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Economic Analysis and Policy

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https://doi.org/10.1016/j.eap.2022.12.011

Economic Analysis and Policy 77 (2023) 1083–1102

Contents lists available at ScienceDirect

Economic Analysis and Policy

journal homepage: www.elsevier.com/locate/eap

Analyses of topical policy issues

Political budget cycles in military expenditures: A meta-analysis

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ARTICLE INFO

Article history:
Received 15 September 2022
Received in revised form 19 December 2022
Accepted 20 December 2022
Available online 23 December 2022

Keywords:
Military expenditures
Election cycles
Meta-analysis

ABSTRACT

In this study, a meta-regression analysis is performed of studies that have examined whether military expenditures are affected by election cycles. Theoretically, the direction of these cycles is not immediately clear. This ambiguity stems mainly from the trade-off governments face when elections are upcoming. On the one hand, the incumbent government may try to improve the odds of being re-elected by boosting the performance of the national economy. One way is by increasing the procurement from the domestic defense industry using additional defense spending. On the other hand, it might be more favorable for the ruling cabinet to cut defense spending in an election year to finance expansions in other public spending categories that are preferred more by voters. The empirical studies considered in this meta-analysis are very different in nature concerning the election measure constructed, the sample of countries used, the time periods covered, model specification, estimation method, and publication outlet. Based on more than two hundred estimates, the main results indicate that there is only a weak negative genuine effect of elections on military expenditures. This finding suggests that governments follow a more contractionary policy in defense spending when elections are approaching. However, it also appears that this election effect substantially differs across country samples and relies to a great extent on certain country-specific characteristics. In particular, it turns out that the relationship between elections and military spending is likely to be positive in countries where the defense industry has some considerable political influence and economic power. Also, in countries that have to deal with serious security risks, this positive relationship is likely to prevail. Finally, I do not find any evidence that a publication bias inflates the election effect found among the considered studies. This is partly explained by the fact that both positive, as well as, negative results are theoretically plausible and, therefore, apparently equally likely to be published.

1. Introduction

National defense is one of the most important objectives of any government because national security is a necessary condition for a government to pursue other policy aims and to create a stable and peaceful environment for everyday economic activities. In practice, national security typically refers to the capacity of a nation to mobilize its military forces to safeguard the integrity of its borders and to deter or successfully defend against physical threats, including military aggression and attacks by non-state actors, such as terroristic organizations. For these reasons, every nation allots a

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https://doi.org/10.1016/j.eap.2022.12.011
0313-5926/

substantial part of its fiscal budget to defense programs (Schick, 2008). To be specific, globally, approximately ten percent of a country's government budget is allocated to military expenditures. Nevertheless, since the end of the Cold War at the beginning of the 1990s, the defense budget in most countries has dropped sharply. However, several recent geopolitical events reversed this trend, including the Russian invasion of Ukraine, the rapid proliferation of conventional weaponry in the Middle-East region, and the political pressure put by the U.S. on the other NATO members to raise their annual defense budgets.

According to the extensive literature, there are different political, economic, and strategic motives for politicians to invest in the military, including the existence of internal and external threats to national security, the international status of the armed forces, access to military technology, a signal of deterrence, the status of the military in domestic politics and supporting the economic position of the domestic defense industry (see, for instance, Wezeman, 2014; Smith, 1995). Special attention has also been paid to the role of general elections on defense spending, as the priority put on the military capabilities of a state typically differs between politicians and voters. Generally, voters assign just a low priority to military spending, as they consider it to be less important in periods of peace. In turn, politicians prefer to spend more on the military as national defense is perceived as a general measure of status and prestige (Bove et al., 2017). The differences in preferences might lead to rent-seeking behavior and election-induced changes in military expenditures to increase the odds of being re-elected. However, the direction of these electoral cycles in the defense budget is less clear. On the one hand, the incumbent government may want to try to ensure their re-election by boosting the performance of the national economy and employment rate. One possible way to achieve this is by raising the defense budget and spending it on domestically produced military items. On the other hand, it might be more favorable for the ruling parties to cut defense spending in an election year to finance expansions in other public spending categories that are preferred more by voters. This latter effect is further reinforced since most countries lack an economically meaningful defense industry. Consequently, a great part of the commercial benefits resulted from defense spending will directly flow abroad and not boost the domestic economy.

More than four decades after the seminal contribution of Nincic and Cusack (1979) on the impact of U.S. presidential elections on the federal defense budget, numerous studies have looked into this debate from an empirical point of view (see, among others, Mintz and Hicks, 1984; Mintz, 1988; Van Dalen and Swank, 1996; Bove et al., 2017; Kollias and Paleologou, 2003; Wielechowski, 2018). Despite this vast number of articles, the empirical literature contains many conflicting results. Several studies report a significant positive effect of elections suggesting that governments expand their defense expenditures when elections are upcoming (e.g., Nincic and Cusack, 1979). In contrast, many other studies conclude that the defense budget is not subject to election cycles or, at times, even that governments cut the budget when elections are approaching (e.g., Bove et al., 2017).

Although various recent studies offer a comprehensive overview of the existing literature, none have yet explored the outcomes of the empirical studies in a systematic way (see, e.g., Bove et al., 2017). In turn, the contribution of this study is that it analyzes the existing literature on the relationship between election cycles and military expenditures using a so-called meta-regression analysis (MRA). Meta-analysis is a methodology that allows me to draw a more general picture of the impact of elections on defense spending than I may arrive at when looking at a single study. When using meta-analysis, each estimation reported in a study is taken as a single observation containing information on the nature of the relationship between upcoming elections and military expenditures. In the meta-analysis, I specifically investigate a number of issues. First, I try to discover if there exists a genuine relationship between upcoming elections and defense spending. Second, I analyze whether studies suffer from a potential publication bias, i.e., whether results published provide a biased distribution of effects found, because there may be a tendency not to publish results that show no significant results. Finally, I analyze the potential impact of the study design on the results reported. In particular, I focus on the effect of differences between studies regarding country samples, time periods, econometric specification, data construction, and estimation methods. Thus, the overarching aim is to account for all these study-to-study differences and quantify the net overall effect.

In three closely related studies, Philips (2016) and Cazals and Mandon (2019) apply the meta-analysis approach to detect whether there are election cycles visible in total government spending, which naturally also includes military expenditure. These meta-analyses share two conclusions. First, they claim that leaders actually manipulate fiscal policy when elections are upcoming by following more expansionary spending policies in order to improve the chance of being re-elected. Second, the extent of this election effect is significantly exaggerated by scholars to increase the likelihood that the study will be published as a journal article or book chapter. However, this study is the first that focuses specifically on election-motivated manipulations in military spending, as it is questionable whether defense spending follows the same pattern as other public spending categories. For some governments, it might be more favorable to cut defense expenditures to finance election-motivated expansions in other public spending categories. In particular, the meta-analysis of Philips (2016) reveals that welfare spending is increased in an election year, while they do not find any statistically significant evidence that tax revenues or the budget deficit are affected when elections are upcoming. This finding suggests that, in an election year, there might be a switching pattern present between different spending categories to avoid large budget deficits or tax increases. A tempting candidate for these cuts to finance additional expenses in other public spending categories is military expenditures, as they are ranked among the least preferred by the median voter (Bove et al., 2017).

However, one difficulty in this particular meta-analysis is that it tests two competing hypotheses—a contraction hypothesis versus an expansion hypothesis. For this purpose, I apply a twostep analysis. First, I estimate a pooled

Hierarchical Linear Model to explore if any of the two hypotheses dominate. Second, I apply a mixture finite model and logit model to see if there coexist significant evidence for both hypotheses. In particular, using over two-hundred election estimates and applying different sensitivity tests, the main results indicate that there is only a weak negative genuine effect of elections on military expenditures. This finding suggests that governments follow a more contractionary policy in defense spending when elections are approaching. However, it turns out that this impact relies to a great extent on the country sample used and the time periods covered in the study. It appears that the relationship between elections and military spending is likely to be positive in countries where the defense industry has some considerable political influence and economic power. Also, in countries that have to deal with serious security risks, this positive relationship is likely to prevail. In most other countries, this relationship is expected to be negative or insignificant. Moreover, in the Cold War period, the genuine election effect is positive, while in the post-Cold War period, this effect turns out to be negative as voters have largely changed their security perceptions between these two time periods. Finally, I find any evidence that a publication bias influences the election effect found among the considered studies. This is partly explained by the fact that both positive, as well as, negative results are theoretically plausible and are, therefore, apparently equally likely to be published.

The remainder of the paper is organized as follows. In Section 2, I present the theoretical foundation underlying the empirical relationship between election cycles and military expenditures, while Section 3 outlines the research process and the methodology of the meta-regression analysis. Section 4 contains the results of the meta-regression analysis. Finally, Section 5 offers the conclusions and discussion.

2. Theoretical background on electoral cycles in military expenditures

The relationship between elections and fiscal policy is explained by the political budget cycle theory (PBC). Political budget cycle research examines election cycles in public spending, taxes, and budget deficits (see for a detailed and comprehensive review of the existing literature De Haan and Klomp, 2013). This theory emphasizes the incumbent's intention to secure his democratic re-election by maximizing his expected vote share at the next election (Nordhaus, 1975). It is assumed that the electorate is backward-looking and evaluates the government based on its track record of the recent past. Voters generally prefer candidates from whom they expect to deliver greater material well-being and better aggregate economic performance (Franzese, 2002). This would imply that governments have a great incentive to adopt expansionary fiscal policies by expanding spending or cutting taxes in the late year(s) of their term in office (Brender and Drazen, 2005). Increasing the defense budget before an election would inject money into the domestic defense sector, create jobs and raise corporate profits (Mayer, 1992, 1995). As a result, the national economy will benefit.

