantly reduced PbCs, and results are strongly robust to the introduction of author dummies. In line with Table 3 and the seminal work of Brender and Drazen (2005), we have suggesting evidence that informed and experienced voters signals democratic maturity and reduce the extent of budget manipulation.

[insert Table 6]

10.2. Paper, methodology and covariates

The MRA reported in Table 7 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 about higher PbCs reported in published papers, relative to unpublished ones. Journals would be more prone to select significant over zero-effect results. The coefficient associated with the variable "*Unpublished*" suggests this is not the case. Second, the quality of the paper, proxied, through the Google Scholar five-year impact factor of the review or series in which the paper is published, does not affect the level of manipulation either. We also focus on a specific journal for which political cycles are one of the main topics, namely the Public Choice journal. Despite an editorial boarding focused on decision-makers' strategic behavior, the journal does not seem to publish disproportionately PbCs-friendly articles. Last but not least, we have suggesting evidence of the decreasing trend of electoral manipulation in the PbCs literature, by taking into account the timing of publication (see columns 3 and 4). As the "*Before2008*" coefficient is significant when controlling for author fixed effects, it could imply a genuine decline in PbCs, which may be hidden by scholars. In line with the structural break identified in Figure 6, the timing of publication could also capture the enhancing effect of massive fiscal stimulus -in response to the global recession of $2008-2009$ (Lee et al., 2009)-, on regression results.

A second block of covariates focuses on the methodology employed by authors. More precisely, we look at the measure of the election and fiscal output variables selected, and at the

estimator selected. According to preliminary evidence, the magnitude of the electoral manipulation differs when the dependent variable refers to fiscal surpluses, as underlined by the coefficient on *YSurplus* (columns $1-4$). This entails that PbCs magnitude is related to the nature of fiscal output.[35] Adjustments on fiscal output also matter. Indeed, strategic manipulation is found to be significantly reinforced when using data from central governments, once taking into account authors' unobservable heterogeneity (columns 3 and 4). Adjustments on the electoral calendar, elections for the executive and predetermined elections can be a serious issue in the PbCs literature. Among adjustments on elections, relying only on predetermined election, is associated with less magnitude in PbCs. The 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 results on PbCs. This findings finds support since the effect disappears once we control for author fixed effects. Conversely, correction of standard errors for heteroskedasticity and autocorrelation contributes to decreasing the magnitude of PbCs (columns $1-4$). That is, using more requiring estimators leads to smaller PbCs. Finally, the last block of Table 7 shows that the choice of the vector of covariates is not neutral and may affect the strength of PbCs reported by researchers. Hence, when determining the way they measure their variables of interest, which control variables they include in their econometric model and which methodology and estimator they use to estimate it, authors may increase their expectancy to obtain specific and significant results in order to validate their ideology or theoretical assumptions.[36]

[insert Table 7]

11. Conclusion

Initiated by Nordhaus (1975), the PbCs literature is still flourishing, as empirical findings are not unanimous on the existence and magnitude of such cycles. A couple of narrative reviews helps 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 PbCs-related academic papers

[35] See Section 3.

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

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 elections on fiscal tools.

We conduct our analysis on the 1,331 estimates of PbCs collected from 57 cross-country studies. The MRA reveals a significant selection bias from scholars translating an inclination to exaggerate the magnitude of PbCs. However, after controlling for this overestimation, we still find a slight but statistically robust evidence 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 edge toward the ideal of democracy.

Interestingly, we show that the deterioration of the primary balance before elections is systematic, but evidence of public revenue and spending manipulation is slightly less robust. We attribute this findings to various strategies from leaders, using either spending or revenue, according to the political costs-benefits trade-off they face. By disaggregating public spending, we find that leaders are more prone to manipulating some subcomponents, such as increasing current expenditures relative to capital spending, but also broad public goods, in pre-electoral period. By contrast, incumbents do not systematically target specific subcomponents of revenue 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 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 PbCs 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, as references). Once again, this impels scientists to further research on political cycles and the way to limit them in order to make democracy more effective.

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

Regression/s.e.	(1) FAT Funnel asymmetry		(2) PET Meta-average		N
	(i) All observations (Adjusted Partial Correlation) Double publication removed				
Robust s.e.	**0.775*** **	(0.083)	**0.023*** **	(0.003)	1,331
Clustered s.e.	**0.775*** **	(0.215)	**0.023*** **	(0.006)	1,331
Double clustered s.e.	**0.775*** **	(0.215)	**0.023*** **	(0.007)	1,331
Robust regression	**0.865*** **	(0.086)	**0.024*** **	(0.003)	1,331
	(ii) Excluding "pork-barrel" (Adjusted partial correlation)				
Robust s.e.	**0.889*** **	(0.086)	**0.021*** **	(0.003)	913
Clustered s.e.	**0.888*** **	(0.200)	**0.021*** **	(0.005)	913
Double clustered s.e.	**0.889*** **	(0.230)	**0.021*** **	(0.008)	913
Robust regression	**0.896*** **	(0.087)	**0.023*** **	(0.003)	913
	(iii) Excluding interactions (Adjusted partial correlation)				
Robust s.e.	**0.803*** **	(0.085)	**0.022*** **	(0.003)	1,037
Clustered s.e.	**0.803*** **	(0.163)	**0.022*** **	(0.005)	1,037
Double clustered s.e.	**0.803*** **	(0.205)	**0.022*** **	(0.007)	1,037
Robust regression	**0.917*** **	(0.090)	**0.022*** **	(0.003)	1,037
	(iv) Ecxluding subsample (Adjusted partial correlation)				
Robust s.e.	**0.638*** **	(0.154)	**0.032*** **	(0.005)	583
Clustered s.e.	0.652	(0.449)	**0.032** **	(0.013)	583
Double clustered s.e.	**0.638***	(0.347)	**0.032*** **	(0.012)	583
Robust regression	**0.728*** **	(0.159)	**0.033*** **	(0.006)	583

Notes: Panel (i) reports all observations. Panel (ii) excludes subcomponents of revenue and spending. Panel (iii) excludes interactive models. Panel (iv) excludes subsamples. The first four rows of each panels use the weighted least squares (WLS), with precision squared (inverse variance) used as weights. Clustering on studies, or double clustering on studies and fiscal outputs. Cohen's (1988) 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.	**0.651*** **	(0.159)	**0.013****	(0.005)	275
Clustered s.e.	0.651	(0.450)	0.013	(0.013)	275
Robust regression	**0.578*** **	(0.180)	**0.016****	(0.007)	275
	(iii) Revenue				
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 revenue				
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.	**-1.065*** **	(0.119)	**-0.029*** **	(0.004)	442
Clustered s.e.	**-1.065*** **	(0.299)	**-0.029*** **	(0.007)	442
Robust regression	**-1.216*** **	(0.116)	**-0.025*** **	(0.004)	442

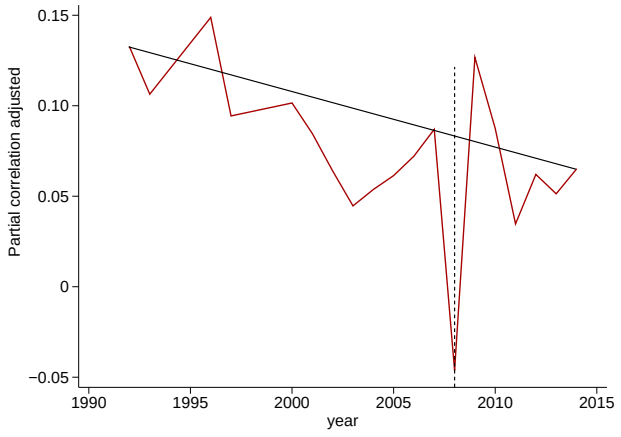
Notes: See Table 1. The dependent variable is the non-adjusted partial correlation between elections and fiscal output. Panel (i) reports observations on spending. Panel (ii) excludes subcomponents of spending. Panel (iii) reports observations on revenue. Panel (iv) excludes subcomponents of revenue.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

Table 3: MRA [Patterns of manipulation]

Variables	Revenue		Spending	
	WLS	FEML	WLS	FEML
Direct Taxes	-0.004	0.007	-	-
External Taxes	-0.006	-0.019	-	-
Indirect Taxes	-0.012	-0.003	-	-
Non Tax Revenue	0.073	**-0.040***	-	-
Current Spending	-	-	**0.041*** **	**0.028*** **
Capital Spending	-	-	**-0.085*** **	**-0.096*** **
Broad Public Good	-	-	**0.042****	**0.048*** **
Local Public Good	-	-	**0.043*** **	0.016
Missing Category	Total revenue		Total Spending	
RMSE	0.048	0.039	0.049	0.038
Adjusted R^2	0.095	0.649	0.335	0.733
Number of cluster	42	42	59	59
FAT/PET	Yes	Yes	Yes	Yes
Authors fixed effect	No	Yes	No	Yes
N	354	354	535	535

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.$*p < 0.10, * * p < 0.05, * * *p < 0.01$.

