MANIPULATING THE MASSES: NEW THEORIES OF POLITICAL CYCLES

A Dissertation

by

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ABSTRACT

Despite a large literature on political cycles, many theories and empirical results conflict with one another. I address this disconnect through three interrelated contributions. I first conduct an extensive quantitative survey of the political budget cycle literature through a meta-analysis. I find that overall there exists a positive, though substantively small political budget cycle effect. Second, I examine how incumbents may use alternatives to fiscal manipulation, such as the passage of redistributive policies, since these send a key signal to voters. Third, I examine how incumbents may not only time fiscal manipulation, but control their placement spatially. This ties in the political budget cycle literature with the literature on distributive politics. Although these findings call into question some of the existing views of political budget cycles, they show that cycles manifest themselves in alternative fashions.

DEDICATION

This dissertation is dedicated to Meghan Winget, who has always supported me in my endeavors.

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Contributors

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1. INTRODUCTION

> "*Incumbent politicians know that if the economy is doing well, they are much more likely to be re-elected. When wages are rising and jobs are plentiful, workers feel happy. Small wonder, then, that many governments attempt to manipulate the economy to boost their political fortunes*." —The Economist[1]

In stable democracies, one of the greatest fears for a politician is the next election.[2] Although they may gain some satisfaction in knowing that their ideological core will most likely continue to support them, voters at the margins are of particular concern. How does an incumbent gain the support of swing voters needed to ensure re-election, while at the same time satisfying the needs of their core constituents?

To answer this question, it helps to ask what voters evaluate the incumbent on. Typically, a relative comparison is made in the eyes of voters, "am I better off now than I was in the past?"[3] These evaluations are based off many factors, such as the incumbent's success at handling the economy, their ability to pass social policies, and how well they conduct foreign policy. While the incumbent may be naturally gifted at addressing these issues, there may also be an incentive for an incumbent to tip the scales in their favor through deliberate fiscal and monetary changes around elections. Such behavior has been termed *political cycles*. First formalized by Nordhaus (1975),

[1] "String-pushers," http://www.economist.com/news/finance-and-economics/21648620-politicians-often-try-manipulate-economy-win-votes-seldom. Accessed 01/17/2017.

[2] This is, of course, assuming that they desire to be re-elected, or are able to run again given constitutional term limits set in some countries.

[3] This statement forms the central point of the literature on economic voting, which holds that current and previous economic conditions are the largest driver of electoral outcomes (Fair 1978; Kramer 1983; Powell Jr and Whitten 1993; Lewis-Beck and Stegmaier 2013). In contrast to the common retrospective approach, prospective theories posit that voters estimate their future well-being, given their vote choice. Note also that voters do not always think of themselves when evaluating the incumbent, in what has become known as pocketbook voting. Instead, they will look at the well-being of those around them, also known as sociotropic voting.

this literature has grown to cover a diverse body of topics, including the manipulation of monetary, fiscal, and other policies over the past 40 years.[4]

What differentiates political cycles from true desire to tilt policy in a certain direction? Time. The time in which elections occur changes the decision-making calculus of the incumbent. All else equal, manipulating closer to the election will result in larger electoral returns, since individuals tend to discount past events (as well as future ones) more than the present. Thus, in all studies of political cycles, the election is the key independent variable that is theorized to drive changes in policy.

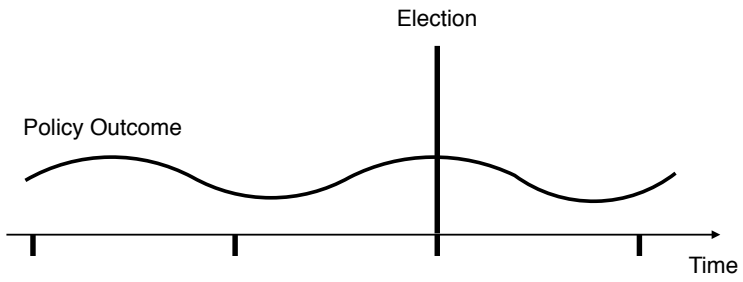
An example of political cycles is shown in Figure 1.1. In Figure 1.1a, policy tools naturally cycle up and down with the economy. For instance, increases in expenditures may coincide with economic downturns, or interest rates might align with economic output. Such policy tools can be classified into three broad categories. The first consists of monetary policy instruments and macroeconomic outcomes. These include manipulating the money supply, interest rates, changes in price levels and unemployment rates (Clark and Hallerberg 2000; Wibbels 2000; Heckelman 2001; Hallerberg, de Souza and Clark 2002; Erlandsson 2004; Ferré and Manzano 2014). The second—and probably the largest body of literature on political cycles—focuses on fiscal tools used to win re-election. For instance, taxes may fall during elections (Schuknecht 2000), while debt, fiscal deficits, and intergovernmental grants may rise (John and Ward 2001; Block 2002; O'Mahony 2011). Last, alternative policies that are neither explicitly fiscal or monetary in nature might be used.[5] For example, incumbents might adjust active labor market policies (Mechtel and Potrafke 2013), start conflicts with other nations (Hess and Orphanides 1995), or employ more public sector workers such as teachers around elections (Coelho, Veiga and Veiga 2006; Tepe and Vanhuysse 2009).

Regardless of the policy tool used, note that during an election in Figure 1.1a, the policy remains "unguided" by the incumbent. However, with the knowledge that policy tools—such as

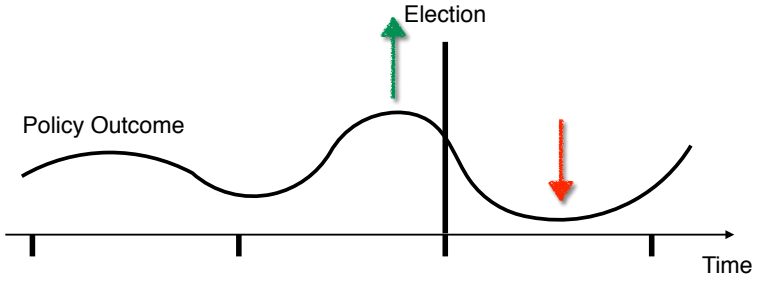
[4]Below, I refer to *political budget cycles* when discussing fiscal manipulation, *political business cycles* when covering monetary manipulation, and use the term *political cycles* to encompass the two (as well as any other alternative form of manipulation).

[5]This is a much smaller, yet growing, body of literature.

Figure 1.1: Using policies as opportunistic tools around elections



(a) No manipulation



(b) Pre-electoral manipulation

expenditures or fiscal deficits—appeal to voters, the incumbent may instead try to increase the policy tool just before an election by explicit manipulation, raising deficit spending or decreasing revenues, for instance. This increase is shown in Figure 1.1b. Such a policy might be popular with voters, perhaps even enough to ensure the incumbent is re-elected. However, most policies themselves are cyclical, and, after rising, must fall. For instance, growing deficits eventually have to be met with either a corresponding increase in revenues or decrease in expenditures. Such post-election busts may be acceptable for the incumbent, since it comes after the election. However, it is still early enough in the term so voters typically forget this downturn by the time the next election comes around. Even so, the incumbent may still face the same pressures to manipulate during the next election, and so the cycle continues.

1.1 A short history

Theories about political cycles have become more sophisticated over time. This progression of research can be best organized into four groups, as shown in Table 1.1. The earliest work focused on the manipulation of monetary policy, specifically the trade-off between inflation and unemployment (Nordhaus 1975).[6] Easy monetary policy—low interest rates set by the central bank—tends to boost nominal output, which has the effect of lowering unemployment towards its natural rate. Unemployment is one aggregate indicator thought to enter into the minds of voters when evaluating the incumbent. In addition, rising nominal wages reward individual voters. Such monetary policy behavior is not benign however; according to the Phillips curve, such expansionary monetary policy precedes rising price levels. Yet if an incumbent could time loose monetary policy to occur just before the election, they would be able to hold off on contracting policy (thus lowering inflation at the cost of rising unemployment) until after the election.[7]

While some empirical studies find evidence in support of the expectations-augmented Phillips curve hypothesis, others find little to no evidence. For instance, Allen (1986) finds that the US

[6] As discussed by Dubois (2016), some speculation about a political business cycle existed before Nordhaus (Åkerman 1947; Downs 1957). However, these were either not empirical, or they did not make explicit the Phillips curve tradeoff that incumbents take advantage of.

[7] This would also have the benefit of altering inflation expectations early on in the incumbent's term.

Table 1.1: Four general movements in the study of political cycles

	Expectations-Augmented Phillips Curve	**Partisan Cycles**	**Rational-PCs/Competency Models**	**Conditional Political Cycles**
Characterized by	Pre-election booms and post-election busts	Heterogeneous Partisan Preferences	Informational asymmetry between incumbent and public	Contexts make political cycles more or less likely
Examples	Nordhaus (1975)	Hibbs (1977)	Alesina (1987); Rogoff and Sibert (1988)	Alt and Lassen (2006*b*); Brender and Drazen (2005)
		Time $\Longrightarrow$		

Federal Reserve provides more accommodating monetary policy around both presidential and congressional elections (see also Haynes and Stone 1989). In contrast, Beck (1982) finds no support of the hypothesis when performing an intervention analysis on monthly US data from 1961 to 1973.

A number of critiques of the exepctations-augmented Phillips curve cycle theory are easy to make, especially in the US case. First, presidential term limits may disincentivize incumbents from pursuing more than one expansionary re-election phase. Although strong party cohesion may allow party after party member to win elections using the same strategy, overall, it seems unlikely that parties could continuously overcome such a coordination problem.

Second, it assumes that Democratic and Republican parties both desire lowering unemployment at the expense of rising prices. While such an assumption seems valid for the Democratic party, Republicans have historically been elected on "tight-money" policies due to their support base, their most outspoken members consisting of finance and business interests from the East coast. Especially outside the US context, it is easy to see that political parties differ substantially in terms of their constituent base and disposition towards worker's interests at the cost of inflation.

Third, it assumes that voters can be tricked into thinking the economy is performing well, when in reality it is simply an expansionary monetary policy that is creating the boom. While

some voters may be fooled, others, particularly sophisticated voters or "issue" voters who prioritize low unemployment or stable price levels (Carmines and Stimson 1980; Gomez and Wilson 2001) may be much harder to convince. In addition, while an incumbent may get away with such a manipulation strategy once, the resulting post-election inflation would make such a strategy less likely to work again.[8]

Last, early theories assumed a single actor (the president) as responsible for policy changes. In reality, there are multiple "hands on the wheel" when steering monetary policy; these institutional constraints may make sudden shifts in policy around elections less likely. Indeed, Franzese (1999) finds that the balance between politicians controlling inflationary outcomes and the central bank is a function of the degree of institutional autonomy the central bank has. Thus, pre-electoral monetary policy shifts might only occur in countries where central bank independence is low.

In light of the second critique above, a variant of the Nordhaus theory was developed to relax the assumption that all incumbents behave homogeneously. As shown in Table 1.1, this is generally known as the partisan cycle hypothesis, and is largely attributed to Hibbs (1977). While still focusing on the short-run unemployment-inflation trade-off, Hibbs relaxed the assumption that all incumbents have homogenous preferences. Instead, parties who are left-leaning ideologically will tend to reward labor interests, while parties of the right—who prioritize stable prices—might instead pursue the opposite strategy.[9] Indeed, as Hibbs showed through a simple correlation of 12 advanced industrialized democracies averaged over 1945 and 1969 (p. 1473), the correlation between the percentage of time a socialist-labor party was in power and the average inflation rate is 0.74. It should also be noted that the partisan cycle hypothesis still emphasizes the importance of time around the election; in order to please their ideological support base, incumbents will time their behavior of certain policies.

Even with the advances that the partisan cycle theory provides, many of the assumptions that underly theories of political business cycles still suffer from two of the critiques given above. Could

[8] Much of this would depend on how much voters discount the high inflation period at the start of the incumbent's term.

[9] I speak of left and right ideologies in the North American/European sense. Parties in regions such as Asia and Latin America are often harder to place on a traditional left-right two-dimensional ideological spectrum.

voters really be fooled—repeatedly—into thinking that the economy was performing better than it truly was before an election? In addition, are incumbents able to exert that much authority over fiscal and monetary policy?

In reality, neither question posed above could be answered by either the expectations-augmented Phillips curve or partisan cycle theories. Much of that changed with the move in economics and political science towards rational expectations. A number of papers adapted the rational expectations framework to work on political cycles (Alesina 1987; Rogoff and Sibert 1988; Rogoff 1990; Persson and Tabellini 1990). This is shown by the third column in Table 1.1. These approaches posit that actors behave in a rational, utility maximizing framework. In terms of political cycles, all else equal, voters prefer a competent to an incompetent leader (i.e., they prefer an individual that can manage the economy). Voters are treated as informed, and not easily fooled by the incumbent manipulating indiscriminately. However, due to some information asymmetries, an incompetent incumbent may be able to still manipulate policy so as to render themselves indistinguishable from its counterpart, thus creating a signaling game. Although voters would like to punish the incompetent incumbent, manipulation is hard to distinguish from true competency. Thus, the literature on political cycles was far from over.

Rational business cycle theory altered many of the previous findings. While monetary policy remained an important area of research, studies of fiscal policy gained in popularity, due to the fact that in many of the rational political cycle models, voters are able to fully anticipate inflation rates, rendering political business cycles impotent (Dubois 2016). Therefore, starting with works by Rogoff and Sibert (1988) and Rogoff (1990)—among others—scholarly focus shifted to government spending (and revenues) around elections, and especially the conditions under which this was more or less likely to occur. This has become known as the study of political budget cycles.

However, even rational expectations models were not without weaknesses. In reality, governments do not function as one cohesive unit. Government agencies may have at times coalescing and diverging interests to elected officials. Moreover, rules and institutions, such as constitutions or the level of fiscal policy transparency, may vastly restrict the magnitude of political cycles, or at

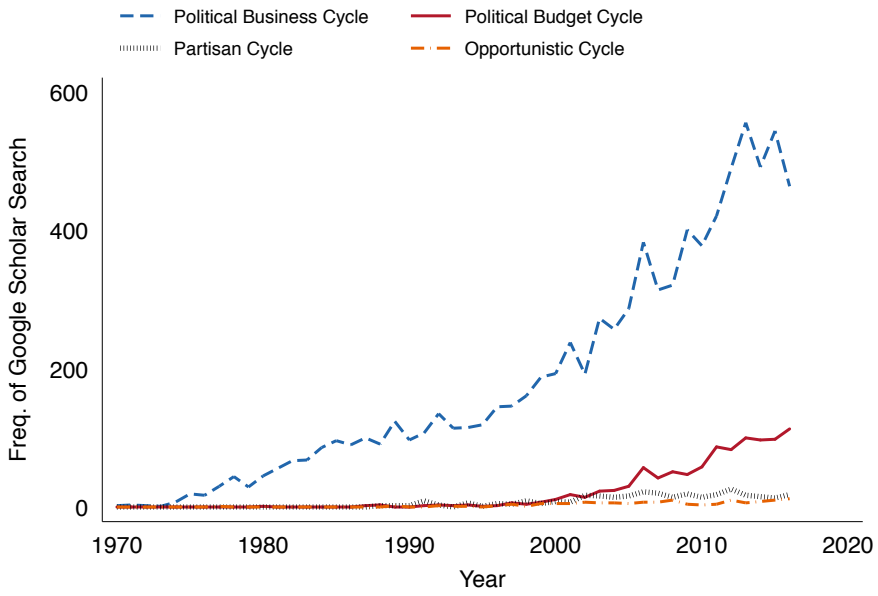
least constrain policy-makers to use alternative tools for manipulation. Such contextualization, or "conditional political cycles," makes up the fourth movement in Table 1.1, and is the best way to classify the current state of the literature.