Meanwhile, this election-motivated incentive might be further reinforced since politicians frequently advocate that the interests of the defense industry should have a high priority in policy decisions as self-sufficiency in the supply of weapons is a vital element for having an independent national security policy (Heidenkamp et al., 2015). As a result of this policy stance, there are close ties between politicians and defense-minded corporations in many arms-producing countries, the so-called military-industrial complex (Eisenhower, 1961; Mayer, 1992, 1995). A driving force behind this relationship is that both sides benefit from this connection—one side from obtaining weaponry and the other from being paid to supply them. This intertwining is especially visible in the United States, where the revolving door has gone in two directions. Former top managers in the defense industry have secured key government positions and vice versa as higher-ranked (retired) military staff is regularly appointed in the managing board of defense companies (Luechinger and Moser, 2014; Moore, 2010; Rundquist, 1978). This reciprocity creates an environment where votes are traded in return for economic benefits or political favors, such as increasing the defense budget or domestic procurement (e.g., Kim, 2019; Tripathi, 2000; Fleisher, 1993). In return, the defense industry is frequently ranked among the top industries that spent the most on election campaign contributions in many arms-producing countries.

Initially, it was expected that political budget cycles only exist in democratic countries as it assumes that elections are competitive and fair. However, several recent studies argue that election cycles may also occur in autocracies (see, for instance, Blaydes, 2006; Wright, 2011; Hyde and O'Mahony, 2010; Ebeke and Ölcer, 2013). This debate primarily rests on three arguments. First, in autocracies, the incumbent has the incentive to buy political support in the later years of his term from the elite on which his power rests, including the armed forces. The idea is that even in authoritarian systems, political leaders have to be sufficiently popular with the elite to avoid contestation and removal (Finer, 2002; Maniruzzaman, 1992). The influence of the elite stems from the lack of a well-functioning system of checks and balances that is necessary to achieve democratic accountability. This shortcoming raises the opportunity for the non-elected elite, such as the military, to gain political power. This is especially the case in political regimes where the military plays a pivotal role in ensuring that the ruling elite stays in power (Pamp and Thurner, 2017; Bove and Brauner, 2016). As a result, in these countries, policy decisions reflect the interests of the armed forces and the ruling elite rather than the welfare of the population at large.

A second explanation for the existence of electoral cycles in defense spending in autocratic countries can be found in the so-called deterrence theory arguing that military spending in an election year sends out a powerful message to potential and actual challengers or foes of the incumbent leader that it will repress any political actions of opponents. A final argument of electoral cycles in the defense budget of autocratic countries is related to the influence of electoral accountability on the likelihood of war. According to Hess and Orphanides (1995, 2001), an incumbent with low

performance in handling the domestic economy, which is often the case in autocracies, has incentives to undertake a quickly winnable war to display leadership capabilities and increase the odds of re-election (Marinov et al., 2015).

The foregoing considerations provide the basis for the first hypothesis that has been tested in many empirical studies.

H_1—Expansion hypothesis**:** *Governments increase defense expenditures in an election year.*

However, several subsequent studies actually assert the opposite of hypothesis 1. This alternative debate is mainly concentrated around three key arguments. First, one fundamental assumption underlying the predictions of Hypothesis 1 is that countries, after all, should have a domestic defense industry. However, most countries lack an economically meaningful defense industry that has a nationwide effect. As a result, the commercial benefits accruing from the increased defense expenditures will flow mainly abroad and not stimulate the domestic economy. This expectation is also largely confirmed in the literature on the fiscal multiplier effect of defense expenditure, as this multiplier is typically lower compared to the multiplier effect of other public spending categories (Barro and De Rugy, 2013). In particular, more than eighty percent of the revenues in the arms market are concentrated in only seven countries—the United States, the United Kingdom, Russia, China, France, Germany, and Italy.

A second argument against hypothesis 1 is that voters do not only reward fiscal expansions when elections are upcoming, but also punish politicians who finance these expansions by taxes or borrowing (Peltzman, 1992). This argument implies that election cycles do not necessarily affect aggregate expenditures, but are more likely to create a switching pattern between different spending categories to avoid large budget deficits or tax increases. In particular, election cycles show up as some pivotal groups of voters are targeted by the incumbent at the expense of others (see Drazen and Eslava, 2006, 2010; Gonzalez, 2002; Kneebone and McKenzie, 2001; Potrafke, 2010; Thies and Porche, 2007; De Haan and Klomp, 2013). More specifically, voters tend to favor welfare spending and reward incumbents with similar spending choices. Applying this latter logic would suggest that governments are likely to cut military expenditures in an election year to finance an expansion in other categories such as social security, education, health care, or public infrastructure that are more visible and valued by voters and are likely to attract more votes (Russett, 1982; Nincic and Cusack, 1979; Cusack and Ward, 1981; Griffin et al., 1982a,b; Mintz and Hicks, 1984; Zuk and Woodbury, 1986; Kamlet and Mowery, 1987; Mintz and Ward, 1989; Mintz, 1988; Su et al., 1993; Efthyvoulou, 2012).

On a related note, recent theoretical PBC models emphasize the role of temporary information asymmetries regarding the politicians' competence level in explaining electoral cycles in fiscal policy. In these models, signaling is the driving force behind the PBC (see, e.g., Rogoff and Sibert, 1988; Tabellini and Persson, 2003; Shi and Svensson, 2006). The larger the number of voters that fail ex-ante to distinguish election-motivated fiscal policy manipulations from incumbent competence, the more the incumbent profits from behaving opportunistically and boosting expenditures before an election. Traditionally, voters assign low priority to military spending, as they consider it to be less important in periods of peace. In contrast, politicians prefer to spend more on the military as national defense is perceived by politicians as a general measure of status and prestige. Voters cannot observe this priority and need to ex-ante distinguish between politicians who manipulate the budget composition to attract votes and those whose spending preferences are consistent with what the median voter wants. Hence, they form expectations regarding the type of politician and, thus, the post-electoral spending by observing the pre-electoral allocation. Following this line of reasoning implies that before the election, an incumbent politician will shift the composition of spending away from defense and towards other spending categories to signal that his preferences are close to those of voters.

Finally, typically the benefits of a security policy are only realized in the long-term, whereas the costs are realized in the short-term. This provides incumbent leaders with an incentive to underinvest in such policies if elections are upcoming. Competent incumbents will have the incentive to signal their ability by keeping costs low, while still meeting the larger national security goals. One can argue that reducing participation in peacekeeping missions or foreign military operations fits this logic described (Buts et al., 2017; Marinov et al., 2015). Especially since these operations typically create a lot of ex-post media attention when there are many civilian causalities to be mourned. This, in turn, will influence the voter perception about international peace and security and provides information about the competence of the incumbent. As a result, democratic leaders have the incentive to underinvest in troop contributions close to elections, which might lead to a lower defense budget.

Thus, based on these more recent insights of the PBC theory, it is expected that governments will cut their defense expenditures in an election year. This yields the alternative hypothesis tested in several studies.

H_2—Contraction hypothesis: *Governments reduce defense expenditures in an election year.*

These opposing theoretical predictions, formulated in the two hypotheses, illustrate that the impact of election cycles on military spending is contested. Thus, the question of whether elections affect military expenditures, and if so, in which direction, might ultimately be an empirical one.

3. Methodology and research process

3.1. Selection of the studies included in the MRA

Meta-regression analysis has become an increasingly popular instrument in economics and political science to examine particular fields of empirical research, especially if there are many alternative specifications leading to diverging

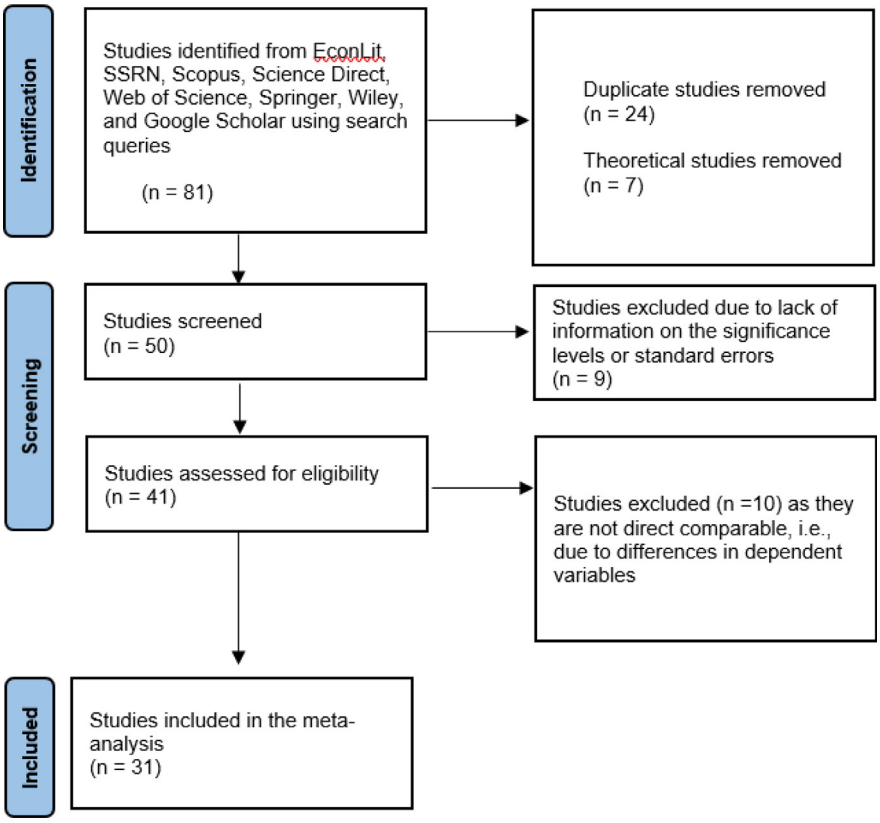

Fig. 1. Selection procedure of the included studies.

conclusions. In this meta-analysis, I focus on the relationship between elections and defense spending. As discussed in Section 3.1, the impact of elections on military expenditures is theoretically not directly clear and has therefore generated considerable coverage over the years. As a result, the impact of elections lends itself perfectly for a meta-analysis. However, to the best of my knowledge, such an analysis has not been done so far specifically for the election effect in military spending.