Figure 6: Average (adjusted) partial correlation of elections, per publication year



Notes: We compute averaged partial correlation for each publication year using our sample of 57 studies. Detailed figures are provided in Table A.11.

Table 4: MRA [Publication timing and PbCs]

Variables	Publication pre 2000[a]	Publication 2001-2005	Publication 2005-2010	Publication 2011-2014
Standard error (FAT)	-0.259	**0.940***	**0.628****	**0.676***
	(0.786)	**(0.499)**	**(0.288)**	**(0.352)**
Constant (PET)	**0.126*****	0.007	**0.043*****	**0.026*****
	(0.040)	(0.021)	**(0.012)**	**(0.010)**
RMSE	0.045	0.046	0.061	0.051
Adjusted R^2	-0.039	0.057	0.060	0.033
Number of cluster	17	30	26	75
N	26	185	267	853

Notes: See Table 1. Dependent variable: adjusted partial correlation. All columns are estimated with WLS (precision squared weights) and double clustered standard errors.[a]: Despite the small number of observations, results are qualitatively unchanged when using White correction instead of clustering.$*p < 0.10, * * p < 0.05, * * *p < 0.01$.

Table 5: MRA [Heterogeneity of PbCs in comparison to benchmark results]

Variables	Income level		Age of democracy		Strength of democracy		Forms of government		Electoral rules	
Standard error (FAT)	0.794***	0.827***	0.747***	0.912***	0.771***	0.780***	0.832***	0.871***	0.794***	0.813***
	(0.224)	(0.219)	(0.225)	(0.218)	(0.215)	(0.216)	(0.213)	(0.237)	(0.218)	(0.222)
Constant (PET)	0.024***	0.023***	0.024***	0.022***	0.024***	0.024***	0.022***	0.021***	0.023***	0.023***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Low-income countries	-0.004									
	(0.011)									
High-income countries		-0.011								
		(0.010)								
Young democracies			0.009							
			(0.010)							
Established democracies				**-0.033*** **						
				(0.011)						
Low-level democracies					-0.011					
					(0.011)					
High-level democracies						**-0.021** **				
						(0.010)				
Presidential							-0.016			
							(0.024)			
Parliamentary								-0.027		
								(0.032)		
Majoritary									-0.021	
									(0.016)	
Proportional										-0.006
										(0.019)
RMSE	0.052	0.052	0.052	0.051	0.052	0.052	0.052	0.052	0.052	0.052
Adjusted R^2	0.054	0.059	0.055	0.091	0.056	0.057	0.058	0.058	0.056	0.055
Number of cluster	148		148		148		148		148	
N	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.

Table 6: Multiple FAT-PET models [Data and model specification]

Variables	All Observations (Adjusted Partial Correlation)			
	General	Specific	FEML	FEML double cluster
		Publication bias		
Standard error	**1.078*** **	**0.913*** **	0.567	**0.567***
	(0.288)	**(0.216)**	(0.363)	**(0.336)**
		Model structure		
Interactive model	-0.000		0.012	**0.012***
	(0.007)		(0.009)	**(0.007)**
Subsample	-0.003		-0.001	-0.001
	(0.003)		(0.004)	(0.004)
ConstitSamp	**-0.028***	**-0.017***	-0.000	-0.000
	(0.014)	**(0.010)**	(0.004)	(0.004)
HighincSamp	-0.008		-0.007	-0.007
	(0.020)		(0.017)	(0.014)
EstdemocSamp	**-0.032*** **	**-0.033*** **	**-0.046*** **	**-0.046*** **
	(0.008)	**(0.006)**	**(0.009)**	**(0.012)**
HighdemocSamp	-0.007		-0.011	-0.011
	(0.009)		(0.010)	(0.008)
BadSamp	0.003		-0.002	-0.002
	(0.012)		(0.011)	(0.011)
		Time and region		
1980s	0.015		0.013	0.013
	(0.019)		(0.026)	(0.028)
1990s	**-0.089*** **	**-0.093*** **	-0.050	**-0.050***
	(0.032)	**(0.031)**	(0.033)	**(0.028)**
Recent	0.005		**0.013***	0.013
	(0.008)		**(0.007)**	(0.011)
Eeca	**0.019** **	**0.009** **	-0.010	-0.010
	(0.008)	**(0.005)**	(0.010)	(0.008)
Lac	0.007		0.004	0.004
	(0.021)		(0.016)	(0.014)
Mena	0.004		-0.009	-0.009
	(0.011)		(0.011)	(0.011)
Sap	0.007		0.008	0.008
	(0.009)		(0.009)	(0.007)
Ssa	-0.015		0.015	0.015
	(0.012)		(0.013)	(0.012)
WeJ	0.008		-0.005	-0.005
	(0.009)		(0.007)	(0.007)
Global	-0.021		-0.014	-0.014
	(0.015)		(0.010)	(0.010)
Constant	**0.090***	**0.112*** **	-	-
	(0.046)	**(0.032)**	-	-
RMSE	0.050	0.050	0.045	0.045
Adjusted R^2	0.138	0.128	0.616	0.616
Number of cluster	57	57	57	148
Number of covariates	19	6	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 include authors fixed effects (not reported). Standard errors clustered by studies in parenthesis. Double clustering on studies and fiscal outputs in column 4. Adjusted R^2 is not strictly comparable across the different models. $*p < 0.10, * * p < 0.05, * * *p < 0.01$.

Table 7: Multiple FAT-PET models [Paper, methodology and covariates]

Variables	All Observations (Adjusted Partial Correlation)			
	General	Specific	FEML	FEML double cluster
			Publication bias	
Standard error	**0.948*** **	**0.823*** **	0.430	0.430
	(0.209)	**(0.198)**	(0.334)	(0.324)
			Paper	
Public Choice	0.012		-0.020	-0.020
	(0.011)		(0.015)	(0.017)
Unpublished	0.012		-0.138	-0.138
	(0.011)		(0.092)	(0.088)
Impact Factor	-0.000		0.002	0.002
	(0.000)		(0.002)	(0.002)
Before 2008	-0.008		**-0.084*** **	**-0.084*** **
	(0.012)		**(0.010)**	**(0.019)**
			Methodology	
Samplesize	0.000		-0.000	-0.000
	(0.000)		(0.000)	(0.000)
Infrannual	-0.006		-0.006	-0.006
	(0.010)		(0.006)	(0.024)
YSurplus	**0.029*** **	**0.023*** **	**0.025*** **	**0.025*** **
	(0.004)	**(0.004)**	**(0.004)**	**(0.006)**
YVar	-0.014		0.001	0.001
	(0.014)		(0.011)	(0.006)
YCycl	**-0.031** **	**-0.030*** **	-0.002	-0.002
	(0.015)	**(0.006)**	(0.015)	(0.018)
YCentral	0.001		**0.057*** **	**0.057*** **
	(0.011)		**(0.015)**	**(0.016)**
ElectDum	**-0.012** **	**-0.012** **	-0.004	-0.004
	(0.006)	**(0.005)**	(0.004)	(0.003)
ElectRat	**-0.025** **	**-0.026** **	-0.008	-0.008
	(0.011)	**(0.010)**	(0.005)	(0.006)
Adjust. Calendar	0.002		-0.009	-0.009
	(0.008)		(0.006)	(0.007)
Adjust. Highest	0.008		0.174	0.174
	(0.009)		(0.114)	(0.111)
Adjust. Exog	-0.011		**-0.012***	**-0.012***
	(0.007)		**(0.007)**	**(0.006)**
EconDynamic	**0.015** **	**0.014** **	0.006	0.006
	(0.006)	**(0.006)**	(0.003)	(0.003)
SE Correction	-0.010		**-0.011*** **	**-0.011*** **
	(0.008)		**(0.003)**	**(0.004)**
			Covariates	
GDPpc.	0.007		-0.015	-0.015
	(0.013)		(0.017)	(0.013)
Trade	-0.010		0.010	0.010
	(0.020)		(0.027)	(0.020)
PopStruct	0.008		0.008	0.008
	(0.019)		(0.015)	(0.012)
OG	-0.005		**-0.023***	**-0.023** **
	(0.012)		**(0.013)**	**(0.011)**
Partisan	**0.042** **	**0.032** **	-0.016	-0.016
	(0.018)	**(0.014)**	(0.013)	(0.014)
Time	0.011		**0.007***	0.007
	(0.007)		**(0.004)**	(0.005)
Constant	0.002	**0.025** **	-	-
	(0.021)	**(0.010)**	-	-
RMSE	0.049	0.050	0.045	0.045
Adjusted R^2	0.167	0.136	0.621	0.621
Number of cluster	57	57	57	148
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 6.