The over-time popularity of different types of political cycles can be visualized in Figure 1.2, which shows the frequency of Google Scholar search results, by year, using the following search terms: "Political Business Cycle", "Political Budget Cycle", "Partisan Cycle", and "Opportunistic Cycle". First, it is clear that, even using a relatively simple search strategy, papers mentioning political business cycles are the most common in the literature, by far. Much of this can be attributed to the fact that political business cycles often focus on both monetary and fiscal policy, the latter of which could be better defined under the political budget cycle label.[10] Second, the frequency of papers mentioning political budget cycles, partisan cycles, and opportunistic cycles are relatively similar (and uncommon), until around 2000, when papers discussing political budget cycles sharply increase, while the other two remain flat. Last, papers discussing either political budget cycles or political business cycles experienced an incredible boom over the past 16 years, although the latter remains the dominant term used in the literature. From 2000 to 2016, the number of papers mentioning political business cycles grew by over 140 percent. During the same time period, the number of papers mentioning political budget cycles grew by nearly 930 percent.

The newest strain of literature on political cycles has two important characteristics. First, while much of the early literature on political cycles offered empirical tests in the United States and other Western democracies, there has been a recent move towards testing theoretical expectations in developing countries (e.g., Sáez and Sinha 2010; Sakurai and Menezes-Filho 2011; Sjahrir, Kis-Katos and Schulze 2013). There has also been a new focus on testing for political budget cycles at different levels of government, such as the national (Katsimi and Sarantides 2012), provincial

[10]The terminology used in articles is often vague, with "political business cycles" referring to any number of fiscal and monetary policies.

Figure 1.2: The rise of the political cycle literature



*N*otes: Frequency of Google Scholar searches (conducted December 26, 2016) for the terms above, in quotation marks. Patents and citations excluded.

(Remmer 2007; Padovano 2012), or municipal level (Veiga and Pinho 2007; Drazen and Eslava 2010*a*; Sakurai and Menezes-Filho 2011).[11]

The second important characteristic is the growing focus on the contexts under which political cycles might be more or less likely to occur. For instance, institutions have been shown to shape a variety of social and economic outcomes (e.g., North 1990). They also shape the magnitude of political cycles, or indeed whether they take place at all, since they often hold executive behavior in check. For instance, fiscal transparency (Alt and Lassen 2006*b*), fiscal rules (Rose 2006), and membership in a currency union (Efthyvoulou 2011) all hinder the magnitude of political cycles. In contrast, some—though certainly not all—developing countries suffer from a lack of independent institutions, which tends to weaken the ability to prevent manipulation around elections. This means that strong executives face few constraints on their ability to co-opt branches of government. Other factors include the level of political sophistication of voters (Shi and Svensson 2006), or the age of political parties (Hanusch and Keefer 2014).

1.2 A unified theory of political cycles

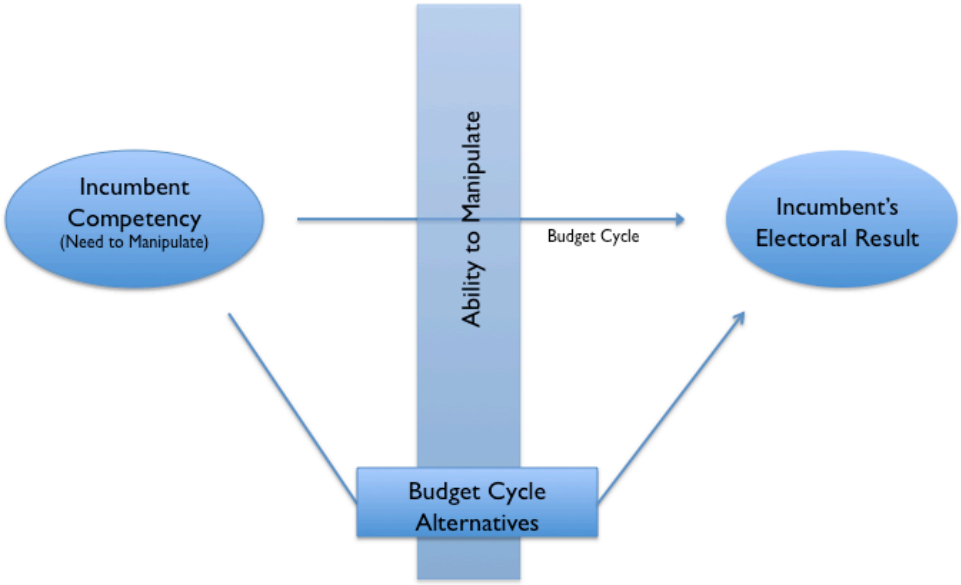
The theory of political cycles can be summarized in a stylized depiction, as shown in Figure 1.3. Moving from left to right, the first step is establishing whether the incumbent needs to manipulate in the first place. If the current political environment is unfavorable, the incumbent may pursue one of two options. First, they may alter the budget in order to win the support of voters, perhaps increasing expenditures or running fiscal deficits. Alternatively, they could pursue budget cycle alternatives, such as extending favorable credit to individuals (Cole 2009), boosting public sector employment (Tepe and Vanhuysse 2014), adjusting monetary policy, or instigating conflict in order to create a "rally around the flag" effect (Hess and Orphanides 1995; Oneal and Bryan 1995).[12]

Even if an incumbent has decided on a tool through which they will manipulate, this does not always translate into an observable political cycle. As discussed above, there are conditional

[11] Although political budget cycles have been observed at all levels of government, to the best of my knowledge there has been no overarching theory as to why these magnitudes may be larger for some elections rather than others. I discuss this more in the conclusion chapter.

[12] Of course, budget cycles and alternatives to such cycles are not mutually exclusive. Depending on the incumbent's competency, any and all methods may be pursued in order to win voter support (Franzese 2002).

Figure 1.3: Political cycles: A theoretical diagram



factors which may make certain tools of manipulation more or less effective, or even whether the incumbent may utilize them at all (e.g., an independent central bank severs the ability of the incumbent to pursue manipulative monetary policy). A number of factors condition the extent and the effectiveness of political cycles. This is shown as the "Ability to Manipulate" in Figure 1.3. These may include the level of institutional independence and capacity of institutions. Others factors include budgetary inertia; an increase in the budget on paper may take some time to trickle down to voters, or the incumbent may face difficulty making radical changes to the budget.

The last step of the theoretical process is shown on the right-hand side of Figure 1.3. Voters incorporate the benefits and subtract the costs of political cycles from their valuation function of the incumbent. Together with prior determinants of incumbent support, such as ideology, personal and economic evaluations, and affinities, the incumbent is either re-elected or is defeated.

1.3 Plan of the dissertation: Three contributions

As shown by the Google Scholar search in Figure 1.2, there is a huge body of literature on political cycles. Despite many important contributions from both political science and economics, important gaps remain. In this dissertation, I address three of the most important gaps. Each chapter is briefly summarized below. In the conclusion chapter, I briefly summarize the contribution of this dissertation to the literature on political cycles, and offer several potential areas of interest for future research.

1.3.1 Seeing the forest through the trees: A meta-analysis of political budget cycles

Despite the unified theory shown in Figure 1.3, the political budget cycle literature contains a substantial number of conflicting theoretical expectations. For instance, Kneebone and McKenzie (2001, p. 757) argue that, "capital expenditures are more 'visible,' and therefore send a stronger signal, than many types of current expenditures." In other words, they expect that incumbents will shift spending to capital expenditures (e.g., road building or infrastructure construction). This stands in sharp contrast to Katsimi and Sarantides (2012, p. 757), who argue that current expenditures (ongoing expenditures such as public sector wages) will increase around elections at the

cost of capital expenditures, "incumbent policymakers may shift public expenditure towards more 'visible' current expenditure and away from less 'visible' capital expenditure."

Not only are theoretical expectations different; empirical findings in the literature also contrast one another. While Brender and Drazen (2005, p. 1292) find that "...results for new democracies are consistent with the view that voters may 'reward' election-year deficit spending, while the findings for established democracies are consistent with the view that they punish it," Alt and Lassen (2006*b*, p. 530) find instead that established democracies are just as likely to observe political budget cycles, "...we find that cycles are present in a sample of 19 advanced industrialized OECD economies, all fully developed and by no means recent democracies."

To reconcile both theoretical differences and empirical findings seen throughout the literature, in Chapter 2 I conduct the first meta-analysis of political budget cycles. Collecting estimates of the political budget cycle effect from 88 studies published between 2000 and 2015 (1198 estimates), I find that there exists a small, yet statistically significant, relationship between elections and budgets. Expenditures and debt tend to increase around elections, while revenues and the fiscal surplus decrease. To investigate how data-, study-, and method-specific differences between articles may be influencing the effect size in the literature, I conduct a meta-regression analysis using Bayesian model averaging. Last, I test for, and find evidence of, publication selection (i.e., "publication bias") in the literature.

1.3.2 Just in time: Political policy cycles of land reform

While the political budget cycle literature focuses on the manipulation of existing budgetary policies, a small number of other articles focus on non-budgetary tools that contain a cyclical component around elections (c.f., DeRouen and Heo 2000; Mechtel and Potrafke 2013; Ahlquist 2010; Tepe and Vanhuysse 2009), as shown in Figure 1.3 under "Budget Cycle Alternatives". However, even among these articles, an analysis of the impact of the passage of redistributive policies themselves remains absent.

In Chapter 3, I contend that policy passage is a strategically timed signal to voters used before elections to benefit the incumbent. Using aggregate data on land reforms passed by 15 states in India from 1957 to 1992, I find that reforms are indeed timed before elections. Second, using historical survey data, I show that land issues remain a strong signal to Indian voters over time, even in states that have already enacted reforms. These findings provide further evidence for budget cycle alternatives.

1.3.3 Strongholds or islands? Spatio-temporal opportunism and intergovernmental transfers

Third, while time has been adequately addressed, space has been completely absent from theories of political cycles. Moreover, the literature on political opportunism has tended to focus on single electoral units in isolation. If an incumbent can strategically time manipulations, can they strategically place them too?

In Chapter 4 I use innovations in spatial econometrics to incorporate theories in distributive politics into the political budget cycle literature. I show that models that incorporate the surrounding spatial units are a better reflection of how incumbents place intergovernmental transfers. Using municipal-level data from Brazil, I find that incumbents tend to reward supporter municipalities surrounded by regions of low support ("islands") rather than core regions of heavy support ("strongholds"). I also find that this strategy changes during municipal and presidential elections towards a more broad-based redistributive scheme. This chapter has implications for the literature on political opportunism and budget cycles, since it suggests the need to take into account the spatial context when examining these cycles at sub-national levels of government.

2. SEEING THE FOREST THROUGH THE TREES: A META-ANALYSIS OF POLITICAL BUDGET CYCLES*

2.1 Introduction

Do incumbents alter budgets before elections in order to secure the support of voters? Known as political budget cycles, research on this question has become a well-established literature in political economy. As of May 2016, a Google Scholar search for "political budget cycle" yields over 1000 results. Despite this large volume of studies, the theoretical underpinnings, empirical findings, and literature reviews of many articles often are at odds with one another. While some scholars have found substantial evidence that governments tend to increase expenditures around elections (Brender and Drazen 2005; Veiga 2012), others have found little to no such evidence (Vergne 2009; Katsimi and Sarantides 2012; Enkelmann and Leibrecht 2013). Although various fiscal instruments have been analyzed by scholars—such as expenditures, revenues, fiscal balance, and public-sector debt—no theory establishes why incumbents might prefer one over the other. More disconcerting, well-argued theories often lead to opposing empirical expectations. For instance, while some scholars theorize that governments increase current transfers during elections at the cost of capital expenditures (Kneebone and McKenzie 2001; Gonzalez 2002; Vergne 2009), others argue that capital expenditures (commonly road or building construction) are more likely than current spending to increase before an election (Schuknecht 2000; Khemani 2004; Drazen and Eslava 2010*b*). All of this begs the question: what do we know about political budget cycles, and how can we reconcile such a large body of literature?

To address this question I use a meta-analysis to conduct the first comprehensive overview of political budget cycle studies. This approach is gaining popularity in economics and political science, having been used to study the relationship between democracy and economic growth, (Doucouliagos and Ulubaşoğlu 2008), oil and democracy (Ahmadov 2014), the factors that influ-

*Reprinted with permission from "Seeing the forest through the trees: A meta-analysis of political budget cycles" by Philips, Andrew Q., 2016. *Public Choice*, 168(3-4):313-341, Copyright 2016 by Springer

ence voter turnout (Smets and Van Ham 2013), and partisan effects on spending (Imbeau, Pétry and Lamari 2001). Surprisingly, no quantitative meta-analysis has studied the relationship between elections and fiscal policies. By treating a regression coefficient from each model in each article as a single observation, this research design offers an ideal way of synthesizing all available information on political budget cycles.

This chapter is motivated by the contexts that make political budget cycles more or less likely to occur. The broad notion that context matters is not new (Franzese 2002; Alt and Rose 2009; Dubois 2016). For instance, countries may have political budget cycles of greater frequency and larger amplitude if they are new democracies (Barberia and Avelino 2011), have less transparent fiscal policies (Alt and Lassen 2006*a*), or lack balanced-budget requirements (Rose 2006). However, attributing how changes in context affect the evidence for political budget cycles is difficult, given that empirical tests are conducted using different data sources, levels of aggregation, and econometric methods. Meta-regression analysis can account for these differences, thus giving us a cleaner assessment as to whether—and in what contexts—political budget cycles exist.

The meta-analytic approach is subject to two common critiques: a meta-analysis combines studies of varying quality, and it compares studies that are fundamentally incomparable owing to study-specific differences (e.g., methodology, data, controls). The first critique is easily addressed by weighting the studies by a measure of "quality", such as the impact factor of the journal in which the article was published, or by the number of citations it has received. The second critique is handled by accounting for any differences between studies that might explain variation in the sizes of estimated effects in a meta-regression analysis. Although excellent qualitative reviews exist (Franzese 2002; De Haan and Klomp 2013; Dubois 2016), by considering all empirical results published from 2000 to 2015, and by quantifying and controlling for observable differences between them, this chapter is more systematic and takes into account more studies than is possible with a single literature review.