In conducting the MRA, I follow the general guidelines for best practices provided by Stanley et al. (2013) and Havránek et al. (2020). Basically, a MRA breaks down into three stages. The first one is the collection of all the relevant studies that meet the objective criteria I have predefined. The second step, is the coding of the estimates encompassed in these studies resulting in a dataset ready to be used in an empirical analysis. The third and last part is the statistical analysis of this dataset. In this section, I will discuss the first two steps.

Fig. 1 illustrates the research process followed. In the first step, I start searching for studies by using the Ideas RePEc database as my primary source. I searched using different broad keyword combinations. In this regard, 'elections' and 'military expenditures' are used as the main keywords. Additional searches are conducted using 'electoral cycles', 'election cycles', 'political budget cycles', 'defense spending', 'defense expenditures', 'defense budget' as well as 'military spending' as keywords. For the sake of completeness of my literature search, I have browsed other databases as well, such as EconLit, SSRN, Scopus, Science Direct, Web of Science, Springer, Wiley, and Google Scholar, using search queries with the same keywords. Moreover, as there is still the concern that I may have missed some relevant studies, I undertook a complementary manual search. First, I looked for additional studies in the references listed in the papers already selected. Second, I checked the publications and working papers of the authors identified in the first round. I stopped searching on June 1st, 2021. The search included all potentially relevant published and unpublished studies from 1979 up to and including 2020.

This research procedure left me with more than eighty studies. However, not all studies can be used in the meta-analysis. In selecting the studies to be included, I use three criteria. First, studies should look at the direct election effect on military expenditures, where spending is measured as the percentage of GDP, the share of government spending, per capita, or total. Thus, studies that, for example, use the total number or value of defense procurement contracts as their dependent variable are left out of the analysis (see, for instance, Mayer, 1992, 1995; Derouen and Heo, 2000; DeRouen and Heo, 2001). Also, studies exploring electoral manipulations in the outcome of the defense budget are excluded. For instance, Buts et al. (2017) and Marinov et al. (2015) investigate the impact of upcoming elections on troop deployment in

Table 1
Studies included in the meta-analysis.

Author	Publication year	#estimates	Sample
Blum and Potrafke	(2020)	12	Cross country
Bove et al.	(2017)	6	Cross country
Caruso and Francesco	(2012)	8	Italy
Castro	(2017)	6	Cross country
Castro and Martins	(2018)	16	Cross country
Cusack	(1989)	1	US
Cusack and Ward	(1981)	1	US
Deng and Sun	(2017)	14	Cross country
Eloranta	(2017)	2	Cross country
Enkelmann and Leibrecht	(2013)	5	Cross country
Griffin et al.	(1982a)	11	US
Griffin et al.	(1982b)	9	US
Hunter and Robbins	(2016)	16	Cross country
Kamlet and Mowery	(1987)	15	US
Kollias and Paleologou	(2003)	1	Greece
Kuokštytė et al.	(2021)	3	Cross country
Mintz	(1988)	8	Israel and US
Mintz and Hicks	(1984)	6	US
Mintz and Ward	(1989)	1	Israel
Nincic and Cusack	(1979)	4	US
Nordvang	(2018)	16	Sweden
Potrafke	(2020)	21	Cross country
Sezgïn	(2010)	4	Turkey
Su et al.	(1993)	1	US
Van Dalen and Swank	(1996)	2	Netherlands
Ward and Mintz	(1987)	1	Israel
Whitten and Williams	(2011)	8	US
Wielechowski	(2018)	1	Cross country
Wielechowski	(2019)	1	Cross country
Yap	(2010)	4	Taiwan & South Korea
Zuk and Woodbury	(1986)	8	US

peacekeeping missions or foreign military interventions, while Klomp (2021) and Mayer (1992, 1995) examine the effect of election cycles on respectively the sales revenues received by major defense companies or the value of closed contracts. Second, both published and unpublished studies are taken up in this analysis. Arguably, it is expected that there are relatively small errors in peer-reviewed studies, and therefore these studies are somehow preferred in the meta-analysis. However, this selection criterion is generally criticized since there may be a tendency towards significant studies only to be published. Meanwhile, working papers are essential in economics and political sciences because of the long publication lags (Cazals and Mandon, 2019). I only retain empirical articles written in English and have removed master theses from the sample as they primarily have an educational purpose and are not so much meant as high-quality academic research. Finally, empirical studies are only included when they report not only the size effect, but also provide information on the significance level (t-statistic, *p*-value, or standard error) and degrees of freedom or sample size. In this way, it allows me to calculate the correlation coefficient for individual estimates. I only include a study in the sample when this information is complete and is left out otherwise. From the studies that only report the estimated coefficient, I have tried to contact the corresponding authors to request the missing information. Based on this systematic search, I ended up having a set of 31 studies containing more than two hundred estimates of the election regression coefficient. Table 1 lists the studies included in the analysis and describes the country sample employed in each study.

Most studies that investigate the presence of election cycles in military expenditures estimate variants of a single-equation panel model. This model specification is typically given by

$$milex_{it} = \alpha + \beta^{n}\mathbf{x}^{n}_{it-q} + \gamma\, elec_{it} + \varepsilon_{it} \tag{1}$$

where the dependent variable *milex* is the military expenditures in country i in year t, the parameter α is the intercept, $\mathbf{x^n}$ is a vector including n (lagged) control variables, *elec* is the election indicator, and γ is the size effect of the election coefficient and the parameter of interest in this meta-analysis. Finally, the parameter ε_{it} is the error term.

In the remainder of Section 1 will discuss some descriptive statistics related to the included primary studies. In total, there are 207 partial correlations. About 25 percent of the estimates find a significant negative relationship, whereas less than 19 percent of the estimates find a significant positive relationship between elections and defense spending. The average t-statistic of the election indicator of all regressions in the sample is around −1.02. This average indicates that the upcoming elections do not affect military spending at commonly used significance levels. Arguably, positive (negative) significant effects may be likely to be offset by negative (positive) ones. The average sample size used in the gathered primary studies is about four hundred observations per estimation, while on average more than thirty countries are included.

3.2. Funnel plot analysis

Meta-analysis employs both the visual inspection of graphs and a statistical analysis, as prescribed by the general guideline. In the first step of the analysis, I have constructed a funnel graph. A funnel graph plots the estimates of the election effect on military expenditures collected in the literature (horizontal axis) against a measure of the precision of these estimates (vertical axis). The precision is typically measured by the inverse of the estimated standard errors. In essence, funnel plots illustrate how the estimates are distributed. Most of the estimates lie at the bottom of the graph. They are, by definition, not precise, and they vary across a wide range of estimate values. Moving to the top, the more accurate, the more concentrated around a single value are the estimates. This value is supposed to reflect the "true" genuine effect of electoral manipulations in defense spending. If different studies estimate a common effect, they should be randomly distributed around a common mean.

The second piece of information I may infer from such graphs is potential selection bias in the literature. In the absence of such a bias, points should be symmetrically distributed around this "true" effect. Any skewness suggests selection bias towards the direction where inclines the distribution (see also Stanley and Doucouliagos, 2010; Yesilyurt and Yesilyurt, 2019). If there is publication bias, where only significant results of the expected sign are published, then only studies where the coefficient is large relative to the standard error get published. In turn, if the distribution is centered around zero, one should conclude the absence of such manipulation.

One complicating factor in constructing the funnel plot is that reported coefficients cannot directly be used to compare size effects due to differences in measures and scales of the dependent and election variables. In contrast, to ensure comparability across the estimates, I convert the coefficients collected into partial correlations using the following formula

$$PCC_i = \frac{t_i^2}{\sqrt{t_i^2 + df_i}} \tag{2a}$$

The corresponding standard error of this precision measure is given by

$$SE_i^{PCC} = \sqrt{\frac{1 - PCC_i^2}{df_i}} \tag{2b}$$

where t and df refer respectively to the t-statistic and the degrees of freedom of the individual estimate i collected from the primary studies. Although the sample size is almost always reported by authors, this is rarely the case for degrees of freedom. Fortunately, partial correlations are weakly sensitive to imprecise degrees of freedom calculations (Stanley and Doucouliagos, 2012; Yesilyurt and Yesilyurt, 2019). This uncertainty is especially marginal as sample sizes in the military PBC literature are typically large. Since this calculation creates a positive partial correlation by construction, it must be converted into a negative correlation if the t-statistic carried that sign, thus bounding the correlation between -1 and 1. This standardization removes the economic meaning of effects but still informs on the magnitude and direction of associations between elections and fiscal manipulation and makes them quantitatively comparable.

Based on the empirical studies, the overall partial correlation coefficient is found to be approximately 0.03. According to the threshold values provided by the Cohen standards, the absolute standardized effect is small if less than 0.10, moderate if it is around 0.25, and large if greater than 0.40 (Cohen, 2013). Using these thresholds indicate that, although the effect is positive, the economic significance is practically negligible. However, one important note in interpreting this figure is that it is only valid if the assumption of homogeneity is not violated and there is no publication or selection bias present.

Fig. 2 provides the funnel plot for the included studies that estimate the relationship between elections and military expenditures. The graph indicates that the distribution is quite symmetrical around zero suggesting the absence of a selection bias as both positive as well as negative findings are being reported and published. This is in accordance with the predictions from the PBC theory, as it theorizes both a positive, as well as, a negative relationship between elections and defense spending. Additionally, the partial correlations at the top of the graph are close to zero. This finding implies that there is a tendency to have any economic effect when the precision increases.