Appendix A. List of studies and descriptive statistics

Table A.8: 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	Nieto-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 (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 do not 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.9: Summary of studies

	Paper	Sample	Time period	Estimator	Fiscal output[a]	Election	Mean (adjust) partial	Median (adjust) partial	No. of estimates
1	Afonso (2008)	15 European Union countries	1970-2003[b]	OLS pooling & Fixed effects	Fiscal sold	Binary variable	-0.11	-0.09	15
2	Alesina and Ronbini (1992)	18 OECD countries	1960-1987	Fixed effects	Fiscal sold	Binary variable	0.13	0.13	3
3	Alesina et al. (1993)	14 OECD countries	1960-1987	Fixed effects	Fiscal sold	Binary variable	0.11	0.12	8
4	Alesina et al. (2006)	Developed and developing countries	1960-2003	Fixed effects	Fiscal sold	Binary variable	0.03	0.03	3
5	Alt and Lassen (2006)	19 OECD countries[b]	1989-1998	AB GMM	Fiscal sold	Binary variable	0.15	0.15	7
6	Ashworth and Heyndels (2002)	18 OECD countries	1965-1995	Fixed effects & Random effects	Direct taxes & Nonfiscal revenue	Binary variable	0.03	0.04	12
7	Barberia and Avelino (2011)	18 Latin America countries	1973-2008	OLS pooling & Fixed effects & AB GMM & System GMM	Fiscal sold & Total spending & Total revenue	Binary variable	0.04	0.05	120
8	Bayar and Smeets (2009)	15 European Union countries	1971-2006	Fixed effects	Fiscal sold	Binary variable	0.08	0.13	3
9	Block (2002)	44 SSA countries	1980-1995	OLS pooling & Fixed effects & AB GMM & System GMM	Fiscal sold & Total spending & Current spending & Nonfiscal revenue	Binary variable	0.07	0.09	12
10	Block et al. (2003)	44 SSA countries[b]	1980-1995	OLS pooling & Fixed effects & AB GMM	Current spending	Binary variable	0.01	-0.01	3
11	Bove et al. (2014)	22 OECD countries[b]	1981-2009[b]	Random effects & Panel corrected SE & LSDV	Broad public goods & Local public goods	Binary variable	0.12	0.12	19
12	Brender (2003)	80 Developed and developing countries[b]	1960-2001[b]	Fixed effects	Fiscal sold & Total spending & Total revenue	Binary variable	0.04	0.05	87
13	Brender and Drazen (2005)	68 Developed and developing countries[b]	1960-2001	Fixed effects	Fiscal sold & Total spending & Total revenue	Binary variable	0.06	0.04	45
14	Buti and Van Den Noord (2004)	11 European & Monetary Union countries	1999-2002	OLS pooling	Fiscal sold & Total spending & Total revenue	Binary variable	-0.09	0.04	5
15	Combes et al. (2015)[c]	70 Developing countries[b]	1990-2010	AB GMM & System GMM	Current spending & Capital spending	Binary variable	0.07	0.06	10
16	Costa-Fernandes and Mota (2013)	12 European Union countries	1976-2008	AB GMM & System GMM	Fiscal sold & Total spending & Total taxes & Broad public goods	Franzese's full ratio	0.00	0.01	16
17	Dreher and Vaubel (2004)	77 Developed and developing countries[b]	1975-1997	OLS pooling & 2SLS estimator & System GMM	Fiscal sold	Other	0.07	0.06	3
18	Eheke and Ölçer (2013)	61 Developing countries[b]	1990-2010	OLS pooling & 2SLS estimator & System GMM	Fiscal sold & Current spending & Capital spending & Direct taxes & Indirect taxes & External taxes	Binary variable	0.01	0.01	16
19	Efthyvoulou (2012)	27 European Union countries[b]	1997-2008[b]	Fixed effects & System GMM	Fiscal sold & Total spending & Total revenue & Current spending & Capital spending & Current revenue & Total taxes	Binary variable	0.21	0.21	35
20	Ehrhart (2013)	56 Developing countries[b]	1980-2006	Fixed effects & System GMM	Direct taxes & Indirect taxes	Binary variable	0.03	0.04	28
21	Franzese (2000)	21 OECD countries	1956-1990	Panel corrected SE	Fiscal sold	Other	0.12	0.12	1
22	Galcotti and Salford (2001)	18 OECD countries	1961-1995[b]	GLS estimator	Fiscal sold & Total spending & Total taxes	Binary variable	0.09	0.08	4
23	Golinelli and Momigliano (2006)	11 European Union countries	1988-2006	GUM estimator	Fiscal sold	Binary variable	0.23	0.23	1
24	Hagen (2007)	24 OECD countries	1989-2005	System GMM	Fiscal sold	Binary variable	0.13	0.13	5
25	Hallerberg et al. (2002)	10 EU accession countries	1990-1999	OLS pooling	Fiscal sold	Franzese's full ratio	0.20	0.20	2
26	Hausuch (2012)	28 Developed countries[b]	1980-2008	OLS pooling & Fixed effects & System GMM	Fiscal sold	Binary variable	0.08	0.09	18
27	Hausuch and Vaaler (2013)	18 Emerging countries[b]	1989-2004	Fixed effects & System GMM	Fiscal sold	Binary variable	0.17	0.17	8
28	Hausuch and Keefer (2014)	67 Developed and developing countries[b]	1975-2008	Fixed effects & AB GMM & System GMM	Total spending & Current spending	Binary variable	0.06	0.06	26
29	Jong-A-Pin et al. (2012)	25 OECD countries	1996-2011	Fixed effects & SUR estimates	Fiscal sold & Current spending & Capital spending & Current revenue	Binary variable	0.00	-0.02	27
30	Kaplan and Thomsson (2014)	16 Latin America countries	1961-2011	Fixed effects & AB GMM	Fiscal sold	Binary variable	0.22	0.23	7
31	Katsimi and Sarantides (2012)	19 OECD countries	1972-2008[b]	Fixed effects & AB GMM	Fiscal sold & Total spending & Total revenue & Current spending & Capital spending & Direct taxes & Indirect taxes	Binary variable & Other	0.03	0.02	73
32	Klasnja (2008)	25 EECA countries[b]	1990-2006	Fixed effects	Fiscal sold & Total spending & Total revenue & Broad public goods & Local public goods	Binary variable	0.13	0.15	5
33	Klomp and de Haan (2013a)	70 Developed and developing countries[b]	1970-2007[d]	PMG & MG & DFE estimator	Fiscal sold & Total spending	Franzese's full ratio	0.04	0.05	38
34	Klomp and de Haan (2013b)	65 Developed and developing countries	1975-2005	Semi-pooled model	Fiscal sold & Total spending	Franzese's full ratio	0.04	0.04	2
35	Klomp and de Haan (2013c)	65 Developed and developing countries[b]	1975-2005	(IV) Fixed effects	Fiscal sold & Total spending	Franzese's full ratio	0.08	0.06	14
36	Klomp and de Haan (2013d)	67 Developed and developing countries[b]	1975-2005	System GMM	Broad public goods & Local public goods	Binary variable & Franzese's full ratio	0.09	0.08	13
37	Kouvavas (2013)	63 Developed and developing countries[b]	1960-2001	Fixed effects & ABO GMM	Fiscal sold	Binary variable	0.07	0.06	15
38	Kraemer (1997)	20 Latin America countries	1983-1996	Fixed effects & ABO GMM	Fiscal sold & Total spending & Total revenue & Current spending & Capital spending	Other	0.09	0.09	5
39	Maurel (2006)	26 European countries	1990-2005[b]	Fixed effects & ABO GMM	Fiscal sold & Total spending & Total revenue	Binary variable	0.03	0.04	12
40	Mosley and Chiripanhura (2012)	21 SSA countries[b]	1980-2008	OLS pooling & Fixed effects & ABO GMM	Fiscal sold	Binary variable	0.10	0.08	5
41	Morozumi et al. (2014)	107 Developed and developing countries[b]	1975-2010	OLS pooling & Fixed effects & ABO GMM	Fiscal sold & Total spending & Total revenue & Current spending & Capital spending & Direct taxes & Indirect taxes & External taxes	Binary variable & Franzese's full ratio	0.05	0.07	232
42	Mourão (2011)	60 Developed and developing countries[b]	1960-2006	AB GMM	Fiscal sold & Total spending & Total revenue	Binary variable	0.02	0.02	30
43	Nieto-Parra and Santiso (2009)	46 OECD & Latin America countries[b]	1990-2006	Fixed effects & AB GMM	Fiscal sold & Total spending & Current spending & Capital spending	Binary variable	0.09	0.08	48
44	Nyblade and O'Mahony (2014)	97 Developing countries	1975-2005	Fixed effects	Fiscal sold	Binary variable	0.06	0.06	1
45	Persson and Tabellini (2003a)	60 Developed and developing countries[b]	1960-1998	Fixed effects	Fiscal sold & Total spending & Total revenue & Broad public goods	Binary variable	0.03	0.03	12
46	Potrafke (2007)	20 OECD countries	1980-2003	OLS pooling & Panel corrected SE & LSDV	Broad public goods	Other	0.03	0.03	6
47	Potrafke (2010)	18 OECD countries	1971-2004	Random effects & LSDV	Broad public goods	Other	0.09	0.09	24
48	Schuknecht (1996)	35 Developing countries[b]	1970-1992	OLS pooling	Fiscal sold	Other	0.15	0.15	3
49	Schuknecht (2000)	24 Developing countries	1973-1992	Fixed effects	Fiscal sold & Total spending & Total revenue & Current spending & Capital spending & Local public goods	Other	0.10	0.09	6
50	Shelton (2014)	108 Developed and developing countries[b]	1980-2007	Fixed effects & AB GMM	Fiscal sold	Binary variable	0.10	0.10	16
51	Shi and Svensson (2006)	85 Developed and developing countries[b]	1975-1995	Fixed effects & System GMM	Fiscal sold	Binary variable	0.07	0.08	24
52	Stanova (2009)	10 NMS countries[b]	1998-2008[b]	OLS pooling	Fiscal sold & Total spending & Total revenue	Binary variable	0.15	0.14	102
53	Streb et al. (2009)	67 Developed and developing countries[b]	1960-2001	OLS pooling & Fixed effects	Fiscal sold & Total spending & Total revenue	Binary variable & Other	0.09	0.10	6
54	Streb et al. (2012)	30 OECD & Latin America countries[b]	1980-2005	Fixed effects	Fiscal sold & Total spending & Total revenue	Binary variable & Other	0.04	0.04	72
55	Troeger and Schneider (2012)	17 OECD countries	1975-2009	FEVD estimator	Total spending & Direct taxes & Indirect taxes	Binary variable	0.03	0.03	4
56	Tujula and Wolswijk (2007)	22 OECD countries[b]	1970-2002	Fixed effects	Fiscal sold	Binary variable	0.11	0.11	6
57	Wright (2011)	116 Dictatorships[b]	1961-2006	Fixed effects	Total expenditure	Binary variable	0.04	0.03	8
Total							0.07	0.06	1331