Using 1198 estimates from 88 studies, I find evidence that fiscal expenditures and government borrowings increase around elections, while revenues and fiscal balance decline. Although the

magnitude of this effect is small, it remains robust to publication bias, which I find evidence of throughout the literature. I also test empirically how certain contexts may be influencing the results by accounting for data- and methodological-specific differences between studies. I find that a number of factors appear to be driving the differences in estimated effects, among them how the election is coded, whether elections are pre-determined, and if dynamics are addressed. After controlling for other confounders, I find evidence that a number of important factors influence the size of the political budget cycle effect, among them democracy and development. In contrast, others—such as electoral competition and ideology—do not. I also test for and find evidence of publication bias. A statistically significant political budget cycle remains even after accounting for this bias.

The rest of this chapter is structured as follows. I first review the literature on political budget cycles. Next I discuss the research design and data collection. I then present calculated effect sizes across a variety of budgetary categories and sub-categories. Next I examine differences in the magnitudes of the political budget cycles by applying a meta-regression analysis. Finally, I discuss publication bias and conclude with a discussion of the implications of these findings.

2.2 Political budget cycles: A review

Political budget cycles developed out of the literature on opportunistic fiscal policies. According to this theory, politicians take advantage of the Phillips curve, or the negative relationship between inflation and unemployment (Nordhaus 1975). Early works posited that an incumbent will reduce unemployment rates before an election to appeal to voters, only to endure the impending rise in inflation after the election. Hibbs (1977) adapted this conjecture by theorizing how an incumbent may spend based on partisan preferences, thus explaining why left-leaning governments favor low unemployment, while right-leaning governments show concern about the growth rate of the money supply. After the paradigm shift towards rational expectations in political business cycles, a new wave of scholars began to focus on the short-run changes in fiscal expenditures around elections—the theory being that these are used to deceive voters into believing that the economy is doing better than it actually is (Rogoff 1990; Persson and Tabellini 1990; Alesina and Roubini 1992). Out of

this came two distinct literatures. One centered on budget cycles that governments create around elections. For the other, scholars of political business cycles focused on public-sector spending as well as monetary policy and growth. Since then, three broad trends have emerged in the literature on political budget cycles.

The first expanded the unit of analysis. While early studies tested theories in the United States or Western European democracies (Schultz 1995), the literature has moved on recently to developing countries, such as Brazil (Sakurai and Menezes-Filho 2011), Indonesia (Sjahrir, Kis-Katos and Schulze 2013), or India (Sáez and Sinha 2010). Combinations of developed and developing countries have been studied in order to examine how other factors, such as the level of democracy and governmental transparency, affect political budget cycles (Brender and Drazen 2005; Shi and Svensson 2006; Klomp and De Haan 2013*b*), with the broad consensus being that countries with low levels of development, democracy and transparency tend to show more evidence of political budget cycles. Scholars also have begun to focus on elections held at intermediate (Galli and Rossi 2002; Khemani 2004) and local levels of government (Drazen and Eslava 2010*b*; Veiga 2012; Aidt and Mooney 2014), although no theoretical argument has been advanced as to why such cycles may be stronger or weaker at different levels of government.

Second, 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). For instance, fiscal transparency (Alt and Lassen 2006*b*), international oversight (Hyde and O'Mahony 2010), and fiscal stability rules (Rose 2006; Streb and Torrens 2013) may moderate budget deficits around elections. Poor economic conditions may make cycles more likely (Schultz 1995), as may election dates that are fixed (i.e., predetermined) rather than called "early" so that incumbents can take advantage of good economic news (Shi and Svensson 2006). These analyses contribute to the heterogeneous findings found in the literature. Thus, while the theoretical underpinnings of political budget cycles have become richer, comparing studies has become more difficult.

Third, the fiscal variable under analysis has become ever more disaggregated over time. Studies look at multiple budgetary instruments, such as revenues, expenditures, or debt (e.g., Rose 2006;

Gonzalez 2002). Although highly aggregated fiscal categories, such as deficits or total expenditures, rise as elections draw near (Alt and Lassen 2006*b*), scholars now argue that disaggregated expenditures are more likely to be manipulated, since it is easier to allocate budgetary resources to a single spending category than to revise the entire public budget. Moreover, particular budget categories may be more visible to voters. For instance, infrastructure spending (Aidt, Veiga and Veiga 2011) and administrative expenditures (Enkelmann and Leibrecht 2013) have been shown to increase around elections.

How have these three shifts in the literature affected the evidence on political budget cycles? As analyses become more diverse in terms of their data, methodological, and theoretical sophistication, it becomes more difficult to explain why results differ across studies. This should not be seen as a disadvantage, since theories of political budget cycles are richer than ever, and tested in a growing number of countries and contexts. However, it makes comparisons between studies more problematic. Moreover, study-specific differences make it hard to identify the potential causes of changes in political budget cycle effects across studies. A meta-analysis addresses these challenges in two ways. First, it can establish whether an effect exists and, if so, whether it differs across contexts. Second, it allows us to parse out which specific data-, study- and methodology-specific choices influence the findings.

2.3 Research design

To conduct a meta-analysis, I first created a specific set of criteria that had to be met for a study to be included in the current study.[1] First, a search using the terms "political budget cycle" and "political business cycle" was conducted, using both Web of Science and Google Scholar, on articles published from 2000 to 2015. Next, study titles, abstracts, and keywords were screened. For a study to proceed past that stage, it had to mention either political budget cycles or discuss a theoretical relationship between elections and a fiscal outcome. For instance, "the prevalence of electoral cycles in fiscal balance," in the abstract of Alt and Lassen (2006*b*, p. 530) made it eligible for in-

[1] Further details are in the appendix to this chapter.

clusion. In addition, studies had to be published in English in peer-reviewed journals.[2] Borderline cases were included rather than excluded. All told, 232 studies passed the screening stage.

Next, studies that made it beyond the first screening had to meet the following set of eligibility criteria through a full-text reading. First, articles had to contain an empirical test, ruling out studies that included exclusively formal models or qualitative overviews of political budget cycles. Second, articles must have used a fiscal measure as a dependent variable, ruling out studies of monetary policy. While studies of monetary cycles are important, these dependent variables (typically output, inflation, and money growth) are too distinct from fiscal cycles to be included in a combined meta-analysis. Third, since the main variable of interest in studies of political budget cycles is the election, studies that did not include some form of election variable were dropped. Fourth, I did not include any studies that exclusively tested an interactive effect between elections and another covariate, since conditional coefficients are not directly comparable across studies. Instead, the meta-analysis below offers a way to tease out how important factors affect political budget cycles indirectly. Finally, an estimate of the size of the effect of elections on budgets had to be reported, along with an associated measure of statistical precision. A total of 88 studies were eligible for inclusion in the meta-analysis. Included studies, as well as the full list of excluded studies are available in the appendix to this chapter.

When conducting a meta-analysis, raw coefficients reported in a study must first be converted into an associated magnitude in order to make them comparable across studies (Stanley 2001; Borenstein et al. 2011). One of the most useful metrics of standardized effects are partial correlation coefficients:

$$\varepsilon_{ij} = \sqrt{\frac{t_{ij}^2}{(t_{ij}^2 + df_{ij})}} \tag{2.1}$$

where ε_{ij} is the partial correlation coefficient of study i and model j, t_{ij}^2 is the squared t-statistic from the regression, and df_{ij} represents the degrees of freedom (Stanley and Doucouliagos 2012).[3] Since this calculation creates a positive ε_{ij} by construction, it must be converted into a negative

[2] As have others (Doucouliagos and Ulubaşoğlu 2008), I did not include unpublished results. I address potential publication bias in the appendix to this chapter.

[3] Standard errors and p-values were converted into t-statistics if they were reported.

correlation if the t-statistic carried that sign, thus bounding ε_{ij} between -1 and 1. Alternatives to calculating partial correlations exist, such as "vote-counting" (a tabulation of significant and non-significant results), meta-probit analysis (Smets and Van Ham 2013), or a "success-rate" of hypothesized directions (Imbeau, Pétry and Lamari 2001). However, partial correlations are preferable since this technique accounts for the sampling error of the estimated effect by adding weights, as shown below (Stanley and Doucouliagos 2012).

The standard error of the partial correlation is given as

$$SE_{ij} = \sqrt{\frac{1-\varepsilon_{ij}}{df_{ij}}} \tag{2.2}$$

Its inverse is used as a measure of estimate precision.

After obtaining partial correlations for each study, the total size of the political budget cycle effect can be obtained as follows:

$$\varepsilon = \frac{\sum(N_{ij}\varepsilon_{ij})}{\sum N_{ij}} \tag{2.3}$$

where the size of the total effect, ε, is given by the sum of the partial correlations calculated in Equation 2.1 multiplied by an assigned weight N_{ij} for each study, divided by the sum of the weights, N_{ij}. A number of weights can be assigned in Equation 2.3. Keeping with the dominant trend in economics and political science (Doucouliagos and Ulubaşoğlu 2008; Ahmadov 2014), I let N_{ij} be the number of observations, although the results remain robust to two other forms of weighting—such as the number of an article's citations, the journal's impact factor, and the inverse of the standard error of the partial correlation.[4]

2.4 Results

To summarize the results of the partial correlations visually, I use funnel plots, shown in Figure 2.1. The calculated sizes of the 1198 models' partial correlations are shown on the the vertical

[4]These results are available in the appendix to this chapter.

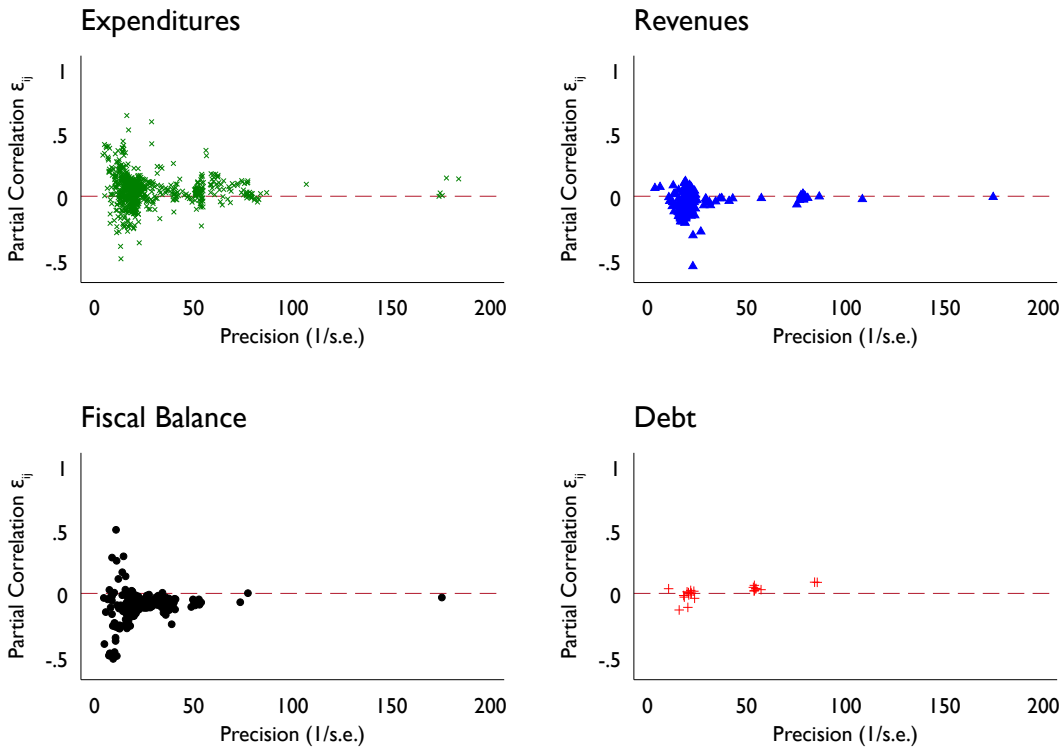
axis, while the precision of the partial correlation (inverse of the standard error) is on the horizontal axis. I disaggregate the calculated partial correlations into four distinct groups:

- Expenditures are the most often-studied dependent variable, and constitute the largest category in the sample (699 of the 1198 study observations).
- Revenues are another important category in the literature, comprising 243 of the 1198 total observations. Evidence suggests that revenues tend to decline around elections (Barberia and Avelino 2011; Katsimi and Sarantides 2012; Aidt and Mooney 2014).
- Fiscal balances (revenues minus expenditures) show the net effect of elections on budgets, and are not directly comparable to either expenditures or revenues. Out of the 1198 observations coded for this analysis, 234 were of fiscal balance.[5]
- Last, debt is a rarely studied (only 22 out of 1198 observations) dependent variable that is distinct from the other categories.

As shown in Figure 2.1, expenditures tend to be above the horizontal dashed line. Since the calculated partial correlation is positive with an average of around 0.05, this indicates that expenditures tend to increase in the election year. Although the effect lies in the expected positive direction, it is not large; Cohen (2013) suggests that a standardized effect is small if less than 0.10, moderate if it is around 0.25, and large if greater than 0.40. With an average partial correlation of 0.01, the same appears to be true for debt, although with only 22 observations the evidence is much less conclusive. In contrast to these two categories, revenues always tend to have negative calculated partial correlations, with an average of -0.05. This suggests—in line with the literature—that revenues fall in an election year. The fiscal balance category shows a similar effect; calculated partial correlations tend always to be negative, indicating that deficit spending increases in election years. In fact, the average partial correlation for fiscal balance is -0.11, about double the magnitude of the other types of dependent variables.

[5] I recoded studies that examined deficit spending as their dependent variable, so that a positive partial correlation indicates an increase in fiscal surplus for all models.

Figure 2.1: Funnel plots of the four budgetary categories



*N*otes: 1198 total estimates for 88 studies.