3.3. Meta-regression analysis: Approach

The key research issues in this meta-regression analysis are whether there is a common genuine effect present in research on the link between elections and defense spending and whether a meaningful election effect remains after a publication bias is filtered out. However, the interpretation of funnel graphs can be somewhat subjective, so a more formal funnel asymmetry test (FAT) or precision effect test (PET) is applied. These tests examine whether the estimated effect size is uncorrelated with the standard error as one would expect with no publication bias. Drawing on Doucouliagos and Stanley (2009) and Stanley and Jarrell (2005), I can explain a typical meta-regression model as follows (see also Stanley, 2005, 2008; Yesilyurt and Yesilyurt, 2019).

$$PCC_i = \beta_1 + \beta_0 SE_i^{PCC} + \alpha_k Z_j^k + e_i \tag{3}$$

where PCC_i is the partial correlation on the relationship of interest, in my case, capturing the correlation between elections and defense expenditures, SE_i^{PCC} is the standard error of the partial correlation computed above, the vector $\mathbf{Z^k}$ includes k

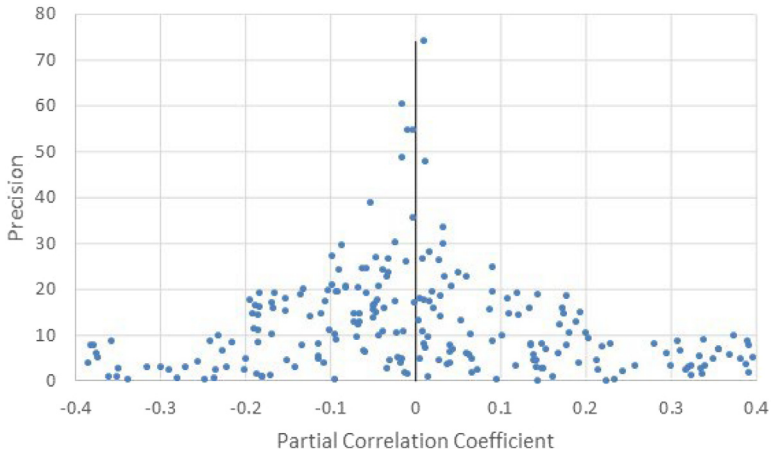

Fig. 2. Funnel plot.

meta-independent variables reflecting differences across studies, α_k is the meta-regression coefficient, which reflects the effect of particular study characteristics, and e_i denotes the meta-regression disturbance term.

In the absence of any publication selection, observed effects should vary randomly around the 'true' value, β_1, independently of the standard error. When all studies are selected for statistical significance, then the publication bias will be proportional to the standard error, $\beta_0 SE_i^{PCC}$. Authors of smaller studies are more likely to engage in specification searchers, on average, to find the sufficiently large estimated effects needed to compensate for their associated larger standard errors. This allows the very common tendency of researchers and reviewers to prefer statistically significant results and for researchers, therefore, to re-run their analysis until they find such significance (Doucouliagos and Stanley, 2009). With increased observations, SE_i^{PCC} will become smaller, approaching zero as the sample size grows indefinitely, and the reported effects will approach β_1, the 'true' effect.

As explained above, studies that try to explain the same relationship usually use different sample sizes and modeling variations. Hence, the random estimation errors of this meta-regression analysis model, e_i in Eq. (3), are likely to be heteroscedastic. In an unusual econometric twist, the independent variable, SE_i^{PCC}, is a sample estimate of the standard deviation of these meta-regression errors. More formally, dividing Eq. (3) by this measure of the heteroscedasticity (SE_i^{PCC}) gives the weighted least squares version of Eq. (3).

$$t_i = \frac{PCC_i}{SE_i^{PCC}} = \beta_0 + \beta_1 \left(\frac{1}{SE_i^{PCC}}\right) + \sum_{k=1}^{K} \alpha_k \left(\frac{Z_j^k}{SE_i^{PCC}}\right) + e_i \tag{4}$$

where t_i is the t-statistic on the election partial correlation coefficient of the ith regression, β_1 is the 'true' value of the parameter of interest, which is called the precision-effect test (PET) (Stanley, 2008). When β_1 is statistically different from zero at common confidence levels, there is a significant relationship between elections and military expenditures. The conventional significance test of the intercept of Eq. (4), β_0, is a test for the existence of a publication bias, in which the sign indicates the direction of this bias, this is the so-called funnel-asymmetry test (FAT). In particular, the no publication bias hypothesis corresponds to the constant in this regression being equal to zero (Egger et al., 1997).

In the analysis, the unit of observation is not a study, but each estimate that is reported. Many studies contain more than one regression, for instance, when they test for the sensitivity of their results using different country samples or time periods. However, one difficulty encountered in that case is that the estimates reported in a single study using the same dataset are not statistically independent (Doucouliagos and Ulubaşoğlu, 2008; Doucouliagos, 2005). As a result, the OLS estimation technique would deliver biased MRA estimates. This is corrected by using a Hierarchical Linear Model (HLM), which is a particular regression technique that is designed to take into account the hierarchical structure of data (Raudenbush and Bryk, 1986). Eq. (4) can be rewritten as

$$t_{ij} = \beta_{00} + \beta_{j1} \left(\frac{1}{SE_{ij}^{PCC}}\right) + \sum_{k=1}^{K} \alpha_k \left(\frac{Z_{ij}^k}{SE_{ij}^{PCC}}\right) + \sum_{n=1}^{N} \gamma_n \left(\frac{V_{0j}^n}{SE_{ij}^{PCC}}\right) + \omega_i + \nu_j \tag{5}$$

where t_{ij} is the t-value of the ith election estimate in study j. The vector of meta-independent variables is split up into two parts. One part explains the differences between regressions and estimates $\mathbf{Z^k}$, while the other part explains study differences $\mathbf{V^n}$. The variation among empirical results may be explained by various study characteristics or model specifications. The types of design elements that are included in this MRA are based on (1) the construction of the data; (2) empirical specification; (3) country and time sample; (4) estimation method, and (5) research objective and publication

outlet (the detailed list of explanatory variables used in the study is provided in Table 3 later on). The ω_i and υ_j are the error terms on estimate and study level, respectively.[1]

According to Doucouliagos and Ulubaşoğlu (2008), there is some disagreement in meta-analysis in economic science on the treatment of independent studies that are drawn from the same dataset. This contrasts with medical science, where meta-regression analysis is rooted, in which each treatment delivers its own unique dataset. In the studies included in the meta-analysis, the majority use military spending data from the SIPRI dataset.

It is, however, standard practice in meta-analysis in economics to treat the studies of different authors as independent estimates even if they use the same dataset (see Abreu et al., 2005; Hunter and Schmidt, 2004; Stanley, 2001). The differences among regression results are only partly explained by the data source used. Other explanations come from the sample used, econometric specification, and estimation technique applied. In particular, in a meta-analysis, empirical estimates are regarded as statistically independent if they are reported by a different author, or if the same author reports them, different samples are used. Magnus and Morgan (1999) demonstrate that even in the case where the exact same data are used, very different econometric estimates are likely to result. The problem of a common data source is only a concern when two studies use the exact same empirical specification, sample, and estimation technique, for instance, for replication purposes. In that case, there is some double-counting present which makes the MRA results potentially biased. However, in my sample, no estimation is identical.

4. Meta-regression analysis: Results

4.1. FAT-PET regression test

Table 2 presents the estimation results of the FAT-PET regression test. To reduce any further statistical dependence in the dataset, I use the bootstrap resampling estimation technique with 1000 replications (Doucouliagos, 2005; Doucouliagos and Ulubaşoğlu, 2008; Shao and Tu, 2012). Column (1) of Table 2 shows that about thirty percent of the total variance is contributed to the variance on the study level. This implies that there is dependence within a study and that a multilevel model is the appropriate model to use. The parameter of the inverse standard errors is negative, but statistically insignificant, demonstrating that military expenditures are generally unaffected when elections are upcoming. However, an alternative explanation for the lack of evidence for a genuine effect is that both positive and negative election effects are found in the literature. The PBC theory discussed above already suggested that the election effect can go in either direction depending on whether or not the additional defense spending will boost the domestic economy and the public spending preferences of voters. Apparently, aggregating all studies will cancel out any individual effect as positive effects that have been found in some studies are offset by negative ones reported by other studies.

Moreover, the constant, β_{00}, is also statistically insignificant at common confidence intervals, meaning that the effect found in the military PBC literature is not subject to a publication bias. Again, this may not be surprising since there is no strong theoretical expectation about the sign of the effect, so both positive, as well as, negative findings might be equally likely to be published. However, this, raises the follow-up question of whether significant results, disregarding their direction, are being published more easily than insignificant results. To explore whether this is the case, I use in column (2) the absolute t-value as the dependent variable. Nevertheless, I find again no statistically significant evidence that the results published are biased towards significant findings.

An alternative way to test for a genuine effect and publication bias is to use a meta-significance test. In this approach, the logarithm of the degrees of freedom is regressed on the absolute t-value of the election coefficient. If the coefficient found on the degrees of freedom is significant, then there exists a genuine effect. The intuition behind this test is that the standardized test statistics vary positively with degrees of freedom because the parameter estimates of large sample studies have a higher precision value. Therefore, when the coefficient on the degrees of freedom is significantly larger than zero, there is a genuine election effect on military expenditures. However, when this coefficient is significantly smaller than zero, the empirical literature suffers from a publication bias because the t-values of the estimated election effects decrease when the size of the observation increases.

The results of this test are shown in column (3) of Table 2. The coefficient on the number of observations is statistically insignificant at commonly used significance levels. This implies that there is, again, no genuine empirical relationship between upcoming elections and defense expenditures. The absolute t-statistic of the election effect does not rise as the size of the observation increases. One explanation is that estimates that are based on a large number of observations typically employ a multi-country analysis. By using a large panel dataset including a diverse set of countries, the likelihood drops that any individual effect will dominate.