Notes: [a]: Fiscal sold refers to fiscal surplus or fiscal deficit. [b]: Subsamples among countries and/or time period, within considered studies.[c]: As the publication date is after december 31^{th} 2014, we take into account the working paper version (Combes et al., 2013).

Table A.10: Descriptive statistics [Repartition of fiscal outputs]

	No. of estimates	No. of papers	No. of papers focus
Spending	535	33	9
Restrictive	275	24	2
Sub-components	260	21	7
Revenue	354	24	2
Restrictive	196	15	0
Sub-components	158	12	2
Surplus	442	47	22

Notes: Column 1 indicates the number of regressions using a given category of fiscal output. Column 2 indicates the number of 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.11: Descriptive statistics [Timeline]

Year of publication	No. of published articles	No. of working papers	No. of estimates	Mean (adjusted) partial	Median (adjusted) partial	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	0.176
2000	2	0	7	0.068	0.081	0.073	0.145
2001	0	1	4	0.085	0.089	-0.007	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	0.054	0.053	-0.158	0.319
2005	1	0	45	0.061	0.043	-0.041	0.168
2006	4	1	47	0.072	0.073	-0.047	0.227
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	0.062	0.050	-0.198	0.416
2013	7	3	160	0.051	0.057	-0.071	0.222
2014	3	3	301	0.065	0.072	-0.151	0.291
Total/Average	38	19	1,331	0.074	0.072	-0.074	0.244
Total	38	19	1,331	0.065	0.063	-0.291	0.652

Notes: We compute descriptive statistics on adjusted partial correlation for comparability purposes.

Appendix B. Variable definitions

Table B.12: Variable definitions

No.	Variables	Variable Description (BD for binary dummy)	N	Mean	Median	S.D.	Min	Max
1	Adjustedpartial	Partial correlation (adjusted for revenue and fiscal surplus).	1,331	0.07	0.06	0.09	-0.29	0.65
2	Partial	Partial correlation (non adjusted for revenue and fiscal surplus).	1,331	-0.02	-0.04	0.11	-0.65	0.63
3	SE	Standard error of the correlation.	1,331	0.05	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	0.48	0	1
5	YSpen	BD if used total expenditure over GDP.	1,726	0.19	0	0.39	0	1
6	YRev	BD if used total revenue over GDP.	1,726	0.14	0	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	0.49	0	1
8	YRev bis	BD if used total (or subcomponents) revenue over GDP.	1,726	0.25	0	0.44	0	1
9	YVar	BD if dependent variable is in first difference or growth rate.	1,726	0.13	0	0.34	0	1
10	YCycl[a]	BD if dependent variable is cyclically adjusted.	1,726	0.06	0	0.23	0	1
11	YCentral	BD if dependent variable explicitely refers to central government.	1,726	0.58	1	0.49	0	1
		Group 2: Measure of elections[b]						
12	ElectDum	BD if elections are captured by electoral dummies.	1,726	0.79	1	0.40	0	1
13	ElectRat	BD if elections are captured by ratio *a la Franzese*.	1,726	0.19	0	0.39	0	1
14	ElectOth	BD if elections are captured by other methods (used as the base).	1,726	0.04	0	0.19	0	1
		Group 3: Adjustment on elections						
15	ACalendar	BD if adjustment for electoral or fiscal calendar.	1,726	0.18	0	0.39	0	1
16	AHighest	BD if adjustment on election for the executive.	1,726	0.59	1	0.49	0	1
17	AExog	BD if adjustment on predetermined election.	1,726	0.33	0	0.47	0	1
		Group 4: Other methodologies						
18	Samplesize	Number of observations included in the sample.	1,726	719.20	466	779.59	15	6,631
19	Infra	BD if infra annual data used.	1,726	0.05	0	0.22	0	1
20	EconDynamic	BD if used dynamic panel estimator.	1,726	0.33	0	0.47	0	1
21	EconOther	BD if used other estimator (used as the base).	1,726	0.67	1	0.47	0	1
22	SE correction	BD if used SE correction for heteroskedasticity or autocorrelation.	1,726	0.50	0	0.50	0	1
		Group 5: Model structure						
23	Interactive	BD if author(s) use interactive models.	1,726	0.38	0	0.48	0	1
24	Subsample	BD if author(s) use subsample technique.	1,726	0.48	0	0.50	0	1
25	ConstitSamp	BD if subsample on specific constitutional forms.	1,726	0.26	0	0.44	0	1
26	HighincSamp	BD if subsample on high-income countries.	1,726	0.24	0	0.43	0	1
27	EstdemocSamp	BD if subsample on established democracies.	1,726	0.16	0	0.37	0	1
28	HighdemocSamp	BD if subsample on high level democracies.	1,726	0.02	0	0.16	0	1
29	BadSamp	BD if subsample on other bad-case senarii for PbCs.	1,726	0.05	0	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	1	0.48	0	1
31	1980s	BD if data for the 80's.	1,726	0.79	1	0.41	0	1
32	1990s	BD if data for the 90's.	1,726	0.97	1	0.18	0	1
33	Recent	BD if data for the 00's and 10's.	1,726	0.86	1	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	1	0.44	0	1
35	Eeca	BD if countries from Eastern Europe & Central Asia were included in samples.	1,726	0.55	1	0.50	0	1
36	Lac	BD if countries from Latin America & Caribbean were included in samples.	1,726	0.62	1	0.49	0	1
37	Mena	BD if countries from Middle-east & North Africa were included in samples.	1,726	0.49	0	0.50	0	1
38	Sap	BD if countries from South Asia & Pacific were included in samples (except Japan).	1,726	0.51	1	0.50	0	1
39	Ssa	BD if countries from Sub-saharan Africa were included in samples.	1,726	0.49	0	0.50	0	1
40	Global	BD if at least two regions were included in samples.	1,726	0.62	1	0.48	0	1
		Group 8: Publications outlet						
41	Public Choice	BD for Public Choice review.	1,726	0.11	0	0.32	0	1
42	Unpublished	BD for unpublished paper.	1,726	0.49	0	0.50	0	1
43	Impact Factor	2015 Google Scholar five years index of journal.	1,726	25.92	15	27.21	0	168
44	Before2008	BD if paper is released $\leq$ 2008.	1,726	0.28	0	0.45	0	1
45	After2008	BD if paper is released $>$ 2008 (used as the base).	1,726	0.72	1	0.45	0	1
		Group 9: Covariates						
46	GDPpc.	BD for per capita GDP as control.	1,726	0.76	1	0.43	0	1
47	Trade	BD for trade as control.	1,726	0.57	1	0.50	0	1
48	PopStruct	BD for population structure as control.	1,726	0.61	1	0.49	0	1
49	OG	BD for output gap as control.	1,726	0.51	1	0.50	0	1
50	Partisan	BD for partisan measure (such as political ideology) as control.	1,726	0.13	0	0.33	0	1
51	Time	BD for time dummies or time trend as control.	1,726	0.56	1	0.50	0	1