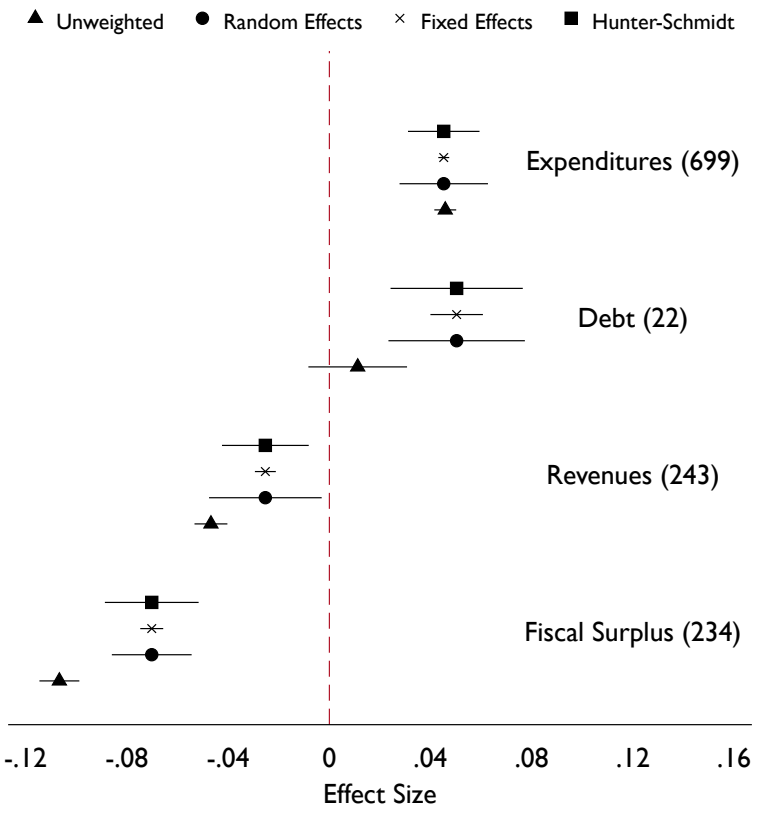
2.5 Disaggregation and measures of uncertainty

Based on Figure 2.1, expenditures and public debt appear to increase during an election year, and revenues and fiscal surpluses to decline. However, these are simple averages and do not take into account the precision associated with each calculated partial correlation. To examine this further, I plot calculations of magnitude in Figure 2.2, along with 95% confidence intervals. Recall from Equation 2.3 that the size of an estimated effect, ε, is the sum of each partial correlation multiplied by an assigned weight (such as the number of observations), divided by the sum of the weights. Confidence intervals are calculated four ways. The first is an estimate of the unweighted effect—analogous to an unweighted average effect. Since the other three confidence intervals weight by the number of observations, the sizes of their effects are identical. However, the estimated effects differ in terms of how the error term is modeled. Confidence intervals calculated using random effects model the error as a function of a purely stochastic component, u_{ij}, as well as a study-specific residual, v_j. In contrast, fixed effects do not allow for study-specific errors. The random effects model is almost always preferable to the fixed effects model since the latter is inappropriate if unexplainable heterogeneity remains in the true effect size; this is common in nearly all social science applications (Stanley and Doucouliagos 2012).[6] The final confidence interval calculation comes from Hunter and Schmidt (2004) and estimates the "heterogeneity variance by calculating the difference between the total variance of the effect estimates and an average of the estimated within-study variances" (Sánchez-Meca and Marín-Martínez 2008, p. 35).

As shown in Figure 2.2, even after accounting for sampling error using random effects, fixed effects, or Hunter-Schmidt confidence intervals, a political budget cycle exists that is statistically significantly different from zero for all four types of dependent variable. The sizes of the effects of expenditures and debt are nearly identical, positive (albeit small), and around 0.05. This means that, taken as a whole, the literature finds statistically significant evidence that expenditures and debt increase in an election year. In contrast, scholars who have examined revenues and fiscal

[6] In fact, fixed effects often overstate our confidence in the coverage probability of the true effect (Sánchez-Meca and Marín-Martínez 2008; Borenstein et al. 2011). This is formally tested through the Q-test; I was able to reject the null hypothesis of no heterogeneity.

Figure 2.2: The political budget cycle effect across four major categories



Notes: Study-model observations in parentheses. 95% confidence intervals reported.

surplus tend to find a statistically significant negative relationship. Moreover, the magnitude of this effect appears to be about twice as large for fiscal surpluses as it is for revenues.

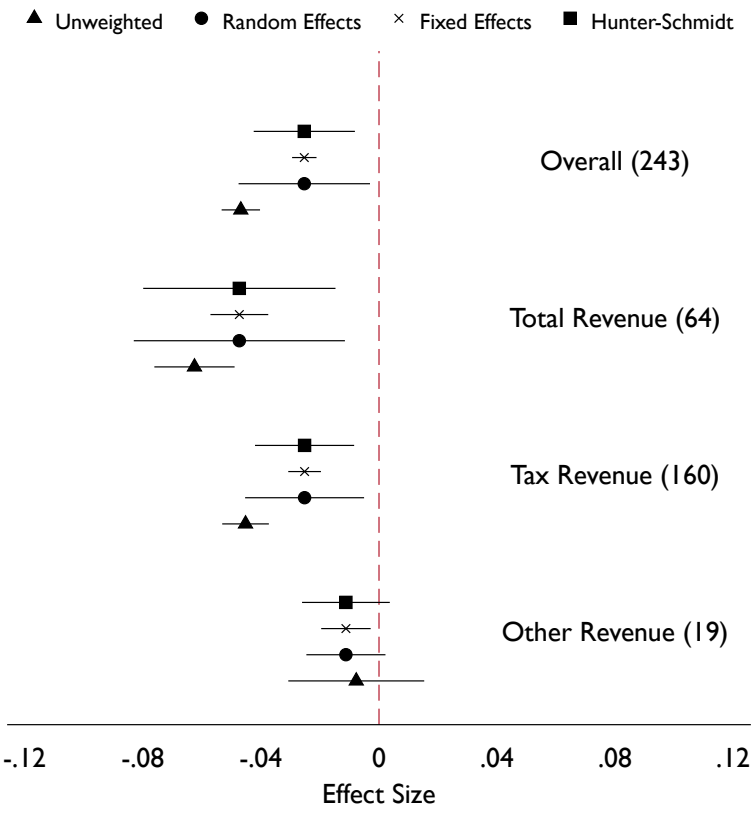
While Figure 2.2 showed evidence of a political budget cycle across the four broad categories of dependent variables, are certain types of expenditures and revenues more likely to be manipulated around elections? Much of the literature has focused on how the pre-election composition of spending may change (Vergne 2009; De Haan and Klomp 2013; Klomp and De Haan 2013*a*). For instance, visible budget items that appeal to a broad range of voters, such as social welfare policy (Chang 2008), may be more likely to increase prior to Election Day than a more narrow budget category like administrative expenditures. For revenues, tax breaks may be targeted to certain key voter constituencies (Khemani 2004).

To investigate whether certain budgetary categories may be driving the sizes of the effects reported above, I disaggregate the calculated effect sizes further, as other meta-analyses have done (Lau, Sigelman and Rovner 2007). I divide the 243 revenue-study observations into three of the largest categories, and the 699 observations for expenditures into seven categories.[7] The results for revenue are shown in Figure 2.3. The overall effect of all 243 revenue observations is shown for reference at the top. I disaggregated revenues into studies that modeled total revenues, those that explicitly modeled tax revenues, and those that modeled "other" types of revenues (mostly non-tax sources). It is clear from Figure 2.3 that studies of total revenue tend to have the largest negative relationship with elections. Tax revenues, such as those on income and property, have an estimated effect size of -0.03, while those for other revenue sources are not statistically significantly different from zero. Taken together, this evidence suggests that total revenues and tax revenues tend to decline the most around elections. Non-tax revenues do not appear to be manipulated around Election Day.

In Figure 2.4, I show the size of the overall effect for expenditures as well as the seven largest sub-categories: inter-governmental grants, total expenditures, administrative expenditures, education and health expenditures, capital expenditures, other, and current expenditures. As with

[7] A detailed breakdown of these categories is in the appendix to this chapter.

Figure 2.3: The political budget cycle effect: Revenue disaggregation



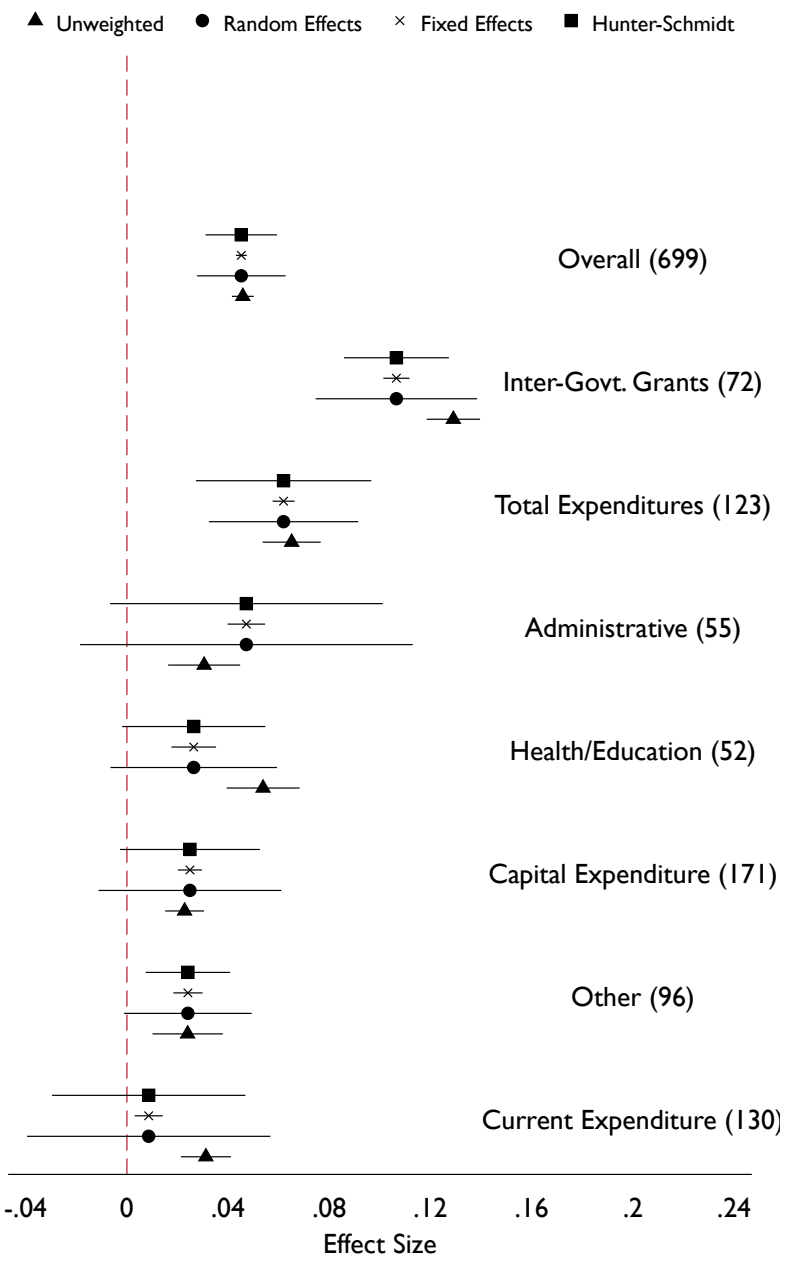
Notes: Study-model observations in parentheses. 95% confidence intervals reported.

revenues, substantial differences are evident in the political budget cycle effect, depending on the fiscal variable considered. The strongest evidence of an effect in Figure 2.4 appears to be for intergovernmental grants. For instance, Veiga (2012) reports evidence that grants from the European Union to Portuguese municipalities tend to increase during election years. John and Ward (2001) find that central government grants to UK local authorities increase in election years, and Padovano (2012) finds a similar effect for Italian regions. Overall, the estimated effects of intergovernmental grants are more than double the overall effect. For studies that use total expenditures as a dependent variable, the effect is slightly larger than the overall effect, although some overlap is evident after accounting for sampling error.

The last five categories presented towards the bottom of Figure 2.4 include some of the most disaggregated components of spending that have been studied in the literature. In fact, theories about "visible" expenditures suggest that capital and current expenditures are some of the most likely budget categories to be manipulated. This makes the finding that these fiscal categories have some of the *weakest* evidence for a political budget cycle effect notable. The findings here may be explained best by the conflicting state of the literature. Some authors find that capital expenditures, which typically create some form of asset in a one-time event, tend to increase before elections (Schuknecht 2000; Khemani 2004; Drazen and Eslava 2010*b*). In contrast, others find that current expenditures, which involve ongoing payments, such as salaries or subsidies, are more susceptible (Kneebone and McKenzie 2001; Gonzalez 2002; Katsimi and Sarantides 2012). Such inconclusive results are clear in this analysis as well. Neither category tends to be statistically significantly different from zero once sampling error is accounted for, though the effect for capital expenditures is slightly larger. Thus, it appears that neither category is consistently manipulated as a more visible form of spending.

Administrative expenditures also have an effect that tends to overlap zero. Such uncertainty is apparent in the literature; examining administrative expenditures, Drazen and Eslava (2010*b*) find a positive effect, Enkelmann and Leibrecht (2013) find virtually no effect, while Aidt and Mooney (2014) find a negative effect. For models that focus on education and health spending, a small,

Figure 2.4: The political budget cycle effect: Expenditure disaggregation



Notes: Study-model observations in parentheses. 95% confidence intervals reported.

insignificant effect is evident. Since these two expenditures are often viewed as likely targets for fiscal manipulation, this finding is particularly interesting. The last calculated effect in Figure 2.4 is an "Other" category, which consists of fiscal variables, such as spending on agriculture, media, and defense. As with the other disaggregated categories, little evidence of a significant political budget cycle effect can be found.

2.6 Explaining context through meta-regression

The previous section reported clear evidence of a political budget cycle for fiscal surplus, expenditures, revenue, and debt. While calculating the effect of the partial correlation adds a measure of uncertainty to our estimates, it does not inform us as to *how much* a particular data-specific or methodological choice alters the magnitude of the budget cycle. Moreover, although calculated effects appear to vary greatly across the various disaggregations explored above, the findings did not account for the possibility that other confounding variables may be explaining some of these differences. To examine this I turn to meta-regression analysis (MRA). This approach attempts to explain between-study variance by regressing the partial correlation of model j of study i on so-called "moderator variables"—data characteristics, methodological choices, and author- or paper-specific characteristics:

$$\begin{aligned} \varepsilon_{ij} = & f(\textit{fiscal variables}, \textit{data characteristics}, \textit{author/study characteristics}, \\ & \textit{methodology}, \textit{election variables}, \textit{moderating variables}) \end{aligned} \tag{2.4}$$

The regressors in Equation 2.4 can identify important contextual conditions in the literature (Stanley 2001). A significant positive (negative) coefficient indicates that the presence of the moderator variable tends to increase (reduce) the partial correlation, ε_{ij}, all else equal. While MRAs cannot identify the magnitude of the effect that a larger or smaller moderator variable has on the partial correlation, it does inform us as to the relative importance of the variable on the size of the effect reported in the literature. Statistical (and substantive) significance of a moderator variable suggests that it should be included in future studies of political budget cycles, since it appears to

condition the relationship between elections and budgets. For variables measuring the characteristics of the model, study, or data, statistical significance in the MRA suggests that an analysis with that particular moderator variable is a relative outlier and should be generalized to the broader population with caution.