To sum up the first results, I do not find any empirical support for the hypothesis that there exists a significant relationship between general elections and military spending. At the same time, I also do not find any evidence that the literature on the link between elections and defense expenditures suffers from a publication bias. As argued above, one explanation is that theoretically, it is a priori not clear what impact elections should have on military spending and that this relationship can possibly be both in the positive and negative direction, depending on country-specific characteristics. This latter argument fits the more general PBC literature as several recent reviews of this literature conclude that the

[1] See also Doucouliagos (2005) and Mookerjee (2006) and Klomp and De Haan (2010).

Table 2
FAT-PET test.

	t-statistic election coefficient (1)		|t-statistic election coefficient| (2)		|t-statistic election coefficient| (3)	
	Coefficient	z-value	Coefficient	z-value	Coefficient	z-value
Constant	−0.085	−1.19	0.052	1.52	0.051	1.05
Inverse standard errors	0.003	1.53	0.002	1.14		
Log number of observations					0.001	1.25
Intra-class correlation	0.312		0.311		0.289	
Number of observations	201		201		201	
Maximum Likelihood ratio *p*-value	0.000		0.000		0.000	
R-squared	0.198		0.169		0.152	

Note: **/* indicates significance at respectively 5 and 10 percent level.

existence of elections cycles in public spending relies to a great extent on the economic conditions and political situation present in a country (see, for instance, De Haan and Klomp, 2013; Dubois, 2016). This latter observation makes it clear that it is important to reveal the mechanism underlying these preliminary results and try to identify which countries or groups of countries are following a contractionary defense policy and which other countries are expanding their military spending when elections are upcoming. This will also be one of the aims of the remaining sections. In particular, I include variables into the MRA to control for different econometric specifications, country samples, data construction, and estimation techniques.

4.2. Controlling for heterogeneity across studies and estimates

4.2.1. Construction of the data

The first discussed set of variables that might explain the observed variation in the election effect found is related to the construction of the election indicator and the source of the military expenditures data used. Most studies base their dependent variable either on the time series or panel data taken from the Military Expenditure Dataset reported by the Stockholm International Peace Research Institute (SIPRI), the Military Balance collected by the International Institute for Strategic Studies (IISS), or the World Military Expenditures and Arms Transfers (WMEAT) dataset produced by the Bureau of Verification and Compliance of the U.S. Department of State. Although the pairwise correlation between these datasets is very high, there still exists at least three important discrepancies that make it difficult to compare the figures directly, and hence, the empirical estimates that are based on the different data sources. First, the datasets use different definitions for military spending. For instance, whereas SIPRI bases its data on actual expenditures, the figures of the IISS rely on the pre-determined budget. Consequently, actual military expenditures generally exceed defense budget figures as it includes military-related expenses often contained in other budgets, such as military pensions or assistance provided to the department of foreign affairs.

On a similar note, the SIPRI dataset includes only the purchase of major conventional weapon systems within a calendar year, while the WMEAT dataset also includes accessory equipment, technical support, maintenance and training services, dual-use commodities, and small and light weapons for a fiscal year. Likewise, SIPRI attempts to measure the volume of transfers of major weapons systems as quantities are multiplied by an estimate of the unit production costs, irrespective of the price actually paid. In contrast, WMEAT tries to measure the value of arms transfers, reflecting the price actually paid (Smith and Tasiran, 2005).

A second issue is the different ways of dealing with missing data in these datasets. A considerable number of states remain reluctant to fully submit information on their military spending within a joint comparative framework to an international body. Some datasets try to fill the gaps of missing data using an iteration process, while other datasets leave these observations blank. As a result, the countries covered in the various datasets differ to a certain degree which is, of course, one explanation of why the results among studies may diverge. A final difference is the use of different currency conversion mechanisms. Purchasing power parity-based conversion mechanisms result in significantly higher military spending figures than conversions based on market exchange rates.

In spite of the studies included in the meta-analysis all use military expenditures as their dependent variable, these studies still differ in the way they have scaled or standardized this variable (i.e., as a share of GDP, as a percentage of the total government expenditures, per capita, taken in logarithmic scale, etc.). To control for the computation of the dependent variable, a series of dummies is created and included in the MRA (see Table 3 for more details).

Turning to the construction of the election variable, one of the main empirical challenges in the PBC literature is to capture electoral cycles in an accurate and relevant manner. As a response, authors have relied on different empirical approaches to capture electoral cycles. The first, and most common way, is a dummy taking the value one in a year when an election is scheduled or, alternatively, the year before it takes place. One disadvantage of this approach is that it ignores the timing of elections within the course of a year. In order to better capture leaders' behavior during the year preceding elections, scholars have offered various refinements to this electoral year dummy, such as coding elections according to which period of the year they occur, i.e., the election variable is calculated as $M/12$ in an election year and $(12 - M)/12$

in a pre-election year, where M is the month of the election. In all other years, its value is set equal to zero. With this method, the election variable is intended to measure how much of a given year may actually be considered as pre-electoral (see also Franzese, 2000). Lastly, several subsequent studies use the number of years up to the elections as their election measure, arguing that election cycles in military spending take longer than just one year.[2] Some studies include next to a (pre-)election variable, also a post-election measure to capture the difference in the year after the elections.

To evaluate the contribution of an election effect in the military budget, it is important to control for the fact whether the incumbent has the authority to control this budget. Albeit facing some arbitrariness of choice, regularly studies control for legislative elections in the case of parliamentary countries and executive elections for presidential regimes. In particular, when the president or prime minister has no legislative powers in the realm of fiscal policy and is accountable to a parliamentary majority that can bring the government down by voting "no confidence", these countries are typically classified as having a parliamentary system.

Moreover, governments need sufficient time to change their military budget. When elections are sudden and unexpected, for instance, when there are elections shortly after the fall of a cabinet, the government may have little opportunity to use fiscal policy as a re-election strategy. For this reason, some studies only include pre-determined elections. In this case, elections are typically included only if it (1) is held on the fixed date (year) specified by the constitution, (2) are held in the last year of a constitutionally fixed term for the legislature, or (3) the election is announced at least one year in advance.

Finally, scholars have theorized about contextual conditions in which political budget cycles may be more or less likely to occur (De Haan and Klomp, 2013; Dubois, 2016; Eslava, 2011). This is typically captured by including an interaction term between the election indicator and a conditional variable. However, one complicating factor is that the t-value reported on the election effect is, in that case, not directly comparable with the others. To have the correct t-value, the standard error on the election variable should be computed conditional on the moderating variable. However, the covariance between the coefficient found on the election variable and the interaction term is largely unknown, and studies are not always clear on the average value of the moderating variable. As a result, I am unable to calculate the exact standard errors for most of the studies. Alternatively, I include a dummy variable in the specification, taking the value one when a regression includes an interaction term of the election indicator and a control variable, and zero otherwise. In this way, I partially control for the bias resulting from neglecting the precise standard errors.[3]

4.2.2. Econometric specification, sample, and estimation method

The next set of covariates controls for differences among studies related to the empirical specification chosen, countries and time periods covered, and the estimation method applied. First, the primary studies in my sample typically include a large set of control variables next to the election measure to avoid any omitted variable bias. Neglecting other determinants of the defense expenditures might increase the risk of inflating the election effect. Admittedly, the literature offers a large number of moderator variables that might potentially play a role in military spending regressions (see, for instance, Wezeman, 2014; Smith, 1995). In particular, studies commonly include explanatory variables related to the structural deployment of the armed forces, the role of national defense policies, and international security. In this respect, I have constructed a series of dummy variables to control for eleven commonly used control variables related to the economic environment, political conditions, and the security situation present in a country (see Table 3 for a detailed list). Additionally, I add the number of covariates included in the specification of a considered estimate as a control variable in the MRA to reduce any further an omitted variable bias. Furthermore, most studies include the lagged dependent variable as there exists a significant autoregressive tendency in the time series on defense spending. Defense expenditures are often tied to long-term obligations such as maintenance, personnel, and prevailing war costs (Yalta and Tüzün, 2021). Neglecting this issue would result in autocorrelation and the variances of coefficients will be biased. This will lead again to an underestimation of the error term.

Moreover, including more covariates usually creates a trade-off since the number of observations might drop due to data availability. This, in turn, is likely to reduce the country coverage in the sample. Thus, adding additional variables reduces omitted variable bias concerns, but might subsequently lead to a sample selection bias. In practice, high-income countries suffer less from these data problems as they have more data at their disposal that are readily available and of high quality. Nevertheless, the countries on which the estimation results are based might influence the existence and the strength of the empirical relationship between elections and defense spending. For instance, the economy of countries that have a large-scale defense industry will benefit much more from an election-motivated expansion of defense expenditures compared to countries that have only a small defense industry. In these latter countries, a large part of the additional spending will flow abroad and not stimulate the domestic economy. To control for all of these aspects, I add a number of variables that describe the country sample on which the reported estimate is based (see Table 3 for more details).

Furthermore, I control for the time periods covered in the studies. Since the end of the Cold War, defense spending has dropped significantly in most countries, on average by three-quarters. Additionally, I correct also for any sample selection by the author, for instance, by excluding outlier observations or by removing particular time periods or countries from

[2] From these studies, I include for reasons of comparison the coefficient on the pre-election year into the MRA dataset.

[3] Nevertheless, this methodological approach creates a certain measurement error that might effect the results. As a response, I have also run the MRA model without this variable. However, the main findings remain unaffected.

the sample. Authors might engage in sample selection to enhance the significance of their results or reveal any difference in impact among countries or time periods.

The next set of control variables in the MRA is related to the econometric technique applied. First, I include two dummy variables reflecting whether the author controls for respectively country or time-fixed effects. By using country-specific intercepts, studies control for time-invariant country-specific characteristics and place the emphasis of the regression on the identification of the within-country variation over time. Moreover, by including year dummies, authors try to control for country-invariant factors that affect the defense spending and are, for instance, related to international security shocks. Finally, I include a dummy capturing if a study controls for the endogeneity issues by estimating, for example, an instrumental variable (e.g., 2SLS) or a (system-)GMM model. Neglecting any possible endogeneity might bias the significance of the estimates.