Notes: We keep 2 digits after comma, for convenience. [a]: We include the discretionary measures of Buti and Van Den Noord (2004) in this category.[b]: Authors can use several measures of elections in the same regression.

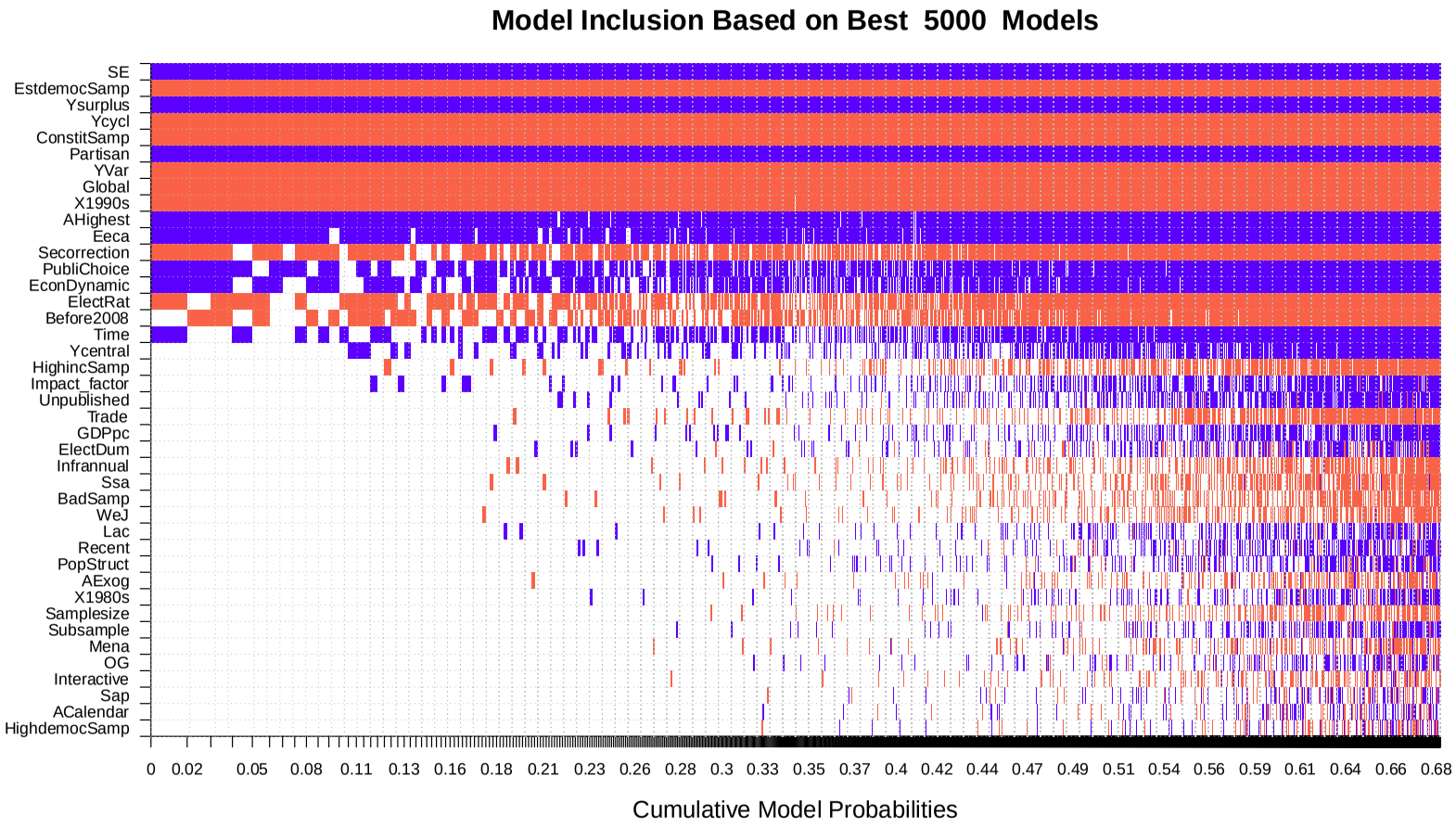
Appendix C. Bayesian meta-analysis [Supplementary material]

The bayesian analysis presents two advantages. 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 sections 10.1 and 10.2 allow reassessing the effect of each covariate without imposing any predetermined structure to our model of estimation.

We follow Havránek (2015) -in which the interesting reader can find more detailed technical developments- and consider the uniform prior on model probabilities together with a UIP g-prior.[37] All results are display in Figure C.7 and Table C.13. We retain all covariates with a posterior inclusion probability (PIP) over 50%, in the frequentist check OLS. We do not intend to discuss results in details but we note in particular that the bayesian analysis confirms the magnitude of publication bias relative to genuine effect. In addition we have further evidence regarding heterogeneities accross time and space, but also the non-neutrality of the empirical methodology employed to estimate PbCs and that maturity of democracy significantly reduces their extent and constraints electorally-driven leaders' manipulation of budget. These results are robust to the use of alternative priors (see Figure C.8 and Table C.14) and to the use of weighted estimates (see Figure C.9 and Table C.15).

[37] 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. 43,818,545 models are visited and the best 5,000 models have a cumulative probability of 69%. Additional details are available upon request.