One drawback to MRAs is that considerable disagreement exists on which moderator variables to include. Multicollinearity and few degrees of freedom can be problematic, just as with standard regressions. Because of this, I am able to analyze only the three most important dependent variable categories: fiscal balance, expenditures, and revenues.[8] In addition to the different categories of the dependent variables analyzed in the previous section, I identified and coded 40 other candidate covariates that may contribute to differences among partial correlations. These are shown in Table 2.1, along with means, standard deviations, and a brief description.

Table 2.1: Candidate covariates that may explain differences in partial correlations

Variable	Variable description	Mean	Std. dev.
Expenditure variables			
Total expenditures	= 1 if dependent variable is total expenditures	0.10	0.31
Inter-govt. grants	= 1 if dependent variable is grants/transfers	0.06	0.24
Capital expenditures	= 1 if dependent variable is capital expenditures	0.15	0.35
Current expenditures	= 1 if dependent variable is current expenditures	0.11	0.31
Administrative expenditures	= 1 if dependent variable is administrative expenditures	0.05	0.21
Education and health exp.	= 1 if dependent variable is health/education expenditures	0.04	0.21
Revenue variables			
Total revenue	= 1 if dependent variable is total revenue	0.05	0.23
Tax revenue	= 1 if dependent variable is tax revenue	0.14	0.34
Data characteristics			
Standard error	Standard error of the partial correlation	0.05	0.03
OECD	= 1 if at least one country is in OECD	0.68	0.47
Latin America	= 1 if at least one country is in Latin America	0.23	0.42
Asia	= 1 if at least one country is in Asia	0.17	0.38
Sub-Saharan Africa	= 1 if at least one country is in Sub-Saharan Africa	0.09	0.29
E. Europe and Fmr. USSR	= 1 if at least one country is in E. Europe and Fmr. USSR	0.14	0.34
Average year	Average year in sample	1986.4	16.97
Quarterly aggregation	= 1 if temporal aggregation is quarterly	0.06	0.23
Monthly aggregation	= 1 if temporal aggregation is monthly	0.02	0.13
Single country	= 1 if single-country study	0.46	0.50
Municipal aggregation	= 1 if election level is municipal	0.21	0.40
State aggregation	= 1 if election level is state/provincial	0.17	0.37
Moderating variables			
Democracy	= 1 if model controls for democracy (dummy, level, or index)	0.01	0.10

[8] Debt has only 22 model-study observations and is excluded.

Table 2.1 Continued

Variable	Variable description	Mean	Std. dev.
Coalition	= 1 if model controls for coalition, maj/min government	0.06	0.23
Debt (control)	= 1 if model controls for debt level or percent	0.04	0.19
Deficit (control)	= 1 if model controls for deficit level or percent	0.03	0.17
Govt. expenditures	= 1 if model controls for total government expenditures	0.03	0.17
Govt. revenues	= 1 if model controls for total government revenues	0.06	0.24
Transfers (control)	= 1 if model controls for inter-governmental transfers	0.07	0.25
GDP	= 1 if model controls for output, in levels	0.61	0.49
GDP growth	= 1 if model controls for output growth	0.34	0.47
Ideology	= 1 if model controls for government ideology (dummy or index)	0.21	0.40
Inflation	= 1 if model controls for inflation	0.03	0.18
Presidential	= 1 if model controls for presidential systems	0.01	0.12
Proportional	= 1 if model controls for PR system	0.03	0.16
Unemployment	= 1 if model controls for unemployment	0.12	0.32
Win margin	= 1 if model controls for margin of victory of past election	0.14	0.35
Methodology			
Unit fixed-effects	= 1 if model has unit/regional fixed effects	0.59	0.49
Lagged dep. var.	= 1 if model includes lagged dependent variable/GMM	0.80	0.40
OLS PCSE GLS	= 1 if model uses OLS, panel-corrected std. errors or GLS	0.14	0.35
Election variables			
Election dummy	= 1 if model uses simple dummy	0.57	0.50
Elec. half-yr	= 1 if model uses dummy with May-June cutoff	0.09	0.29
Franzese	= 1 if model uses Franzese (2000) method	0.17	0.37
Election pre-determined	= 1 if election variable is for fixed elections	0.13	0.34
Election early	= 1 if election variable is for elections called early	0.12	0.32
Election$_{t+1}$	= 1 if model includes period after election	0.10	0.30
Election$_{t-1}$	= 1 if model includes period before election	0.19	0.40
Study characteristics			
Total models	Total number of models per study	79.12	94.48
Cites per year	Average article citations per year	3.00	3.48
Impact factor	Journal's impact factor (2013)	1.11	0.84

Notes: $N = 1176$ for 79 studies. Debt is excluded due to lack of observations.

Based on the earlier findings as well as the previous literature, I expect a number of covariates to be particularly influential. Since there was substantial heterogeneity in estimated effects across the seven expenditure categories and three revenue categories, these are each included as dichotomous variables, with "other expenditures" and "other revenues" as the omitted category for each analysis. A number of data characteristics also may influence the results. The standard error of the calculated partial correlations is included to proxy for estimation precision (Stanley and Doucouliagos 2012). I also include regional dummies since some regions may be more susceptible to political budget cycles than others, as cross-national analyses of recently democratic countries in Latin America

(Barberia and Avelino 2011), and elsewhere (Brender and Drazen 2005) have shown. In addition, since the estimated effect could be influenced by temporal aggregation and the governing level at which elections were held, these, too, are entered. While previous authors have investigated how temporal aggregation may affect the likelihood of observing political budget cycles within a single dataset (Streb, Lema and Garofalo 2012; Klomp and De Haan 2013*b*), a MRA offers a more comprehensive approach as to how aggregation may influence the sizes and significances of political budget cycles.

I include 15 moderating variables that may condition political budget cycles. Each variable is a dichotomous indicator equal to one if the study in question included that variable. When entered into a MRA, a significant coefficient suggests that this variable may have an important conditional relationship with political budget cycles. Democracy is added with the expectation that including democracy in a regression will reduce the sizes of the partial correlations. Coalition governments are controlled for since they may affect political budget cycles (Hanusch 2012*a*). A variety of fiscal controls, such as debt and expenditures, are entered since they are common in most analyses of the political budget cycle phenomenon. I control for whether economic conditions, such as GDP and unemployment, are held constant. I also include variables for presidential and proportional systems in addition to the win margin of the victorious candidate or party.

A variety of dichotomous variables are entered to account for the methodology used in the analysis. I include a dichotomous variable equal to one if the model contained unit fixed-effects. I also include a variable controlling for whether or not a lagged dependent variable was entered in the model, as well as if a simple OLS or a GLS model was estimated.

Since the election indicator is the key independent variable of interest, I explore how differences in coding affect political budget cycles. Three variables account for the most common types seen in the literature. The first, *Election dummy*, takes on a value of one if the election variable also takes on a value of one in the election period. *Elec. half-yr.* accounts for studies that establish a half-year cutoff for which the dummy variable equals one in the year of the election if it is held after May or June, or equals one in the year *before* the election if the election is held prior to May or June.

Last, some studies adopt the technique attributed to Franzese (2000), which enters a variable equal to $\frac{M}{12}$ in the election year (where M equals the month of the election), and $1 - \frac{M}{12}$ in the year before the election, thus weighting the election-year indicator based on the month in which the election is held. I also account for whether the study controlled for the period before an election, $Election_{t-1}$, as well as the period after the election, $Election_{t+1}$.

MRAs can also control for a variety of study characteristics that may proxy quality. To investigate how this may affect the size of the partial correlations, I include the total number of models in a given study, the average number of citations per year an article has received, and the 2013 impact factor of the journal in which the article was published. I expect that, especially if any form of publication bias exists, studies with more citations or published in journals with high impact factors will have stronger evidence for political budget cycles.

2.7 Results

Results from the MRA for expenditures are shown in Table 2.2. Model 1 uses the random-effects specification described earlier by regressing the calculated partial correlations on the list of characteristics that could be influencing the estimated political budget cycle effect. Model 2 assumes no between-study variance using the fixed-effects specification, so its large proportion of statistically significant results should be interpreted with caution. Both Models 1 and 2 are weighted by the number of study observations. In addition to a frequentist approach, Model 3 uses a data-driven approach to see which moderator variables are important for explaining differences in the partial correlations through Bayesian model averaging (BMA). This strategy has been used in previous meta-analyses (Iršová and Havránek 2013; Moeltner and Woodward 2009), and is useful for a number of reasons. First, it can be used to find the model that explains more variation than all others the algorithm covers. Second, we obtain posterior inclusion probabilities for each covariate—or the likelihood that a given variable enters the final model. By convention, variables with posterior inclusion probabilities exceeding 0.10 are deemed important; these are shown in italics in Model 3. Third, the resulting model produces a posterior mean and standard deviation, which are analogous to a coefficient and standard error in a frequentist model. Finally, by searching for the model

that maximizes explained variance by including or excluding candidate covariates, BMA offers an ideal tradeoff between a parsimonious (yet potentially underspecified) model, and one that—by including all covariates—is as comprehensive as possible, yet potentially over-saturated and full of extraneous variables (Aguinis, Gottfredson and Wright 2011). Model 3 is estimated using a Markov chain Monte Carlo approach to select candidate models using the Metropolis-Hastings algorithm (Feldkircher and Zeugner 2009).[9] Last, Model 4 re-estimates a random-effects model using only variables that had posterior inclusion probabilities of 0.10 or larger in Model 3.

Table 2.2: Explaining differences in partial correlations of expenditures

	Model 1 RE		Model 2 FE		Model 3 BMA		Model 4 RE	
Data characteristics								
Standard error	-3.372***	(1.060)	-3.372***	(0.396)	*1.100*	(0.077)	0.668*	(0.392)
OECD	0.811**	(0.378)	0.811***	(0.140)	0.002	(0.008)		
Latin America	0.011	(0.034)	0.011	(0.010)	0.001	(0.008)		
Asia	-0.015	(0.031)	-0.015	(0.009)	*-0.021*	(0.031)	-0.012	(0.022)
Sub-Saharan Africa	-0.001	(0.041)	-0.001	(0.012)	0.038	(0.048)		
E. Europe and Fmr. USSR	0.061	(0.043)	0.061***	(0.017)	*-0.002*	(0.009)	0.022	(0.029)
Average year	-0.051**	(0.021)	-0.051***	(0.009)	*0.000*	(0.000)	0.001	(0.0004)
Quarterly aggregation	0.002***	(0.001)	0.002***	(0.000)	0.001	(0.009)		
Monthly aggregation	0.111*	(0.060)	0.111***	(0.029)	0.001	(0.008)		
Single country	-0.120	(0.087)	-0.120***	(0.029)	0.002	(0.008)		
Municipal aggregation	0.000	(0.042)	0.000	(0.009)	*0.016*	(0.022)	0.039**	(0.019)
State aggregation	0.052**	(0.024)	0.052***	(0.005)	*-0.028*	(0.031)	0.057**	(0.023)
Moderating variables								
Democracy	0.042	(0.029)	0.042***	(0.009)	*0.014*	(0.036)	0.062	(1.466)
Coalition	0.076	(0.072)	0.076***	(0.025)	0.002	(0.009)		
Debt	-0.078**	(0.037)	-0.078***	(0.009)	0.002	(0.010)		
Deficits	0.055	(0.033)	0.055***	(0.016)	0.001	(0.006)		
GDP	-0.011	(0.044)	-0.011	(0.011)	0.000	(0.002)		
GDP growth	0.026	(0.018)	0.026***	(0.005)	-0.000	(0.002)		
Expenditures (control)	-0.073**	(0.036)	-0.073***	(0.012)	*-0.036*	(0.039)	-0.047*	(0.026)
Ideology	-0.061**	(0.027)	-0.061***	(0.006)	0.001	(0.006)		
Inflation	0.052**	(0.021)	0.052***	(0.005)	0.002	(0.010)		
Presidential	0.001	(0.053)	0.001	(0.011)	0.005	(0.020)		
Proportional	-0.005	(0.059)	-0.005	(0.022)	-0.000	(0.008)		
Revenues (control)	0.022	(0.062)	0.022	(0.021)	*0.079*	(0.026)	0.030*	(0.022)
Transfers (control)	0.035	(0.023)	0.035***	(0.006)	*0.008*	(0.019)	0.037*	(0.021)
Unemployment	0.017	(0.033)	0.017***	(0.006)	*0.038*	(0.024)	0.026*	(0.015)
Win margin	0.001	(0.022)	0.001	(0.008)	0.000	(0.004)		

[9]There are two important priors to specify. The first is how many variables should be included in the "true" model. Since I have no prior expectation as to how many variables should be included, I chose a diffuse beta-binomial model prior (Ley and Steel 2009). The second set of priors concern the coefficients. I chose uninformed coefficient priors (Fernandez, Ley and Steel 2001), although the findings remain robust to alternative priors, as detailed in the appendix to this chapter.

Table 2.2 Continued

	Model 1 RE		Model 2 FE		Model 3 BMA		Model 4 RE	
Methodology								
Unit fixed-effects	0.028	(0.027)	0.028***	(0.008)	-0.000	(0.003)		
Lagged dep. var.	-0.036	(0.049)	-0.036***	(0.005)	*-0.094*	(0.016)	-0.0108	(0.026)
OLS PCSE GLS	-0.015	(0.022)	-0.015**	(0.006)	*0.013*	(0.022)	0.001	(0.022)
Election variables								
Election dummy	-0.050	(0.055)	-0.050***	(0.008)	*-0.010*	(0.018)	-0.026	(0.019)
Franzese	-0.031	(0.027)	-0.031***	(0.010)	0.000	(0.002)		
Elec. half-yr	0.042	(0.028)	0.042***	(0.010)	*0.005*	(0.015)	0.010	(0.425)
Election pre-determined	-0.067	(0.047)	-0.067***	(0.015)	*-0.008*	(0.017)	-0.031**	(0.015)
Election early	-0.029	(0.025)	-0.029***	(0.010)	0.000	(0.004)		
Election_{t+1}	0.008	(0.025)	0.008	(0.010)	*0.010*	(0.022)	0.040	(0.027)
Election_{t-1}	0.104***	(0.033)	0.104***	(0.012)	-0.000	(0.002)		
Study characteristics								
Total models	-0.031	(0.023)	-0.031***	(0.005)	-0.000	(0.000)		
Cites per year	0.000	(0.000)	0.000*	(0.000)	*0.005*	(0.004)	0.006*	(0.003)
Impact factor	0.004	(0.004)	0.004***	(0.001)	*0.006*	(0.010)	-0.028***	(0.010)
Expenditure variables								
Administrative expenditures	-0.003	(0.017)	-0.003	(0.006)	-0.000	(0.003)		
Capital expenditures	0.015	(0.026)	0.015***	(0.005)	-0.000	(0.002)		
Current expenditures	-0.020	(0.027)	-0.020***	(0.005)	0.002	(0.008)		
Inter-govt. grants	-0.028	(0.028)	-0.028	(0.005)	*-0.077*	(0.022)	0.112***	(0.024)
Education and health exp.	0.068**	(0.032)	0.068***	(0.008)	0.000	(0.002)		
Total expenditures	-0.005	(0.028)	-0.005	(0.006)	*0.016*	(0.020)	0.034	(0.027)
Constant	0.016	(0.031)	0.016***	(0.005)			-1.209	(0.790)
R^2	0.32						0.30	
I^2	0.88						0.89	

Notes: Dependent variable is the partial correlation of study-model ij. Coefficients with standard errors in parentheses for Models 1, 2, and 4. Posterior means with posterior standard deviations in parentheses for Model 3. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ for frequentist models. Estimates in italics in Model 3 indicate a posterior inclusion probability greater than 0.10. 699 model-study observations for 61 studies for all models, with a total sample size of 1,027,186.