4.2.3. Research objective, authors, and publication outlet

Finally, I control for differences in the objective of the research, author characteristics, and the publication outlet. First, I include a dummy reflecting whether a study is published in a book or journal, or as a working paper, respectively. To disaggregate the journal publications some further, I distinguish between publications in general economics journals and more public choice-oriented outlets on the one hand and articles published in field journals that are dedicated specifically to defense economics or military politics on the other.[4] Second, I include the publication year in the meta-regression analysis, allowing me to analyze differences over time in the t-statistics. Third, I add a variable whether the objective of the study is primarily to explore election cycles in military spending or, alternatively, if the main aim is to explain the general existence of PBCs in aggregate spending or in several different spending components, including military expenditures. Fourth, I control for some research team characteristics (i.e., country of origin, number of authors). Finally, the journal impact factor reported by Clarivate is added as a quality measure.[5]

4.2.4. Empirical results

Table 3 presents the definition of the control variables used in the meta-regression analysis based on the discussion above. All control variables are divided by the standard error of the election coefficient as given in Eq. (5). Table 4 presents the results of this heterogeneity analysis. Based on the findings, I can draw several conclusions. First, after controlling for the heterogeneity among studies and estimates, the genuine effect suddenly turns significantly negative. This finding suggests that governments, on average, reduce their defense expenditures when elections are upcoming. However, this effect is likely to be largely conditional as the results also show a significant difference between single-country studies that use time-series data and multiple countries studies based on a panel analysis. To be precise, country-specific studies commonly find a significant positive effect of elections on defense expenditures. Having a closer look at the particular countries covered in this latter set of studies, one can argue that these studies are mainly concerned with countries that either must deal with some serious security risks in the last decade (i.e., Greece, Israel) or have a major domestic defense industry (i.e., US). Both these factors are identified in the earlier literature to create some upward pressure on the military spending when elections are upcoming. In particular, in the case of dealing with severe security risks, voters are likely to assign a relatively higher value to military spending due to security concerns, thereby making their spending priorities become more aligned with those of politicians.

Moreover, also the existence of a major domestic defense industry, as is the case for the U.S., might shape the electoral incentives of the incumbent, as already argued above. In countries with a major defense industry, the national economy will gain the most from expanding defense spending for re-election purposes. At the same time, in these countries, the defense industry is likely to have the most political influence as it represents many votes. This creates an environment where votes are traded in return for economic benefits.

To shed some light on whether these ideas are supported by the data, I have run the meta-analysis again in Table 5, including two additional control variables. First, I include the share of countries in the sample of the primary study that have an economically meaningful defense industry. As already mentioned above, the majority of the revenues in the defense market is concentrated in only seven countries-the United States, the United Kingdom, Russia, China, France, Germany, and Italy. These countries are therefore recognized as having a major defense industry. Second, I add the share of countries in the sample of the primary study that have to deal with serious security threats. The classification of whether a country has a low or high-security risk is based on the median score on the conflict risk indicators reported in the International Country Risk Guide. The results presented in Table 5 confirm my expectations as the coefficient on the two additional variables are highly significant and compensates for the negative general election effect. This result strengthens the idea that the occurrence of election cycles in defense spending is, to a great extent, conditional on country-specific elements that shape the rent-seeking behavior of the incumbent. However, these latter results should be interpreted with caution as due to a lack of detailed information about the sample used in the primary studies, the number of observations in the MRA drops considerably.

[4] I have classified the following journals as specific field journals in defense economics and military politics: Journal of Peace Research, Defense and Peace Economics, Journal of Conflict Resolution, the Economics of Peace and Security Journal, Peace Economics, Peace Science and Public Policy and Armed Forces and Society.

[5] When an impact factor is lacking this variable is set equal to zero.

Table 3
Variables used in the MRA.

Dependent variable	
mil_tot	A dummy variable taking the value one when total military spending is used as a dependent variable, zero otherwise
mil_pc	A dummy variable taking the value one when military spending per capita is used as a dependent variable, zero otherwise
mil_GDP	A dummy variable taking the value one when military spending as a share of GDP is used as a dependent variable, zero otherwise
mil_gov	A dummy variable taking the value one when military spending as a share of government spending is used as a dependent variable, zero otherwise
mil_log	A dummy variable taking the value one when the dependent variable is taken in natural logarithm, zero otherwise
mil_sipri	A dummy variable taking the value one when the data about military spending is taken from SIPRI, zero otherwise
Election cycle measure	
elec_course	A dummy variable taking the value one when the election measure takes into account the course of a year, zero otherwise.
elec_dummy	A dummy variable taking the value one when an election year dummy is applied, zero otherwise.
elec_post	A dummy variable taking the value one when also a post-election year is coded instead, zero otherwise.
elec_interaction	A dummy variable taking the value one when also an interaction variable with the election variables is included in the specification, zero otherwise.
elec_budauth	A dummy variable taking the value one when the election variable controls for budgetary authority (i.e., presidential vs. parliamentary elections), zero otherwise.
elec_predeter	A dummy variable taking the value one when only predetermined elections are included, zero otherwise.
Empirical specification	
numcv	The number of control variables included in the regression.
lagdep	A dummy variable taking the value one when lagged dependent variable is included, zero otherwise.
cv_ecodev	A dummy variable taking the value one when controlled for the economic development, zero otherwise.
cv_sizeco	A dummy variable taking the value one when controlled for the economic size, zero otherwise.
cv_pop	A dummy variable taking the value one when controlled for population characteristics, zero otherwise.
cv_inst	A dummy variable taking the value one when controlled for institutional quality, zero otherwise.
cv_infl_exch	A dummy variable taking the value one when controlled for the price level or exchange rate, zero otherwise.
cv_polideo	A dummy variable taking the value one when controlled for the political ideology, zero otherwise.
cv_polecosys	A dummy variable taking the value one when controlled for the political and/or electoral system, zero otherwise.
cv_allian	A dummy variable taking the value one when controlled for alliance relationships, zero otherwise.
cv_glob	A dummy variable taking the value one when controlled for the economic integration, zero otherwise.
cv_secur	A dummy variable taking the value one when controlled for security risks, zero otherwise.
cv_mil	A dummy variable taking the value one when controlled for the influence of the military, zero otherwise.
Sample	
single	A dummy variable equal to one if the regression is based on a single country study, zero otherwise.
US	A dummy variable equal to one if the regression is based on only the US, zero otherwise.
numcount	The number of countries included in the sample.
numobs	The number of observations on which the estimation is based.
lowinc	A dummy variable equal to one if the sample includes only high-income countries based on the definition of the World Bank, zero otherwise.
autocr	A dummy variable equal to one if the sample includes autocratic countries or periods based on definition of the Polity IV, zero otherwise.
Y70	A dummy variable equal to one if data refer to the 1970s, zero otherwise.
Y80	A dummy variable equal to one if data refer to the 1980s, zero otherwise.
Y90	A dummy variable equal to one if data refer to the 1990s, zero otherwise.
Y00	A dummy variable equal to one if data refer to the 2000s, zero otherwise.
sampres	A dummy variable equal to one if an author restricts the total sample, for instance, to correct for outliers or perform a sample split, zero otherwise.
Estimation method	
endog	A dummy variable equal to one if the author controls for endogeneity by estimating, for instance, an IV or GMM model, zero otherwise.
fix_count	A dummy variable equal to one if fixed country effects are included, zero otherwise.
fix_time	A dummy variable equal to one if fixed time effects or a time trend is included, zero otherwise.
ercor	A dummy variable equal to one if the results are corrected for heteroskedasticity or autocorrelation, zero otherwise.
Publication characteristics	
wp	A dummy variable equal to one if the study is a working paper, zero otherwise.
defjour	A dummy variable equal to one if the study is published in a defence specific journal, zero otherwise.
pbc	A dummy variable equal to one if the objective of the study is only investigating election cycles in military expenditures, zero otherwise.
if	SSCI impact factor.
pubyear	Publication year (1979 = 1, 1980 = 2, 1981 = 3, etc.)
num_author	The size of the research team.
us_author	A dummy variable equal to one when at least one of the authors is affiliated with a US research institute.

It also appears that there is a difference between high and low-income countries. In most high-income countries, a negative election dominates as a large majority of these countries lack a major defense industry or do not suffer from security threats. Governments in these countries are likely to benefit more from expanding public spending in other

Table 4
Heterogeneity between estimates.