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



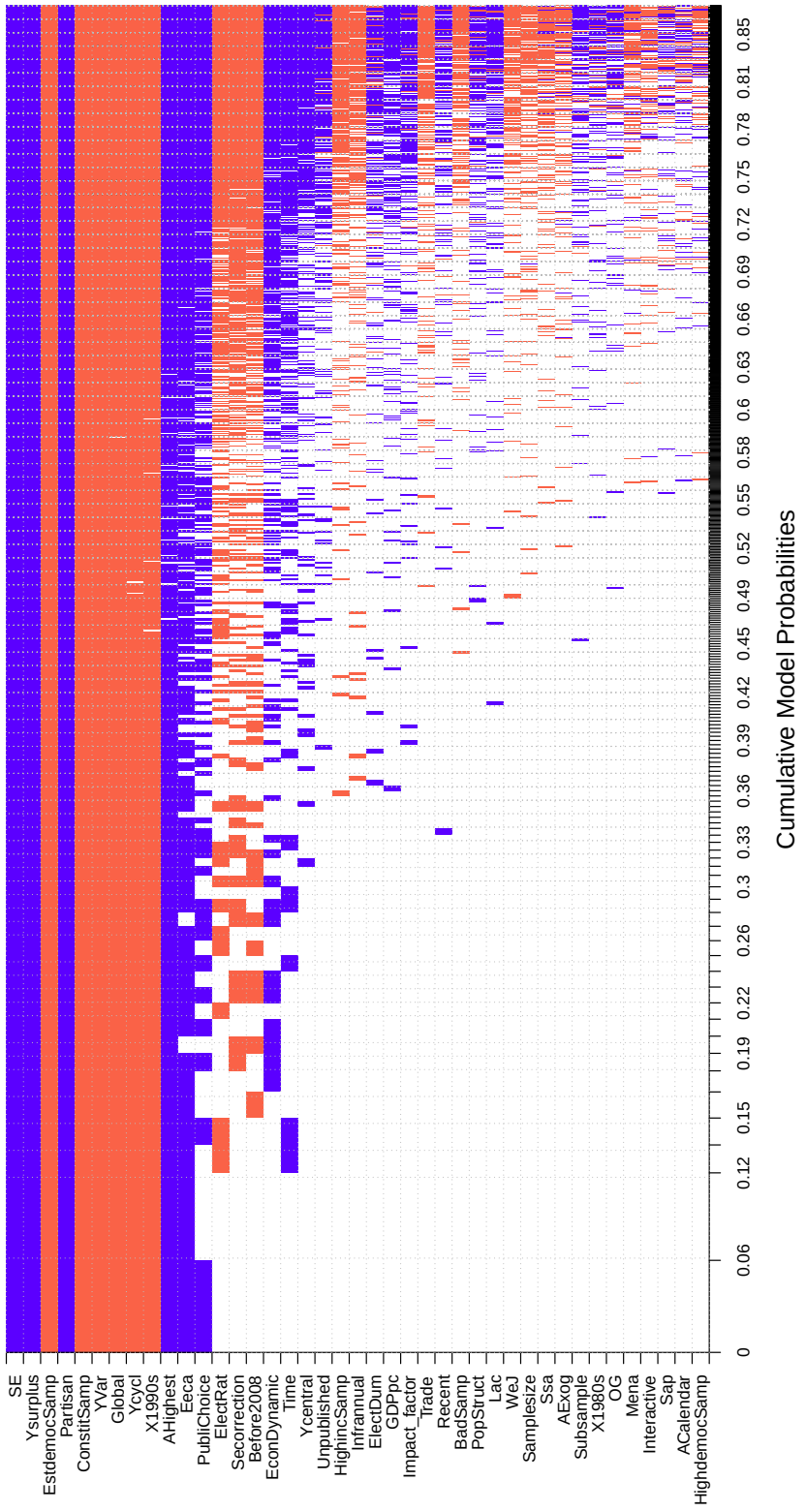
Notes: Dependent variable: adjusted partial correlation. Birth-death sampler used. Columns denote individual models; the variables are sorted by posterior inclusion probability (PIP) in descending order. Blue color (darker in grayscale): the variable is included and the estimated sign is positive. Red 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 C.13. A detailed description of all the variables is available in Table B.12.

Table C.13: 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**	**0.905**	**0.113**	**1.000**	**0.804*** **	**(0.183)**	**0.804*** **	**(0.188)**
Model structure								
Interactive model	0.017	0.000	0.001	0.070				
Subsample	0.022	0.000	0.001	0.915				
ConstitSamp	**1.000**	**-0.033**	**0.008**	**0.000**	-0.014	(0.011)	-0.014	(0.012)
HighincSamp	0.100	-0.001	0.005	0.000				
EstdemocSamp	**1.000**	**-0.042**	**0.009**	**0.000**	**-0.030*** **	**(0.006)**	**-0.030*** **	**(0.011)**
HighdemocSamp	0.012	0.000	0.002	0.483				
BadSamp	0.036	-0.001	0.003	0.000				
Time and Regions								
1980s	0.025	0.000	0.002	0.930				
1990s	**0.992**	**-0.052**	**0.014**	**0.000**	**-0.073*** **	**(0.022)**	**-0.073*** **	**(0.019)**
Recent	0.034	0.000	0.002	0.884				
Eeca	**0.891**	**0.021**	**0.010**	**1.000**	**0.016** **	**(0.006)**	**0.016***	**(0.008)**
Lac	0.035	0.000	0.002	0.968				
Mena	0.021	0.000	0.002	0.095				
Sap	0.013	0.000	0.001	0.612				
Ssa	0.048	-0.001	0.004	0.014				
WeJ	0.036	0.000	0.002	0.003				
Global	**0.999**	**-0.042**	**0.009**	**0.000**	**-0.017** **	**(0.007)**	**-0.017***	**(0.009)**
Paper								
Public Choice	**0.677**	**0.015**	**0.012**	**1.000**	**0.013***	**(0.007)**	0.013	(0.015)
Unpublished	0.075	0.001	0.004	0.964				
Impact factor	0.100	0.000	0.000	0.993				
Before 2008	0.487	-0.010	0.011	0.000				
Methodology								
Samplesize	0.024	0.000	0.000	0.019				
Infrannual	0.051	-0.001	0.004	0.003				
YSurplus	**1.000**	**0.034**	**0.005**	**1.000**	**0.025*** **	**(0.004)**	**0.025*** **	**(0.006)**
YVar	**0.999**	**-0.039**	**0.008**	**0.000**	-0.009	(0.013)	-0.009	(0.014)
YCycl	**1.000**	**-0.053**	**0.012**	**0.000**	**-0.018***	**(0.010)**	-0.018	(0.014)
YCentral	0.193	0.003	0.006	1.000				
ElectDum	0.054	0.000	0.003	0.800				
ElectRat	**0.601**	**-0.012**	**0.011**	**0.000**	-0.010	(0.008)	-0.010	(0.009)
ACalendar	0.012	0.000	0.001	0.634				
AHighest	**0.957**	**0.022**	**0.008**	**1.000**	**0.017** **	**(0.007)**	**0.017** **	**(0.008)**
AExog	0.026	0.000	0.002	0.094				
EconDynamic	**0.603**	**0.009**	**0.009**	**1.000**	**0.009***	**(0.005)**	**0.009***	**(0.005)**
Se Correction	**0.691**	**-0.012**	**0.009**	**0.000**	-0.006	(0.007)	-0.006	(0.006)
Covariates								
GDPpc.	0.060	0.001	0.004	0.999				
Trade	0.069	-0.001	0.004	0.001				
PopStruct	0.031	0.000	0.003	0.896				
OG	0.019	0.000	0.001	0.856				
Partisan	**1.000**	**0.042**	**0.011**	**1.000**	**0.029** **	**(0.012)**	**0.029** **	**(0.011)**
Time	0.453	0.007	0.009	1.000				
Constant	**1.000**	**0.076**	NA	NA	**0.084*** **	**(0.025)**	**0.084*** **	**(0.024)**
RMSE			-		0.048		0.048	
Adjusted R^2			-		0.205		0.205	
Number of studies			57		57		57	
Number of cluster			-		57		148	
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 C.8: BMA: Model inclusion [BRIC g-prior; beta-binomial model prior]