2.7.1 Data characteristics

In a MRA, characteristics of the data often influence the sizes of the partial correlations. The variable that accounts for the standard error of the partial correlation has a large negative effect in Models 1 and 2 and a somewhat smaller positive effect in Models 3 and 4. These mixed yet strong effects are not surprising given that the funnel plot in Figure 2.1 suggested that estimates with less precision (larger standard errors) tend to produce large positive or negative partial correlations. In other words, studies with less precision find evidence of a substantial political budget cycle

effect, but they also are likely to report evidence that expenditures *decline* around elections. With the exception of the OECD, the five regional dummies generally reveal no statistically significant effects on the partial correlation. This OECD finding is surprising; after controlling for other factors, a substantial relationship exists between elections and expenditures in OECD countries. The same is true for studies examining countries of Eastern Europe and the former Soviet Union, though the effect is smaller. In contrast, Asian countries appear to be less susceptible to political budget cycles. The average year covered by the sample appears to have some influence on the partial correlation, though this effect is negative in Models 1 and 2 and positive and near-zero in Models 3 and 4.

Characteristics relating to the temporal aggregation and level of analysis also appear to affect the sizes of political budget cycles. Relative to studies using annual data, those using quarterly and monthly observations tend to find more positive partial correlations, all else equal. This suggests that temporally disaggregated data are better at detecting political budget cycles. Studies of one country only appear to produce results similar to studies examining multiple countries. In contrast, the level of government at which an election is held appears to be driving some of the differences in partial correlations; studies of municipal elections are positive (albeit near-zero for Models 1 and 2) across all models. Studies at the state or provincial level find even stronger political budget cycle effects, with the positive coefficient remaining robust across all estimates except Model 3. All of this evidence suggests that sub-national political budget cycles tend to be stronger than national ones.

2.7.2 Moderating variables

The significance of some of the moderating variables in Table 2.2 suggests that important conditional relationships may play a role in political budget cycles. The consolidation and duration of democracy has been theorized to lead to smaller political budget cycles (Brender and Drazen 2005; Barberia and Avelino 2011; De Haan and Klomp 2013). Table 2.2 supports this; studies that control for the effects of democracy tend to have a larger partial correlation, all else equal. Thus, democracy appears to be an important factor that explains the relationship between elections and

expenditures. Controlling for coalitions does not have a statistically significant effect, which suggests that coalitions and non-coalitions are equally likely to create political budget cycles. While accounting for debt tends to decrease the partial correlations, deficits appear to be unrelated to the political budget cycle effect. Neither GDP in levels or GDP growth appears to influence the size of the partial correlations, suggesting that both advanced and developing economies are equally susceptible to political budget cycles. In contrast, controlling for expenditures appears to be an important predictor of the size of the partial correlations. Controlling for expenditures leads to smaller correlations, as does ideology. In contrast, accounting for inflation appears to increase the size of the partial correlations. Neither presidential or proportional systems, or win margin, appear to have any moderating effect on political budget cycles. This is consistent with the mixed evidence on the effect that parliamentary and majoritarian systems have on political budget cycles (Persson and Tabellini 2005; Streb, Lema and Torrens 2009; Klomp and De Haan 2013*b*). Last, controlling for revenues, transfers, and unemployment appears to be important, as evidenced by the positive coefficients in Table 2.2.

2.7.3 Methodology

The significance of some of the moderating variables in Table 2.2 suggests that important conditional relationships may play important roles in the political budget cycle literature. The consolidation and duration of democracy has been theorized to lead to smaller political budget cycles (Brender and Drazen 2005; Barberia and Avelino 2011; De Haan and Klomp 2013). Table 2.2 supports this finding; studies that control for the effects of democracy tend to produce a larger partial correlation, all else equal. Thus, democracy appears to be a key factor in explaining the relationship between elections and public spending. Controlling for coalition goverments does not have a statistically significant effect, which suggests that coalition and non-coalition governments are equally likely to create political budget cycles. While accounting for debt tends to reduce the partial correlations, deficits appear to be unrelated to the political budget cycle effect. Neither GDP in levels nor GDP growth appears to influence the size of the partial correlations, suggesting that both advanced and developing economies are equally susceptible to political budget cycles. In contrast,

controlling for expenditures appears to be an important predictor of the size of the partial correlations. Controlling for expenditures leads to smaller correlations, as does political ideology. On the other hand, accounting for inflation appears to increase the size of the partial correlations. Neither presidential nor proportional systems, or win margins, appear to have any moderating effect on political budget cycles. This is consistent with the mixed evidence on the effect that parliamentary and majoritarian systems have on political budget cycles (Persson and Tabellini 2005; Streb, Lema and Torrens 2009; Klomp and De Haan 2013*b*). Last, controlling for revenues, transfers and unemployment appears to be important, as evidenced by the positive coefficients in Table 2.2.

2.7.4 Election variables

Since many studies investigate robustness by varying the electoral "window" (e.g., Streb, Lema and Garofalo 2012), or the coding of the election variables, the results in Table 2.2 are of particular importance. I find evidence that coding the election as a simple dummy variable in the year of the election tends to lead to smaller partial correlations. In contrast, fine-grained approaches, such as coding with a half-year cutoff point, tend to lead to larger partial correlations. This result suggests that, all else equal, political budget cycles may be influenced by how the election timing is controlled for, although note that the method used by Franzese appears not to affect the size of the partial correlations. Moreover, the significance of the pre-determined election variable suggests that differences exist between fixed and non-fixed election dates. This finding is in line with the large body of literature on how elections are not always exogenous to the public budget process (Shi and Svensson 2006). Last, pre- and post- election dummy variables often are included in studies of political budget cycles. I find that studies that enter such control variables tend to find more positive partial correlations. Accounting for pre- and post-election periods thus seems to be important in studying electoral effects on public budgets. Taken together, these results suggest that political budget cycles are sensitive to the coding of elections (more fine-grained indicators are better at uncovering the underlying effect) as well as varying the electoral window.

2.7.5 Study characteristics

Study characteristics appear to have only minor effects on political budget cycles. The total number of models appear to be unrelated to the partial correlations. Both the number of study citations per year and the journal's impact factor are positively related to the partial correlations in Models 1, 2, and 3, although this effect is substantively very small. However, the fact that both variables enter into Model 4 (i.e., had a high posterior inclusion probability using Bayesian model averaging) suggests that they are important predictors of the partial correlations.

2.7.6 Fiscal variables

Out of the six fiscal variables in Table 2.2, only intergovernmental grants, education and health spending, and total expenditures appear to explain differences among the partial correlations. Although nearly all fiscal variables are statistically significant in Model 2, the result should be interpreted with caution, since the large I^2 value in Models 1 and 4 suggest that the random-effects model is preferable because of the large share of variance explained by study heterogeneity. Model 4 indicates that intergovernmental grants are positively associated with the partial correlations, as are total expenditures, although the latter is not statistically significant. That a highly aggregated category such as total expenditures produces some of the strongest evidence for political budget cycles runs contrary to the theoretical arguments about how disaggregated categories of spending (e.g., administrative, capital, or current expenditures) should be the most likely to reveal a political budget cycle effect (Vergne 2009; Brender and Drazen 2013).

2.7.7 Revenues

The previous section examined evidence of political budget cycles in public spending in the context of a MRA. Do similar patterns emerge in public revenues? Table 2.3 uses the same estimation strategy as before, but examines the 243 studies in which revenue was the dependent variable. Interestingly, neither the impact factor nor the standard error of the partial correlation produced posterior inclusion probabilities large enough to merit inclusion in Model 8. This result suggests

that publication bias may be less of a problem for studies that examine revenues rather than expenditures. While temporal aggregation and the level of analysis influence governmental expenditures, only quarterly aggregation appears to be an important predictor for revenues (albeit not statistically significant) in Model 8. All else equal, studies using quarterly data tend to find that revenues *increase* prior to Election Day. The only regional dummies that reveal a posterior inclusion probability exceeding 0.10 are studies examining the countries of Eastern Europe and the former Soviet Union. Since the coefficient is positive, it appears that both revenues and expenditures increase in these regions during elections, all else equal.

Unlike expenditures, few moderating variables have significant effects on the partial correlations for revenues. GDP growth is positively signed in Model 8, suggesting that it may be necessary to include that variable in studies of revenue cycles. Controlling for revenue and unemployment, both of which have negative signs (i.e., including these variables tends to intensify the decline in revenues during elections), may also be important. The finding that unemployment matters is similar to the findings for expenditures in Table 2.2. Taken together, these results suggest that incumbents manipulate budget balances—either by increasing expenditures or reducing revenues—only when the unemployment rate is high.

The results for the methodology and election variables in Table 2.3 have some similarities and some differences with respect to the expenditure results. For both, I find that dynamics are particularly important to incorporate into models of political budget cycles by entering a lagged dependent variable. However, less fine-grained election indicators, such as simple election year dummies, are associated with more negative partial correlations for both the revenue and expenditure results. For revenues, the negative coefficient indicates that revenues are reduced more around elections when a coarse electoral window is defined. Last, in both models of revenues and expenditures, differences seem to exist between elections that are fixed and those that are called early.

A few study characteristics seem to affect the size of the partial correlations for revenues. Both the total number of models in a study and the average number of citations received per year largely are significant across all models, although the magnitude of this effect is small and switches signs

Table 2.3: Explaining differences in partial correlations of revenues

	Model 5 RE		Model 6 FE		Model 7 BMA		Model 8 RE	
Data characteristics								
Standard error	1.273	(1.065)	1.273*	(0.676)	-0.004	(0.067)		
OECD	0.018	(0.041)	0.018	(0.025)	-0.000	(0.002)		
Latin America	-0.004	(0.050)	-0.004	(0.031)	-0.000	(0.002)		
Asia	-0.048	(0.119)	-0.048	(0.084)	0.001	(0.008)		
Sub-Saharan Africa	0.031	(0.146)	0.031	(0.109)	0.003	(0.015)		
E. Europe and Fmr. USSR	0.053	(0.084)	0.053	(0.066)	*0.010*	(0.027)	0.027	(0.039)
Average year	0.003**	(0.001)	0.003***	(0.001)	0.000	(0.000)		
Quarterly aggregation	0.194	(0.136)	0.194**	(0.089)	*0.037*	(0.034)	0.074	(0.059)
Monthly aggregation	-0.455	(0.325)	-0.455**	(0.231)	0.001	(0.009)		
Single country	0.015	(0.089)	0.015	(0.053)	0.000	(0.006)		
Municipal aggregation	-0.032	(0.134)	-0.032	(0.075)	-0.002	(0.012)		
State aggregation	-0.154	(0.137)	-0.154	(0.095)	-0.001	(0.013)		
Moderating variables								
Coalition	0.048	(0.104)	0.048	(0.082)	-0.000	(0.006)		
Debt	0.040	(0.192)	0.040	(0.148)	0.000	(0.009)		
GDP	-0.045	(0.028)	-0.045**	(0.021)	-0.000	(0.003)		
GDP growth	-0.165	(0.105)	-0.165**	(0.078)	*0.006*	(0.018)	0.056	(0.051)
Expenditures (control)	0.083	(0.073)	0.083	(0.053)	-0.000	(0.004)		
Ideology	0.001	(0.032)	0.001	(0.024)	-0.001	(0.006)		
Revenues (control)	-0.445***	(0.164)	-0.445***	(0.119)	-0.003	(0.021)		
Transfers (control)	0.050	(0.072)	0.050	(0.038)	0.001	(0.008)		
Unemployment	-0.272**	(0.124)	-0.272***	(0.089)	-0.001	(0.011)		
Methodology								
Unit fixed-effects	0.014	(0.015)	0.014	(0.011)	-0.000	(0.002)		
Lagged dep. var.	-0.067	(0.051)	-0.067**	(0.031)	-0.030	(0.034)	-0.052	(0.034)
OLS PCSE GLS	0.026	(0.047)	0.026	(0.017)	0.000	(0.003)		
Election variables								
Election dummy	-0.021	(0.049)	-0.021	(0.029)	*-0.015*	(0.024)	-0.012	(0.019)
Franzese	0.172**	(0.085)	0.172***	(0.055)	0.001	(0.006)		
Election pre-determined	-0.023	(0.052)	-0.023	(0.031)	-0.000	(0.004)		
Election early	0.013	(0.052)	0.013	(0.031)	*0.017*	(0.024)	0.039***	(0.014)
Election$_{t+1}$	0.347***	(0.124)	0.347***	(0.088)	0.000	(0.005)		
Election$_{t-1}$	-0.153*	(0.084)	-0.153***	(0.054)	0.000	(0.003)		
Study characteristics								
Total models	0.001*	(0.000)	0.001**	(0.000)	0.000	(0.000)	-0.000	(0.000)
Cites per year	-0.004	(0.006)	-0.004	(0.004)	*0.001*	(0.002)	0.001	(0.003)
Impact factor	0.083*	(0.050)	0.083**	(0.034)	0.000	(0.002)		
Revenue variables								
Tax revenue	-0.006	(0.025)	-0.006	(0.007)	-0.000	(0.003)		
Total revenues	-0.029	(0.031)	-0.029***	(0.009)	-0.001	(0.008)		
Constant	-5.418**	(2.455)	-5.418***	(1.792)			0.012	(0.026)
R^2	0.47						0.29	
I^2	0.59						0.69	

Notes: Dependent variable is the partial correlation of study-model ij. Coefficients with standard errors in parentheses for Models 5, 6, and 8. Posterior means with posterior standard deviations in parentheses for Model 7. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ for frequentist models. Estimates in italics in Model 7 indicate a posterior inclusion probability greater than 0.10. 243 model-study observations for 23 studies for all models, with a total sample size of 245,404. Due to the smaller number of observations, the variables on democracy, presidential, proportional, win margin, deficit, inflation, election half-year were excluded.

when moving to Model 8. The journal's impact factor is positive and significant in Models 5 and 6, indicating that higher quality journals (according to impact factor) often find that revenues *increase* during elections. Last, in contrast to public spending, I find no evidence of any systematic differences across the types of dependent variable used in the analysis. These results suggest that, after accounting for other factors, political budget cycles are equally likely in all revenue categories.