	Hierarchical linear model					Bayesian model averaging		
	Coefficient (1)		z-value (2)	Coefficient (3)		z-value (4)	Avg. Beta (5)	Avg. SE (6)	PiP (7)
Constant	−0.0851		−0.67	−0.0915		−1.05	−0.0907	NA	1.00
Inverse standard errors	−0.0037	*	−1.90	−0.0044	**	−2.41	−0.0076	0.005	0.95
Dependent variable and election cycle measure									
mil_tot	0.0015		0.92				0.0028	0.004	0.21
mil_GDP	−0.0014	*	−1.89	−0.0017	*	−1.94	−0.0014	0.001	0.92
mil_log	0.0027		1.06				0.0019	0.001	0.08
mil_sipri	0.0019		0.78				0.0046	0.004	0.38
elec_course	−0.0024	*	−1.73				−0.0042	0.002	0.92
elec_dummy	0.0018		1.05				0.0021	0.002	0.28
elec_post	0.0016		0.72				0.0026	0.004	0.29
elec_interaction	0.0026		1.51				0.0060	0.005	0.35
elec_budauth	−0.0008		−0.95				−0.0011	0.001	0.44
elec_predeter	0.0000		−1.41				0.0000	0.000	0.12
Empirical specification, sample, and estimation method									
numcv	0.0014		1.53				0.0011	0.001	0.35
lagdep	0.0012		0.84				0.0022	0.004	0.49
cv_ecodev	0.0012		1.57				0.0025	0.002	0.12
cv_sizeco	0.0019		1.38				0.0014	0.001	0.49
cv_pop	0.0025		0.90				0.0023	0.004	0.38
cv_inst	0.0026		1.42				0.0043	0.004	0.08
cv_infl_exch	0.0012		0.86				0.0010	0.001	0.50
cv_polideo	0.0011		0.67				0.0023	0.003	0.04
cv_polecosys	0.0018		0.89				0.0021	0.003	0.23
cv_allian	0.0022		0.70				0.0042	0.005	0.22
cv_glob	0.0015		1.54				0.0025	0.002	0.37
cv_secur	0.0013		1.39				0.0021	0.002	0.52
cv_mil	0.0024		1.17				0.0056	0.007	0.04
single	0.0073	**	2.70	0.0091	*	1.81	0.0116	0.003	0.91
US	0.0063	**	2.20	0.0081	*	1.66	0.0097	0.004	0.94
numcount	0.0030		0.92				0.0063	0.006	0.24
numobs	0.0012		0.65				0.0029	0.004	0.44
lowinc	0.0024	*	1.81	0.0031	*	1.93	0.0038	0.002	0.91
autocr	0.0069	*	1.77	0.0057	*	1.77	0.0101	0.005	0.92
Y70	0.0071	**	2.38	0.0121	**	2.25	0.0133	0.004	0.94
Y80	0.0069	*	1.82	0.0108	**	2.39	0.0116	0.007	0.91
Y90	−0.0025		−1.07				−0.0037	0.003	0.10
Y00	−0.0026	*	−1.89	−0.0039	*	−1.66	−0.0046	0.003	0.92
sampres	0.0016		1.09				0.0013	0.001	0.41
endog	0.0026		1.22				0.0042	0.003	0.34
fix_count	0.0015		1.30				0.0026	0.003	0.29
fix_time	0.0026		1.55				0.0021	0.002	0.26
ercor	0.0028		0.67				0.0049	0.005	0.39
Publication characteristics									
wp	0.0027		1.02				0.0035	0.004	0.41
defjour	0.0022		0.86				0.0041	0.003	0.40
pubyear	0.0019		1.15				0.0032	0.003	0.24
pbc	0.0012		0.89				0.0029	0.003	0.49
if	0.0028		0.70				0.0066	0.010	0.39
num_author	−0.0015		−1.20				−0.0034	0.003	0.31
us_author	0.0026		1.54				0.0062	0.008	0.47
Intra-class correlation	0.296			0.342					
Number of observations	185			185				185	
Maximum Likelihood ratio p-value	0.000			0.000					
R-squared	0.295			0.258					

Note: The columns 'Avg. Beta' and "Avg. SE" report the unweighted average coefficient and standard error, respectively. 'PiP' is the posterior inclusion probability. **/* indicates significance at respectively 5 and 10 percent level.

domains at the expense of defense spending since voters are likely to prefer welfare spending instead. In contrast, the negative election effect is moderated, or may even turn positive, in low-income countries as the dummy that controls for the fact when a sample also includes low-income countries is significantly positive and largely compensates for the initial negative effect. One explanation is that developing countries traditionally suffer more from security issues and are more often associated with autocratic regimes.

Table 5
Defense industry and security risk.

	Coefficient (1)		z-value (2)
Constant	−0.0824		−1.52
Inverse standard errors	−0.0047	*	−1.81
Single	0.0016		1.29
US	0.0021	*	1.74
Share major defense industry	0.0709	**	1.98
Share security risk	0.0683	**	2.27
Intra-class correlation	0.307		
Number of observations	109		
Maximum Likelihood ratio p-value	0.000		
R-squared	0.257		

Note: **/* indicates significance at respectively 5 and 10 percent level.

To control this latter issue, I have also included a dummy taking the value one when the sample includes autocratic countries or periods. The results indicate that studies that also include autocratic countries find a more positive *t*-value. In these particular countries, the government might be more inclined to increase military spending in an election year for three reasons. First, to send a message of deterrence to challengers by signaling the military capabilities. Second, autocratic countries, in particular, suffer from national security threats such as violent conflict or terroristic attacks. As already argued above, the incumbent might increase defense spending to signal its competence in dealing with these situations. Third, to express their loyalty to the ruling nonelected elite, such as the military.

Additionally, the significance pattern on the series of time dummies indicates that there is a clear declining time trend in the genuine effect. In particular, the election effect is weakly positive during the Cold War period, but turns significantly negative after the end of the Cold War at the beginning of the 1990s. This strengthens the idea that the security perceptions of voters are important as they will affect their preferences for military expenditures. This, again, will affect the likelihood, as well as, the direction of election-induced manipulations of the defense budget by the ruling parties. As already argued above, more expansionary military spending policies are used in an insecure environment by the incumbent to signal its competence.

Moreover, studies using defense expenditures as a share of the total government expenditures as their dependent variable typically find a lower t-statistic. The explanation for this finding possibly originated from the way of constructing this variable. The general PBC theory already assumes that governments follow an expansionary fiscal policy during an election year to improve their odds of being re-elected. So, when the relative positive change in military spending is smaller than the relative positive change in total government spending or if the defense spending drops when elections take place shortly, a negative association exists between elections and military spending as the ratio between military expenditures and total government spending falls. Also, studies taking into account the course of a year when an election is held find a lower t-statistic.

Finally, the results indicate that the objective of a study or the publication outlet does not affect the existence of a genuine effect or publication bias. None of the included variables is significantly related to the *t*-value of the election effect. This strengthens my previous findings that there is no publication bias since there is no difference between published and unpublished studies.

To assess the robustness of the findings, I apply the general-to-specific approach as suggested by Hendry (2000). This method entails dropping the least significant variable from the total model and estimating the model again. This procedure is repeated until only variables that are significant at a ten percent level remain. The final results of this approach are presented in columns (3)–(4) of Table 4. The findings indicate that almost the same variables turn out to be significant as in the previous column, including all control variables. Only the significance of the dummy controlling whether researchers use an election variable that takes into account the course of a year appears to change from significant to insignificant.

4.3. Bayesian model average analysis

The central difficulty in empirical research is that several different models may all seem reasonable and plausible given the theory and data, but yield different conclusions about the parameters of interest as they sometimes differ substantially in their significance or even in direction. For this purpose, I employ a so-called Bayesian Model Averaging (BMA) that is widely used in MRA studies to test the robustness and validity of earlier findings. In essence, the BMA method involves the estimation of the distribution of unknown parameters by using various combinations of control variables to test whether the variables of interest are significantly and robustly related with explaining differences in the reported findings across studies and estimates (see Havranek and Irsova, 2017; Philips, 2016; Próchniak and Witkowski, 2013; Polák, 2019; Iršová and Havránek, 2013; Duan et al., 2020). Variables that matter in one empirical model may not be statistically significant in another specification due to the presence of other control variables. Thus, I do not merely check the significance of a variable's coefficient from one specific specification—instead, I keep track of its coefficient in all possible specifications

with additional control variables and focus on the probability distribution over the space of possible models. To evaluate the posterior model probability, the BMA uses the Bayesian Information Criterion to approximate the Bayes factors that are needed to compute the posterior model probability.

The statistical framework includes two sets of explanatory variables. First, a set of so-called focus regressors that are included in every model, in my case, the inverse standard errors to test for a genuine effect. Second, the vector of auxiliary regressors explains the observed variation in the election effect found across studies and estimates. BMA addresses model uncertainty related to the choice of the auxiliary regressors by estimating models for all possible combinations and taking a weighted average over all models. It attaches prior probabilities to the different models and averages them based on derived posterior probabilities. More formally, the probability that model j, M_j, is the "true" model given the data y, i.e., the posterior model distribution given a prior model probability, is defined as

$$p\left(M_j|y\right) = \frac{p\left(y|M_j\right)p\left(M_j\right)}{\sum_{i=1}^{2^k} p\left(y|M_j\right)} \tag{6}$$

where $P(y|M_j)$ is the marginal likelihood of model M_j given data y, and $p(M_j)$ is the prior model probability. The weight for a given model is normalized by the sum of the weights of all models, represented in the denominator in Eq. (6).

In the final two columns of Table 4, I present the BMA estimation results. The commonly used cut-off point for a variable to be considered robustly linked to the dependent variable is a PiP value of 0.9 or higher. The findings largely confirm the previous results. Again the genuine election effect turns out to be significantly negative with a PiP close to one. Moreover, the variables that explain the heterogeneity in the election effect that have been found significant in the HLM estimation typically have a high post-prior inclusion probability.

4.4. Competing hypotheses

The major difficulty within this particular meta-analysis is that it tests two competing hypotheses. However, there is a possibility that both hypotheses should be accepted. However, based on the analyses performed so far, I can only conclude if one hypothesis dominates the other. In particular, studies that are primarily based on a sample of countries that increase their defense spending when elections are upcoming are likely to report a genuinely positive effect, while subsequent studies that use a sample of contractionary countries probably find the opposite. In the previous analysis, I have tried to disentangle this heterogeneity by controlling for differences in sample, model specification, and data. However, it is possible that the variation in reported results in the different studies comes is attributed to unobserved variables, or perhaps the identifying variable is simply missing. In such a case, a finite mixture model (FMM) might resolve some of these concerns. This particular estimation technique models the probability of belonging to each unobserved group, estimates distinct parameters of a regression model or distribution in each group of studies, classifies individual studies into groups, and draws inferences about how each group of studies behaves. In this method, studies are endogenously classified with concomitant variables instead of ad hoc selection based on exogenous thresholds. The differences in parameters in the various groups allow me to observe the distinct genuine effect of elections on defense expenditures. The first part of Table 6 reports the two group estimations results. In particular, the average genuine election effect in group one, measured by the reported t-value, is about -1.82 in group 1 and around 1.01 for group 2. Based on these simple average, one can argue that group 1 consist mainly of studies that report a negative correlation, and group 2 includes, for a large part, studies with a positive relationship between elections and defense expenditures. The findings reported in columns (1) and (2) indicate that there is a significant negative election effect present, but no positive significant genuine effect. Only in countries with a large defense industry or states that suffer from a security risk might there be a positive effect of elections on military spending.