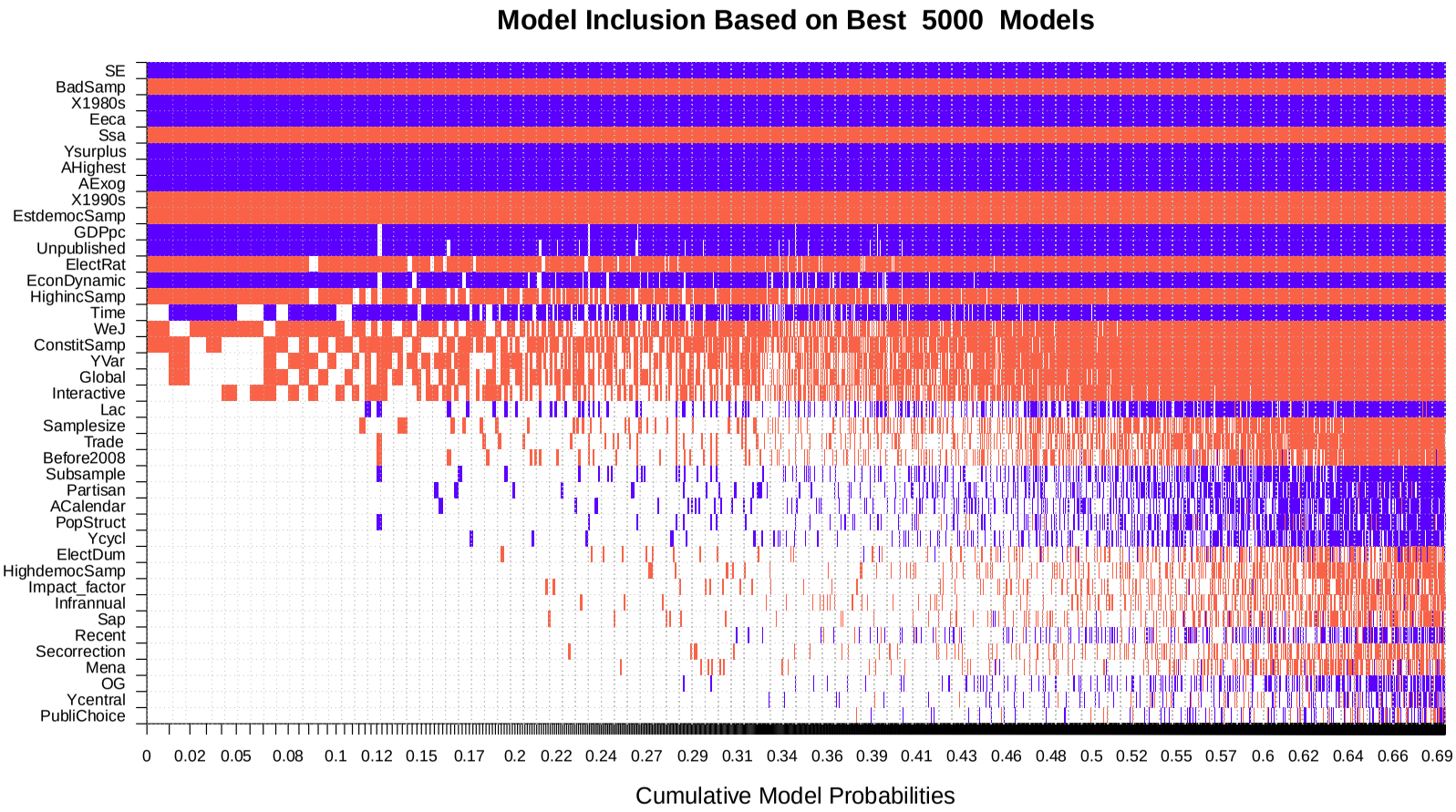
Notes: See Figure C.7.

Table C.14: 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**	**0.909**	**0.116**	**1.000**	**0.851*** **	**(0.188)**	**0.851*** **	**(0.190)**
Model structure								
Interactive model	0.009	0.000	0.001	0.229				
Subsample	0.014	0.000	0.001	0.931				
ConstitSamp	**0.999**	**-0.033**	**0.007**	**0.000**	-0.015	(0.010)	-0.015	(0.012)
HighincSamp	0.051	-0.001	0.003	0.000				
EstdemocSamp	**1.000**	**-0.043**	**0.008**	**0.000**	**-0.031*** **	**(0.006)**	**-0.031*** **	**(0.012)**
HighdemocSamp	0.008	0.000	0.001	0.419				
BadSamp	0.023	0.000	0.003	0.000				
Time and Regions								
1980s	0.012	0.000	0.001	0.891				
1990s	**0.985**	**-0.054**	**0.015**	**0.000**	**-0.077*** **	**(0.023)**	**-0.077*** **	**(0.020)**
Recent	0.024	0.000	0.002	0.928				
Eeca	**0.847**	**0.020**	**0.011**	**1.000**	**0.019*** **	**(0.005)**	**0.019** **	**(0.008)**
Lac	0.020	0.000	0.002	0.981				
Mena	0.010	0.000	0.001	0.188				
Sap	0.008	0.000	0.001	0.645				
Ssa	0.016	0.000	0.002	0.041				
WeJ	0.019	0.000	0.002	0.001				
Global	**0.995**	**-0.040**	**0.010**	**0.000**	**-0.016** **	**(0.007)**	**-0.016* **	**(0.009)**
Paper								
Public Choice	**0.511**	**0.011**	**0.012**	**1.000**	0.013	(0.008)	0.013	(0.016)
Unpublished	0.055	0.001	0.004	0.979				
Impact factor	0.041	0.000	0.000	0.975				
Before 2008	0.335	-0.006	0.010	0.000				
Methodology								
Samplesize	0.017	0.000	0.000	0.012				
Infrannual	0.046	-0.001	0.005	0.000				
YSurplus	**1.000**	**0.033**	**0.005**	**1.000**	**0.025*** **	**(0.004)**	**0.025*** **	**(0.006)**
YVar	**0.998**	**-0.039**	**0.008**	**0.000**	-0.011	(0.013)	-0.011	(0.014)
YCycl	**0.994**	**-0.050**	**0.012**	**0.000**	-0.010	(0.009)	-0.010	(0.013)
YCentral	0.113	0.002	0.005	1.000				
ElectDum	0.043	0.000	0.003	0.926				
ElectRat	0.344	-0.006	0.010	0.000				
ACalendar	0.008	0.000	0.000	0.677				
AHighest	**0.962**	**0.024**	**0.008**	**1.000**	**0.016** **	**(0.007)**	**0.016* **	**(0.008)**
AExog	0.015	0.000	0.001	0.115				
EconDynamic	0.330	0.005	0.008	1.000				
Se Correction	0.335	-0.005	0.008	0.000				
Covariates								
GDPpc.	0.042	0.001	0.003	1.000				
Trade	0.029	0.000	0.003	0.000				
PopStruct	0.021	0.000	0.003	0.954				
OG	0.012	0.000	0.001	0.911				
Partisan	**0.999**	**0.040**	**0.010**	**1.000**	**0.024*** **	**(0.013)**	**0.024** **	**(0.012)**
Time	0.263	0.004	0.007	1.000				
Constant	**1.000**	**0.075**	NA	NA	**0.084*** **	**(0.024)**	**0.084*** **	**(0.025)**
RMSE			-		0.048		0.048	
Adjusted R^2			-		0.198		0.198	
Number of studies			57		57		57	
Number of cluster			-		57		148	
N			1,331		1,331		1,331	

Notes: See Table C.13.

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



Notes: Inverse number of estimates weights. See Figure C.7.