2.7.8 Fiscal balance

As shown in Figure 2.2, studies specifying fiscal surplus as a dependent variable tended to find some of the strongest evidence of political budget cycles. To see that result remains robust to the addition of study and data characteristics, Table 2.4 shows the results from the 234 such studies in the sample that use fiscal surplus as a dependent variable. I find few differences across regions, and, moreover, temporal aggregation does not appear to influence the results. In contrast, while single-country analyses tend to find evidence of a "reverse" political budget cycle (the positive coefficent indicates that fiscal surpluses rise during elections), Table 2.4 suggests that municipalities tend to run budget deficits during elections.

Although a number of moderating variables are statistically significant across Models 9 and 10, none had high enough posterior inclusion probabilities in Model 11 to be entered into Model 12. Nor did any of the election variables or study characteristics. In contrast, the coefficient on lagged dependent variables is positive and statistically significant across all models, indicating that studies accounting for dynamics find evidence of less deficit spending during elections. All of this suggests that relatively few differences across studies help explain the underlying political budget cycle effect of fiscal surpluses.

2.8 Discussion

The findings in this chapter have substantial implications for the political budget cycle literature. I emphasize a number of key points. First, I disaggregated the dependent variables common in these studies into four categories: expenditures, revenues, fiscal balance, and public debt, as well as a number of sub-categories. As expected, I find that expenditures and debt increase during elections,

Table 2.4: Explaining differences in partial correlations of fiscal balance

	Model 9 RE		Model 10 FE		Model 11 BMA		Model 12 RE	
Data characteristics								
Standard error	-1.146***	(0.381)	-1.146***	(0.367)	-0.005	(0.057)		
OECD	0.000	(0.009)	0.000	(0.008)	-0.000	(0.001)		
Latin America	0.001	(0.010)	0.001	(0.009)	-0.000	(0.001)		
Asia	-0.080***	(0.026)	-0.080***	(0.025)	0.000	(0.001)		
Sub-Saharan Africa	0.006	(0.024)	0.006	(0.023)	0.000	(0.001)		
E. Europe and Fmr. USSR	0.045**	(0.021)	0.045**	(0.020)	0.000	(0.001)		
Average year	-0.001**	(0.000)	-0.001**	(0.000)	0.000	(0.000)		
Quarterly aggregation	-0.021	(0.014)	-0.021	(0.013)	0.000	(0.001)		
Monthly aggregation	-0.140*	(0.081)	-0.140*	(0.075)	-0.000	(0.008)		
Single country	0.176***	(0.038)	0.176***	(0.037)	*0.024*	(0.048)	0.068**	(0.029)
Municipal aggregation	-0.311***	(0.044)	-0.311***	(0.042)	*-0.041*	(0.077)	-0.027	(0.041)
State aggregation	-0.121**	(0.049)	-0.121***	(0.047)	*0.070*	(0.073)	0.012	(0.033)
Moderating variables								
Democracy	0.087*	(0.051)	0.087*	(0.048)	0.000	(0.007)		
Coalition	-0.056	(0.044)	-0.056	(0.043)	-0.001	(0.009)		
Debt	-0.005	(0.020)	-0.005	(0.020)	-0.000	(0.005)		
GDP	-0.001	(0.017)	-0.001	(0.017)	-0.000	(0.002)		
GDP growth	0.030**	(0.012)	0.030**	(0.012)	0.000	(0.002)		
Ideology	0.091***	(0.018)	0.091***	(0.017)	0.004	(0.015)		
Inflation	0.020	(0.020)	0.020	(0.018)	-0.001	(0.008)		
Presidential	-0.035	(0.038)	-0.035	(0.035)	-0.000	(0.006)		
Revenues (control)	-0.041*	(0.023)	-0.041*	(0.022)	-0.001	(0.009)		
Transfers (control)	0.088***	(0.027)	0.088***	(0.020)	0.000	(0.004)		
Unemployment	-0.057*	(0.031)	-0.057*	(0.029)	-0.006	(0.025)		
Win margin	0.095**	(0.041)	0.095**	(0.040)	0.001	(0.011)		
Methodology								
Unit fixed-effects	0.007	(0.017)	0.007	(0.016)	-0.000	(0.002)		
Lagged dep. var.	0.140***	(0.038)	0.140***	(0.036)	*0.220*	(0.094)	0.192***	(0.025)
OLS PCSE GLS	0.009	(0.021)	0.009	(0.019)	-0.001	(0.005)		
Election variables								
Election dummy	0.018*	(0.009)	0.018**	(0.008)	-0.000	(0.001)		
Franzese	-0.017	(0.021)	-0.017	(0.020)	-0.000	(0.003)		
Elec. half-yr	-0.054**	(0.023)	-0.054***	(0.020)	-0.000	(0.005)		
Election pre-determined	0.070***	(0.020)	0.070***	(0.018)	0.002	(0.011)		
Election early	0.022	(0.020)	0.022	(0.018)	-0.000	(0.001)		
Election_{t+1}	0.016	(0.010)	0.016*	(0.009)	0.000	(0.002)		
Election_{t-1}	-0.002	(0.013)	-0.002	(0.011)	-0.000	(0.001)		
Study characteristics								
Total models	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)		
Cites per year	0.003**	(0.001)	0.003***	(0.001)	0.000	(0.000)		
Impact factor	0.002	(0.011)	0.002	(0.010)	-0.000	(0.001)		
Constant	1.640*	(0.970)	1.640*	(0.872)			-0.271***	(0.025)
R^2	0.89						0.29	
I^2	0.11						0.46	

Notes: Dependent variable is the partial correlation of study-model ij. Coefficients with standard errors in parentheses for Models 9, 10, and 12. Posterior means with posterior standard deviations in parentheses for Model 11. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ for frequentist models. Estimates in italics in Model 11 indicate a posterior inclusion probability greater than 0.10. 234 model-study observations for 39 studies for all models, with a total sample size of 201,888. Due to the smaller number of observations, the variables on expenditures (control), proportional, and deficit were excluded.

while revenues and fiscal surpluses decline. Based on funnel plots of the weighted partial correlations, as well as the absence of significant variables in the meta-regression analysis, it appears that public budget balance shows the most consistent evidence of a robust political budget cycle; governments consistently run larger deficits around elections.

Using meta-regression analysis, I find that political budget cycles may be more likely to occur in some expenditure categories than in others. Although many scholars have theorized that highly disaggregated "visible" spending categories are most likely to be manipulated, I find instead that total expenditures rise during elections. This evidence suggests that incumbents may be manipulating multiple budgetary line items at once rather than increasing spending in a single category. I also find that grants between tiers of government show some of the strongest political budget cycle effects.

I also find that aggregating data annually rather than quarterly tends to reduce the sizes of observed political budget cycles for expenditures, but less so for revenues and fiscal surpluses. This finding complements existing work on how raising the level of temporal aggregation may hide an important dimension of variation (Streb, Lema and Garofalo 2012; Klomp and De Haan 2013*b*). In addition, this study was the first to test how the level of government at which elections are held affects the evidence for political budget cycles. Municipal- and national-level elections appear to be associated with increases in public spending and budget deficits. In contrast, election-based cycles in revenues are not more prevalent at sub-national levels of government.

The results indicate that political budget cycles are present in a variety of contexts. Continental dummies largely are inconsequential throughout much of the meta-regression analysis, and factors thought previously to condition the budget cycle, such as electoral competitiveness and government ideology, have negligible effects in the MRA reported herein. Neither were most methodological choices, or decisions about coding the electoral window. Several important exceptions to that conclusion emerged, however. First, it appears that specifying dynamics is important; the significance of a lagged dependent variable in the meta-regression results suggests the need to account for budgetary incrementalism. Second, democracy and economic development appear to be two important

moderating variables that have conditioning effects on political budget cycles, as evidenced by the significant coefficients in the meta-regression, and by previous studies (e.g., Brender and Drazen 2005). In addition, governmental revenues and expenditures, when entered as control variables, tend to affect the size of the partial correlations. In addition, building on Streb, Lema and Garofalo (2012), I find that more fine-grained election coding is associated with stronger political budget cycles, and that controlling both for pre- and post-election periods may be necessary, at least for public spending.

Meta-analyses can also test for publication bias. There is evidence that many bodies of literature in the social sciences suffer from that bias (Stanley and Doucouliagos 2012). To see if this is true for political budget cycles, I performed a series of simple tests to explore whether the size of the overall political budget cycle effect remains robust to various measures of article and journal quality, as measured by a journal's impact factor.[10] To be brief, the results are not reported here but are available in the appendix to this chapter. I find evidence that publication bias exists in the literature. However, the existence of political cycles across the four budget categories remains robust after accounting for such bias. I also find that budgetary surpluses remain the most manipulated budget category during elections.

2.9 Conclusion

This chapter was motivated by the importance of addressing the conflicting theories, literature reviews, and inconclusive empirical results on political budget cycles. By combining all available studies over the last 15 years, a meta-analytic approach offered an ideal way to synthesize our knowledge of this topic. The meta-analysis herein does not provide conclusive evidence that political budget cycles exist in every context. Factors such as the quality of government institutions and fiscal transparency could not by analyzed in this chapter. Moreover, exploring the magnitudes of conditional relationships (interactions) is difficult when using meta-regression analysis. Still, this chapter goes beyond qualitative literature reviews by summarizing and evaluating the current state

[10]These are a regression of journal quality on precision, the precision-effect/funnel asymmetry (PET-FAT) test, and the precision effect estimate with standard error (PEESE).

of the empirical literature, as well as by identifying the circumstances under which we are more or less likely to observe political budget cycles. Rather than relying on a single dataset, by aggregating multiple study results I was able to identify the extent to which particular study-, methodological-, and data-specific choices affect the evidence for political budget cycles. I investigated these differences across four different types of commonly studied variables in a number of ways, including testing for publication bias, using Bayesian model averaging, and calculating and plotting the sizes of the overall effects across budgetary subcategories.

Several important conclusions were reached. First, the strongest evidence for political budget cycles appears to be for public budget balance (the "fiscal stance"). Second, the lack of evidence in disaggregated expenditure categories suggests either that no effect exists, or that scholars have not yet pinned down a mediating variable that explains why these categories are manipulated only at certain times. Third, while the results are robust across many types of data and coding decisions, dynamics appear to be an important component to account for across all meta-regression results. Fourth, I find evidence that context matters. While this issue has been discussed widely in the literature, differences in data and methodology have made identifying particularly important contextual elements (or comparing them while controlling for other factors) difficult. The results reported herein also suggest that the level of government at which the election is held, as well as the level of temporal aggregation, may merit further investigation. Last, although publication bias exists in the literature, the size of the overall effect remains robust to this bias.

The purpose of this chapter was to begin reconciling the conflicting state of the political budget cycle literature. Results from the meta-analysis suggest that although budget cycles are present in expenditures, revenues, fiscal balance, and debt, the effect is substantively small. The results also indicate the importance of accounting for context-conditional situations as well as controlling for study- and data-specific factors when probing the robustness of future empirical studies. Scholars of political economy should take note since meta-regression analysis suggests the necessity of making cross-study comparisons more carefully. Responding to a recent question posed by Klomp

and De Haan (2013*b*, p. 329), "do political budget cycles really exist?"—the answer appears to be yes, but they are small and depend on a number of factors that must be taken into account.

3. JUST IN TIME: POLITICAL POLICY CYCLES OF LAND REFORM

3.1 Introduction

For parties in government, elections represent a period of uncertainty. To reduce this uncertainty, incumbents may create a variety of economic and distributional distortions around elections in order to remain in office. For instance, they may increase total spending, run budget deficits, or use targeted forms of distribution to specific voter groups. These are known broadly as political cycles. With few exceptions, the extant literature has focused only on the manipulation of existing policies. While fluctuations in expenditures and revenues around elections are important, analyses of the strategic passage of new policies remain understudied in the literature on political cycles.

In this chapter I argue that political parties, conscious of the need to win support among large segments of the population, will time the passage of redistributive policies in order to maximize their support. Although policies do not consist of incremental changes like budgets, I find consistent evidence that certain policies are passed just before elections. Moreover, this process repeats itself over time. I argue that this is because these policies are an important tool used to win over voters. While some studies have examined electoral cycles in various non-budgetary areas such as government contracts (DeRouen and Heo 2000), active labor market policies (Mechtel and Potrafke 2013), social pacts (Ahlquist 2010), and artists employed in public theaters and orchestras (Tepe and Vanhuysse 2014), I focus on the timed policy passage of redistributive policies.

This chapter addresses two related lines of literature: distributive politics and political cycles. Scholars of distributive politics primarily focus on the distributive consequences of elections, and less on advantageous timing of policies. Likewise, those who study political cycles address mostly macro-level manipulation of existing fiscal policies, rather than examining specific policies designed to benefit targeted constituencies. Drawing off work emphasizing how governments gain electoral support through increasing spending to "visible" budgetary categories, I argue it is not necessarily the actual rewards—but the promise of future rewards—that has been overlooked in

the literature. Policy passage is an ideal form of manipulation, since it provides a clear signal to voters of promised future benefits, while remaining less costly to governments than a strategy of altering existing policies.

I test for political policy cycles in two ways. First, I examine the passage of state-level land reform legislation in India. Following independence, the authority to pass land reform was devolved to states. Rather than envisioning land reform as a public good, I argue that certain groups clearly benefit more than others; primarily the large number of landless and poor working farmers, a valuable voting bloc in India. Thus, promises of land reform are a clear signal meant to attract a substantial number of votes from the middle and lower classes. Second, I use historical survey data from the Indian National Election Study to examine whether the salience of land issues decreased over time, and if this decrease can be attributed to land reform passage.

Results suggest that policies such as land reform are indeed passed before an election. This result remains robust to a variety of econometric specifications as well as the inclusion of additional control variables, providing support for the theory that policies are passed strategically in order to win votes. Moreover, the results from the survey show that voters consistently view land reform as a salient issue, regardless of previous policy enactments. Taken together, these results indicate that land reform policy is a signal to voters that politicians use to their advantage.

In the following section I briefly summarize the relevant literature. I then discuss political policy cycles in greater detail. I test my theoretical expectations by conducting an empirical analysis using land reform policy at the state-level. Tests of the continued saliency of land reform using survey-level evidence follow. Finally, I discuss these findings and conclude with broader implications.