In a similar fashion, in the final columns of Table 6, I estimate a multinomial logit model to see which control variables are significantly related to an expansionary or contractionary defense spending policy in an election year and to test if these are the same determinants. To do so, the test statistics obtained from primary studies are split into three groups: negative significant, insignificant, and positive significant observations. The distinction is made by using the critical value of the test statistic distribution at the ninety percent significance level threshold. The multinomial logit results again confirm my earlier conclusions. For instance, country-specific studies have a higher probability of finding a significant positive election effect, while they have a lower likelihood of reporting a significant negative effect. Also, studies focussing only on the U.S. find a significantly higher likelihood of reporting a significant positive election effect.

Based on the results from the general-to-specific approach, the BMA analysis and the Finite Mixture Model it appears that the MRA results do not rely so much on the empirical specification chosen and are robust to the estimation technique applied.

5. Conclusion and discussion

During the last decades, many political and economics scholars have explored the role of upcoming general elections in the observed variation in military expenditures between countries and across years. However, the general picture that emerges from the empirical evidence is somewhat inconclusive. To shed some light on this relationship and come up with a more comprehensive overview, I apply the meta-analysis regression methodology. In the analysis, I first split the

Table 6
Competing hypotheses.

	Mixture finite model				Multinominal logit model			
	Group 1 (1)	z-value (2)	Group 2 (3)	z-value (4)	Coefficient + (5)	z-value (6)	Coefficient − (7)	z-value (8)
Constant	−0.0808	−0.75	0.0646	1.47	0.0829	1.36	0.0609	0.76
Inverse standard errors	−0.0019 *	−1.88	0.0027	1.35	0.0866 *	1.90	0.0745 *	1.93
Dependent variable and election cycle measure								
mil_tot	0.0007	0.83	0.0013	1.51	0.0941	1.08	0.0786	0.81
mil_GDP	−0.0005 *	−1.67	−0.0013 *	−1.72	0.0832	1.51	0.0825	1.00
mil_log	0.0016	1.41	0.0024	1.13	0.0527	0.65	0.0968	1.40
mil_sipri	0.0008	1.57	0.0017	1.40	0.0606	1.11	0.0534	1.26
elec_course	−0.0013	−1.29	−0.0022	−0.87	0.0915	1.19	0.0941	1.26
elec_dummy	0.0009	1.33	0.0012	1.00	0.0632	1.03	0.0905	1.52
elec_post	0.0015	1.52	0.0012	1.47	0.0884	1.08	0.0830	0.77
elec_interaction	0.0027	0.78	0.0019	1.54	0.0972	0.83	0.0745	0.63
elec_budauth	−0.0009	−1.32	−0.0008	−1.48	0.0698	0.82	0.0909	1.10
elec_predeter	0.0000	−1.27	0.0000	−1.05	0.0562	0.69	0.0903	1.45
Empirical specification, sample, and estimation method								
numcv	0.0012	0.61	0.0007	1.15	0.0782	1.08	0.0691	1.12
lagdep	0.0011	0.81	0.0004	0.63	0.0729	1.49	0.0805	0.85
cv_ecodev	0.0011	1.21	0.0004	0.92	0.0838	1.04	0.0901	1.33
cv_sizeco	0.0007	0.74	0.0009	0.62	0.0969	1.14	0.0955	1.52
cv_pop	0.0017	1.16	0.0009	0.83	0.0793	1.44	0.0835	1.32
cv_inst	0.0010	0.77	0.0016	1.05	0.0839	0.76	0.0730	0.69
cv_infl_exch	0.0006	1.56	0.0008	0.72	0.0729	1.59	0.0683	0.92
cv_polideo	0.0009	1.15	0.0005	0.70	0.0755	1.51	0.0953	1.48
cv_polecosys	0.0006	1.04	0.0013	0.88	0.0693	0.86	0.0741	1.31
cv_allian	0.0018	1.24	0.0010	1.58	0.0568	1.58	0.0585	0.90
cv_glob	0.0015	1.27	0.0005	1.31	0.0562	1.39	0.0528	1.08
cv_secur	0.0009	0.82	0.0008	1.35	0.0615	1.44	0.0550	1.31
cv_mil	0.0012	0.95	0.0026	1.60	0.0952	0.79	0.0726	0.94
single	0.0025	1.46	0.0062 **	2.74	0.0571 **	2.73	−0.0820 *	1.95
US	0.0061	1.23	0.0024 **	2.06	0.0740 *	1.66	0.0926	1.27
numcount	0.0020 *	1.89	0.0014	0.79	0.0694	1.25	0.0848	1.20
numobs	0.0005	0.97	0.0005	1.21	0.0847	1.02	0.0709	0.96
lowinc	0.0013	1.02	0.0021	0.72	0.0551	1.32	−0.0540 **	2.79
autocr	0.0041	1.21	0.0036	0.62	0.0762	1.57	−0.0835 **	2.81
Y70	0.0047 **	2.92	0.0035	0.73	0.0925 *	1.83	−0.0514 *	1.78
Y80	0.0073 **	2.51	0.0047	1.41	0.0576	0.92	0.0722	1.54
Y90	−0.0019	−1.27	−0.0014	−1.33	0.0145 **	2.63	0.0693	0.71
Y00	−0.0013 *	−1.79	−0.0013 *	−1.86	−0.0715 **	−2.96	0.0705 *	1.80
sampres	0.0009	1.51	0.0015	0.92	0.0819	1.05	0.0862	1.01
endog	0.0017	0.83	0.0024	0.73	0.0667	1.58	0.0954	0.70
fix_count	0.0010	1.23	0.0012	1.59	0.0960	0.77	0.0508	0.83
fix_time	0.0014	0.96	0.0009	1.42	0.0890	0.79	0.0511	1.26
ercor	0.0014	1.32	0.0028	0.81	0.0723	1.30	0.0744	0.66
Publication characteristics								
wp	0.0019	1.51	0.0019	1.13	0.0592	1.15	0.0847	1.37
defjour	0.0007	0.92	0.0008	1.22	0.0877	1.51	0.0533	1.51
pubyear	0.0014	0.77	0.0008	1.25	0.0847	0.91	0.0868	1.16
pbc	0.0010	1.28	0.0012	1.19	0.0502	1.35	0.0854	0.94
if	0.0029	1.23	0.0011	1.25	0.0982	1.06	0.0594	1.11
num_author	−0.0009	−1.19	−0.0013	−0.92	0.0707	1.25	0.0905	0.80
us_author	0.0025	0.84	0.0019	1.57	0.0623	0.66	0.0791	1.60
Share of observations	63%		37%					
Number of observations	185				185			
Maximum Likelihood ratio p-value	0.0000				0.000			
R-squared					0.127			

Note: . **/* indicates significance at respectively 5 and 10 percent level.

election effect into the 'true effect' and the 'publication bias effect'. Moreover, I have examined whether study design, data construction, and differences regarding country samples, time periods, and estimation methods may explain the observed differences in the results found within and between studies.

However, one difficulty in this particular meta-analysis is that it tests two competing hypotheses—a contraction hypothesis versus an expansion hypothesis. By using about two hundred election estimates, the main results of the

analysis can be summarized as follows. First, there is a weak negative genuine effect of elections on military expenditures. This finding suggests that, on average, governments follow a more contractionary policy in defense spending when elections are approaching. However, this impact is likely to rely to a great extent on the country sample used and the time periods covered in the study. It appears that the relationship between elections and military spending is likely to be positive in countries where the defense industry has some considerable political influence and economic power. Also, in countries that have to deal with security risks, a positive relationship is likely to prevail. In contrast, in most other countries, this relationship is expected to be negative or insignificant. Second, in the Cold War period, the election effect is positive, while in the post-Cold War period, this effect turns out to be negative as voters have largely revised their peace threat perception between these two periods and attach a lower weight to military spending in the latter period. Finally, I find any evidence that a publication bias influences the election effect found among the considered studies. This is partly explained by the fact that both positive, as well as, negative results are theoretically plausible and apparently equally likely to be published.

Based on these findings, one can argue that I have found some support for both hypotheses that are being tested in this literature. This would indicate that military spending is not only driven by security or strategic considerations that should improve national security, but also for political rent-seeking. This, again, could have some negative consequences for national security and should therefore be avoided. One way is by establishing more formal budgetary rules on military spending that leave less room for the discretion of the policy maker. A suggestion for future research on this topic would be to focus much more on the conditional impact of elections by revealing the political, strategic, and economic mechanisms underlying these political cycles. This will give us a better opportunity to answer the question of why some countries follow a contractionary defense policy when elections are upcoming and others a more expansionary policy, and especially why, and develop an effective legislative framework to prevent any short-term political use.

However, one main limitation is the little variation in the data sources used on military expenditure. This could makes that when there is a selection bias present during the process of collecting spending information, this cannot be traced within the current meta-analysis. For instance, there is no clear consensus which spending items should be included into the defense expenditure. Not all forms of defense spending are susceptible to electoral manipulation. Some items are more amenable to manipulation than others, while some other major parts are again altogether uncontrollable or are subject to varying degrees of timing discretion. Budget authority, obligations, and outlays are usually not interchangeable, nor are military pay and procurement accounts. Similar, personnel expenditures and retirement pensions are relatively uncontrollable. These concerns could affect the reliability of the MRA results.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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