Table C.15: 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
Publication bias								
SE	**1.000**	**1.549**	**0.106**	**1.000**	**1.073*** **	**(0.245)**	**1.073*** **	**(0.223)**
Model structure								
Interactive model	0.420	-0.005	0.007	0.000				
Subsample	0.093	0.001	0.004	1.000				
ConstitSamp	**0.643**	**-0.015**	**0.013**	**0.000**	**-0.024***	**(0.013)**	**-0.024***	**(0.012)**
HighincSamp	**0.783**	**-0.024**	**0.015**	**0.000**	-0.010	(0.006)	-0.010	(0.007)
EstdemocSamp	**0.999**	**-0.032**	**0.008**	**0.000**	**-0.037*** **	**(0.009)**	**-0.037*** **	**(0.013)**
HighdemocSamp	0.039	-0.001	0.006	0.000				
BadSamp	**1.000**	**-0.051**	**0.010**	**0.000**	0.003	(0.012)	0.003	(0.011)
Time and Regions								
1980s	**1.000**	**0.085**	**0.011**	**1.000**	0.007	(0.017)	0.007	(0.016)
1990s	**1.000**	**-0.050**	**0.011**	**0.000**	**-0.090*** **	**(0.027)**	**-0.090*** **	**(0.023)**
Recent	0.031	0.000	0.002	0.815				
Eeca	**1.000**	**0.052**	**0.008**	**1.000**	**0.015***	**(0.008)**	0.015	(0.012)
Lac	0.168	0.004	0.010	0.978				
Mena	0.028	0.000	0.002	0.070				
Sap	0.033	0.000	0.003	0.060				
Ssa	**1.000**	**-0.070**	**0.010**	**0.000**	-0.001	(0.012)	-0.001	(0.014)
WeJ	**0.659**	**-0.014**	**0.012**	**0.000**	0.009	(0.009)	0.009	(0.011)
Global	**0.505**	**-0.015**	**0.017**	**0.000**	**-0.023** **	**(0.010)**	**-0.023** **	**(0.011)**
Paper								
Public Choice	0.012	0.000	0.001	0.698				
Unpublished	**0.923**	**0.020**	**0.008**	**1.000**	0.002	(0.007)	0.002	(0.010)
Impact factor	0.036	0.000	0.000	0.014				
Before 2008	0.107	-0.001	0.004	0.008				
Methodology								
Samplesize	0.143	0.000	0.000	0.000				
Infrannual	0.033	0.000	0.003	0.001				
YSurplus	**1.000**	**0.030**	**0.005**	**1.000**	**0.025*** **	**(0.004)**	**0.025*** **	**(0.006)**
YVar	**0.525**	**-0.011**	**0.012**	**0.000**	-0.000	(0.011)	-0.000	(0.013)
YCycl	0.060	0.001	0.005	1.000				
YCentral	0.016	0.000	0.001	0.733				
ElectDum	0.045	0.000	0.003	0.133				
ElectRat	**0.894**	**-0.020**	**0.010**	**0.000**	0.003	(0.011)	0.003	(0.009)
ACalendar	0.066	0.001	0.003	0.997				
AHighest	**1.000**	**0.032**	**0.006**	**1.000**	0.009	(0.008)	0.009	(0.007)
AExog	**1.000**	**0.042**	**0.007**	**1.000**	-0.001	(0.008)	-0.001	(0.009)
EconDynamic	**0.863**	**0.014**	**0.007**	**1.000**	0.008	(0.006)	0.008	(0.005)
Se Correction	0.031	0.000	0.001	0.000				
Covariates								
GDPpc.	**0.957**	**0.028**	**0.009**	**1.000**	0.012	(0.009)	0.012	(0.010)
Trade	0.112	-0.002	0.005	0.000				
PopStruct	0.060	0.001	0.006	0.935				
OG	0.027	0.000	0.002	0.967				
Partisan	0.076	0.001	0.004	0.989				
Time	**0.735**	**0.011**	**0.008**	**1.000**	0.005	(0.006)	0.005	(0.007)
Constant	**1.000**	**1.004**	NA	NA	**0.078** **	**(0.035)**	**0.078** **	**(0.031)**
RMSE			-		0.048		0.048	
Adjusted R^2			-		0.188		0.188	
Number of studies			57		57		57	
Number of cluster			-		57		148	
N			1,331		1,331		1,331	

Notes: Inverse number of estimates weights. See Table C.13.

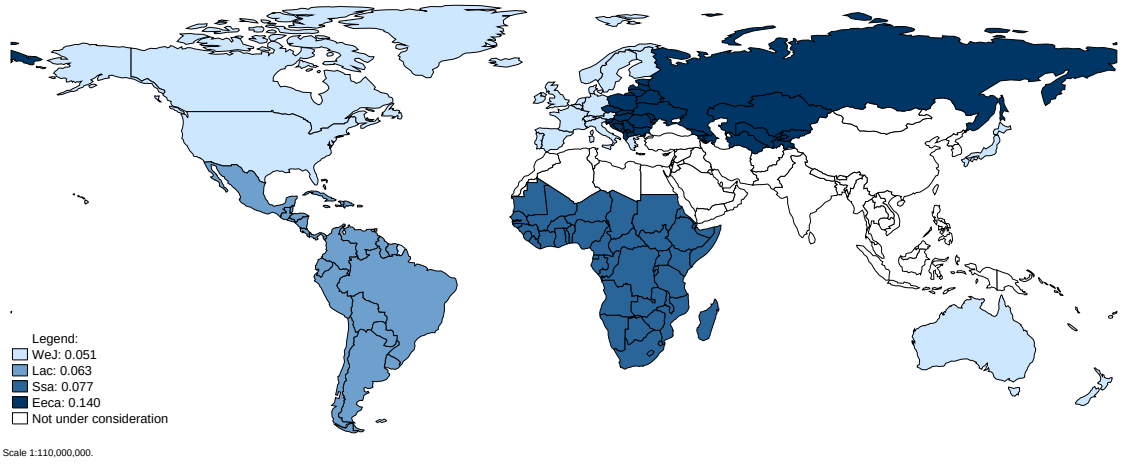
Appendix D. Mapping PbCs [Supplementary material]

Table D.16: Geopolitics of PbCs

Geography of PbCs	No. of estimates	Mean (adjust) partial	Median (adjust) partial	Lower bound	Upper bound
WeJ	221	0.046	0.051	-0.260	0.319
Eeca	112	0.146	0.140	-0.291	0.652
Lac	166	0.059	0.063	-0.094	0.291
Ssa	20	0.067	0.077	-0.213	0.386
Low-level democracies	45	0.040	0.044	-0.071	0.211
High-level democracies	26	0.060	0.034	-0.167	0.330
Including authoritarian states (all regressions including authoritarian obs.)	261	0.065	0.070	-0.213	0.386
Young democracies	160	0.091	0.080	-0.167	0.652
Established democracies	231	0.035	0.038	-0.260	0.411

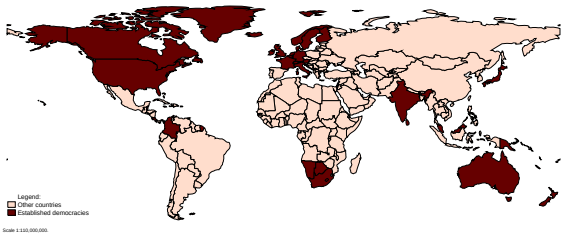
Notes: We focus on the following geographic regions: Western Europe & Japan (WeJ), Eastern Europe & Central Asia, Latin America & Caribbean (Lac), and Sub-saharan Africa (Ssa). Not any one of the 57 studies focus exclusively on other regions. Descriptive statistics regarding age and level of democracy also include countries from other regions.

Figure D.10: PbCs accross the world



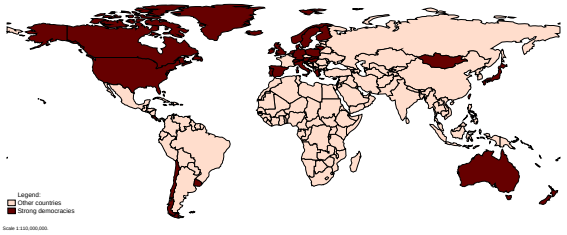
Notes: We compute averaged partial correlation for each geographic region defined in Table D.16. This repartition of PbCs is not relevant in failed states, due to the absence of state apparatus, or in countries without electoral races. For more details, see the Center for Systemic Peace website, the Fund for Peace website, V-Dem website, Cheibub et al. (2010), and Boix et al. (2013), among others.

Figure D.11: Established democracies at the January 1^{st}, 2015



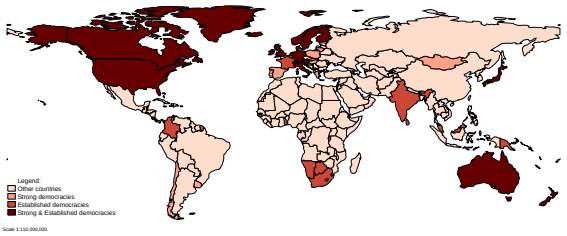
Notes: Age of democracy, regarding methodology of Brender and Drazen (2005). Despite overlapping in `spmap` code, Lesotho should be considered as "Other country".

Figure D.12: High-level democracies at the January 1^{st}, 2015



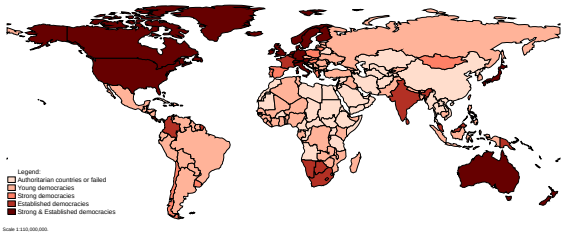
Notes: Level of democracy, regarding methodology of Brender and Drazen (2005).

Figure D.13: Quality of democracy at the January 1^{st}, 2015



Notes: Age and level of democracy regarding methodology of Brender and Drazen (2005). Despite overlapping in `spmap` code, Lesotho should be considered as "Other country".

Figure D.14: Political regime at the January 1^{st}, 2015



Notes: Age and level of democracy regarding methodology of Brender and Drazen (2005). To split between authoritarian countries, and democratic states, we consider positive polity2 index, but also Cheibub et al. (2010), and Boix et al. (2013), for uncovered countries. Despite overlapping in `spmap` code, Lesotho should be considered as "Young democracy".