3.2 The determinants of political budget cycles

A large body of literature focuses on the factors that affect political budget cycles. For instance, the flexibility of election timing may determine if budget cycles are necessary (Kayser 2005). Another factor is the length of time a country has been a democracy (Gonzalez 2002); without

a long history of elections, voters are likely to be fooled through manipulation since they have little prior information on how a competent incumbent should behave (Brender and Drazen, 2005; Barberia and Avelino, 2011). In addition, institutions are typically less independent in emerging democracies, leaving them vulnerable to political capture.

As an election nears, governments are likely to create budget cycles in order to sway voters by bending the cyclical line in their favor through fiscal or monetary manipulation (Block, 2002). Equally important is timing; policies must be implemented at a time in which the party in government can take credit for it. In order for such implementation to be effective, policies must be passed close enough to an election that they are still fresh in the minds of voters. Although the focus of many of these theories has been on timing, the beneficiaries of particular strategies has been underemphasized in the literature.

While scholars of political cycles have written extensively about the causes of fiscal changes around elections, scholars of distributive politics have focused on targeted spending designed to win the support of particular groups. Dixit and Londregan (1996) propose that instead of providing benefits to core supporters, parties in a two-party system will compete for swing voters by allocating redistributive policies towards them (see also Kwon, 2005; Stokes, 2005). This model contrasts with the core-voter theory put forth by Cox and McCubbins (1986) and others (Calvo and Murillo, 2004; Nichter, 2008). In a compromise between these two competing theoretical expectations, Albertus (2012) finds that governments employ a mix of short-term rewards for their core voters while giving more permanent funds to swing supporters in order to build clientelistic relationships.

Targeted distribution may also be strategically timed to coincide with elections (Franzese, 2002; Franzese and Jusko, 2006). For instance, Cole (2009) finds that agricultural lending to Indian farmers increases between 5 and 10 percent during an election year. Drazen and Eslava (2010) find that targeted expenditures, such as spending on roads and infrastructure, significantly increase during election years in Colombia in order to gain voter support. In other instances, more programmatic policies may be enacted to benefit entire constituency types. In an example of broad-based distribution, Sáez and Sinha (2010) find that Indian states increase spending on public goods such

as education and health prior to an election. Overall however, using a meta-analysis of political budget cycles, Philips (2016) finds weak evidence that any particular category of expenditures is routinely increased before elections.

Party ideology is also an important component of distributive policies. However, the extent to which ideology applies to distributive spending appears to be conditional on whether parties spend opportunistically or based on partisan considerations. The literature suggests that they do both. On the one hand, Sáez and Sinha (2010) hypothesize that supporter composition influences the ideological spending preferences of Indian political parties. Yet they find almost no significant ideological effect across parties, evidence that they are opportunistic; parties spend in any budget category necessary to ensure reelection. In contrast, the investigation of pre-election spending in Portuguese municipalities by Veiga and Veiga (2007) suggests that parties of the left tend to spend more than right-wing parties in the year of and the year preceding an election. Therefore, ideology may affect not only the type, but also the size of spending before elections.

Finally, the intensity of competition between political parties may determine whether distributive benefits are directed towards core supporters or the population more broadly (Schultz, 1995; Chhibber and Nooruddin, 2004; Cole, 2009; Aidt, Veiga and Veiga, 2011). Parties will reward core supporters in less competitive regions, yet reward swing voters as competition increases, as long as core supporters remain loyal even without an increase in benefits. The effective number of political parties competing in elections also affects the intensity of competition. As the number of electorally viable parties increase, pre-electoral policy passage is more likely since uncertainty (especially common in developing democracies) tends to lead towards short-term, rather than long-term, policy strategies (Lupu and Riedl, 2013).

3.3 A theory of political policy cycles

Despite the substantial literature on both distributive policies and political budget cycles, there is relatively little research linking the two together. Although existing policies may change during elections, does the passage of distributive policies become more likely as well? In other words, is

it possible for the government to gain the support of voters through mechanisms other than fiscal spending before an election? A few scholars have found that non-budgetary policies can be used as signals to voters. For example, Tepe and Vanhuysse (2014) find that more artists are hired for public theaters and orchestras in Germany around elections. DeRouen and Heo (2000) conclude that US defense contracts are more numerous before elections. Mechtel and Potrafke (2013) find that job-creation schemes are passed before elections in Germany, while Ahlquist (2010) finds that social pacts in developed economies are more likely to occur before elections.

Like the scholars above, I contend that the *passage* of certain policies—not just changes in spending—constitutes a signal to voters of promised returns in the future, should they re-elect the incumbent. In effect, a contract between promising to deliver the policy and the assurance of a vote is struck. For voters, it is the visibility component that matters most (Akhmedov and Zhuravskaya 2004; Veiga and Veiga 2007). Just as with an increase in spending, policy passage changes a voter's evaluation of the incumbent. Policy passage contains both retrospective and prospective components. Policies passed before an election stay fresh in the minds of voters, increasing their likelihood of a positive retrospective evaluation. Yet unless the policy is implemented quickly, there is also a prospective component; a need to re-elect the incumbent so that the policy may be fully implemented. Both components rely on the assumption that the policy is visible to voters.

In addition to visibility, the policy must be easily attributed to the current government. It must also be cost-effective (feasible to pass and implement within certain budgetary constraints), and must be a salient issue for large segments of the population. While existing research has emphasized visibility mechanisms, it has not extended to the context of the passage of policies aimed at maximizing electoral support. Although policy passage may not be the only timed opportunistic tools governments use, it may serve as a substitute for manipulating the budget. This is due to policy salience. Below I show how land reform is one example of an ideal policy instrument.

3.3.1 Land reform in India: A signal to voters?

> *Giving land is like parting with your soul or body. People are more attached to land than anything else.* —Shri Uma Shankar Dikshit, Governor of Karnataka[1]

To test for political policy cycles, I examine the passage of Indian land reform from 1957 to 1992 in 15 major Indian states. There are several reasons why a single-country analysis is an ideal research design. Land reform is a polarizing issue in many countries, especially India. In principal, they were designed with three goals in mind. First, to rectify historical disparities between groups. Second, to alleviate poverty, since India is home to a full third of the world's poor. Third, to reallocate land in order to use it more productively, and in turn boost growth. Advocates argue that reform leads to increased land security, property values, land rights, and even social capital (Deininger et al. 2003; Teofilo and Garcia 2003). After India gained independence in 1947, each state has been tasked with land reform policy and implementation. Any pressure from the central government for land reforms is largely an advisory role.[2] Moreover, turnout for state elections in India is much higher than for the national election, sometimes approaching 80 percent (Gill 1998), and evidence suggests that there is a strong sub-national identity in India (Singh 2015). Finally, in the sample used in this study, political activity below the level of state government was largely inactive (Banerjee and Somanathan, 2007).

The land system in India in the 20^{th} century can trace its origins to colonial British rule, although a landlord-tenant system was almost certainly in place even before the British. Largely relying on property levies for revenue, the British devised a land system comprised of three primary categories (Banerjee and Iyer, 2005). The *zamindari* systems were traditional landlord-tenant based structures, in which the landholder was the intermediary between tenants and the British. Zamindaris were typically more extractive than other systems since landlords were able to keep any

[1] Spoken at the All India Conference Committee on Land Reforms, August 18 and 19, 1976. Quoted from Zaidi (1985, p. 117).

[2] As Rural Development Minister Jairam Ramesh said of the central government's hands-off role in forming land reform policy, "[the states] are culprit of not coming out with land reforms." http://timesofindia.indiatimes.com/india/Jairam-Ramesh-signs-pact-averts-siege/articleshow/16775475.cms?referral=PM. Accessed 03/03/2014.

windfall revenues after paying a fixed amount to the colonial government. These existed primarily in the northern and eastern regions. The second category were village-centered *mahalwari* systems, in which the entire village collectively paid a tax. These were established in what is now Punjab and Uttar Pradesh. The third category was comprised of individual farmer systems, or *raiyatwari*. These were established along the eastern and western coasts, and in parts of Assam. Raiyatwari generally had the most developed property rights, and in some cases even legal titles to land. Taxes were paid directly to the state. Still, a significant portion of the population lacked land. Moreover, land under the raiyatwari system was leased to tenants on occasion. Zamindari and raiyatwari systems were the most common land structure in India, comprising some 95 percent of the total (Hanstad et al., 2009).

Land reforms are best classified into four types.[3] First, tenancy reforms were enacted, "to regulate tenancy contracts both via registration and stipulation of contractual terms...as well as attempts to abolish tenancy and transfer ownership to tenants" (Besley & Burgess, 2000, p. 392). Second, certain land reforms dissolved the intermediary landholders that had been crucial to the agricultural system in British India. Much of these estate-type systems were prevalent in the East, in what later became Bengal-state. Third, ceilings on landholdings were enacted to limit the amount of land a person, or in some cases families, could own. Fourth, some landholdings were allowed to be consolidated, in a move to allow greater agricultural efficiency. As shown in Figure 3.1b, although tennancy reforms were the most popular type throughout much of the 20^{th} century, there is substantial variation over time in all reform categories.

Saliency of land reform, and political pressure to implement reforms, has existed even before Indian independence. Since landholders often had tightly aligned interests with the colonial government, by independence they had become deeply unpopular (Hanstad et al., 2009). Taking advantage of this growing political realization, the Communist Party of India incorporated a motto of "land to the tiller" into its party platform in 1948. This was followed by the Congress Party creating an Agrarian Reforms Committee a year later (Joshi, 1975, p. 38). Even into the 1970s,

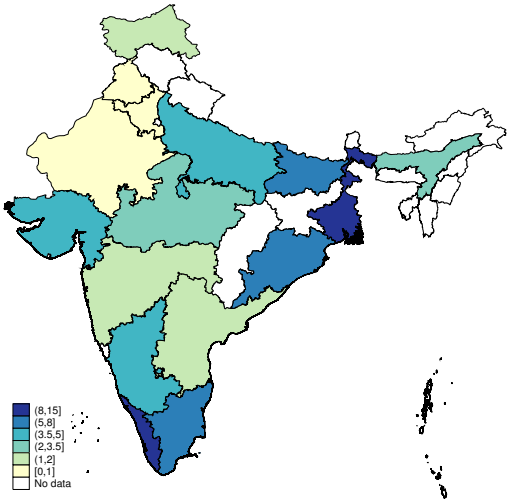
[3]This slightly differs from Hanstad et al. (2009) who add an additional category of transfers of government-owned 'wasteland' to landless individuals.

mass campaigns for agrarian reform took place such as Operation Barga in West Bengal in 1978, which tried to create a stronger legal basis for tenants (Bandyopadhyay, 1986). However, as shown in Figure 3.1a, which depicts the number of land reforms enacted in the 15 major states in India from 1957 to 1992, the fact that almost every state enacted at least some type of reform suggests that land reform is not simply an ideological policy implemented by the left, but rather an opportunistic policy due to its sheer popularity. As one prominent Assamese legislator pointed out, "we are to look to the interests of 96% of the cultivators of land and not to the 4% of the landlords or capitalists or big people who are holding lands for generations depriving these cultivators for generations of their dues." (Borgohain, 1992, p. 49).

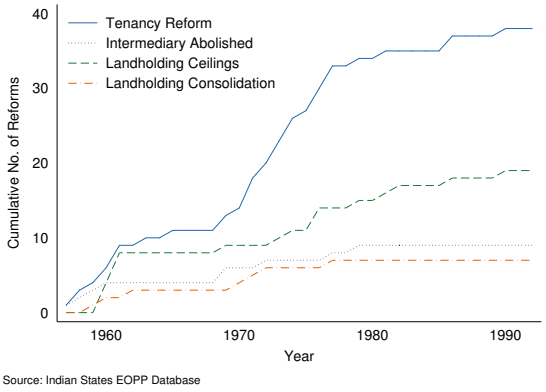
In addition to the continued saliency described above, many reforms differed in what they hoped to accomplish. In some states tenancy reforms enabled land to be passed down by families, while in other cases legal and administrative hinderances greatly limited the effectiveness of land reform (Bardhan & Mookherjee, 2010). Policies such as ceilings on landholdings varied between states as well; in the 1960s Assam had a ceiling of 20 hectares, while Rajasthan had a maximum of 136 hectares that any single landholder could own (Das, 1995). Some reforms permitted families to have twice the ceiling on landholdings if they had five or more members in their family. Others offered an exemption, "of tea, coffee, rubber, cardamom and cocoa plantation[s] and of lands held by religious and charitable institutions beyond normal ceiling limits." (Bandyopadhyay, 1986, p. A-51) In some areas, landless residents even received free brick houses provided by the government. Although reforms differed in what they hoped to accomplish, they all tried to implement at least some form of redistribution in the interests of the poor (Das, 1995). Thus, land reforms serve as a clear signal to specific voting segments of the population, although they may vary slightly based on the particular context.

In regards to poverty, land reforms were an effective policy instrument implemented at the state level since they altered the terms and structure of property contracts (Besley and Burgess, 2000). Agricultural reforms such as landholding ceilings benefitted both landless tenants and the rural poor, which make up a sizable constituency in India. They were also popular politically, since

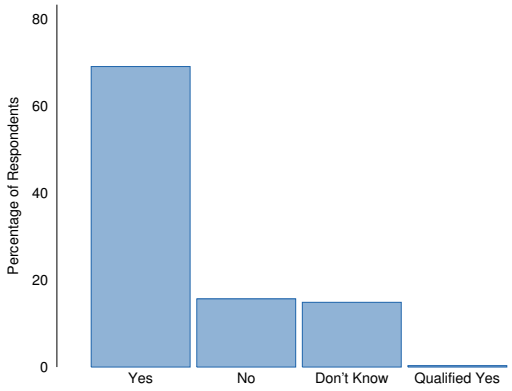
Figure 3.1: Land reform is dispersed over time and space, and is highly salient



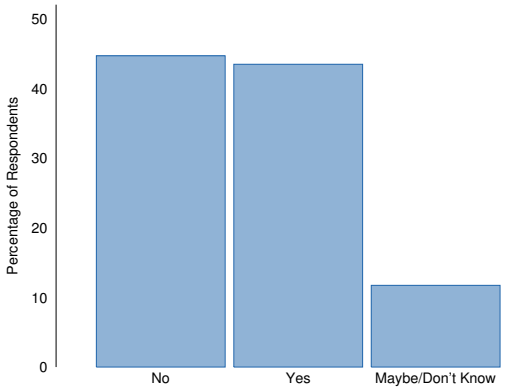
(a) Spatial distribution of reforms



(b) Types of reforms over time



(c) "Should there be a ceiling on property?"



(d) "Do you approve of land grabs?"

public opinion was strongly in favor of limiting the power of intermediary zamandari landholders. In just one example of the visibility of reforms, Bardhan and Mookerjee document how, "A mass-mobilization campaign involving party leaders, local activists, and the administrators was mounted to identify landowners owning more land than the ceiling, or leasing to sharecroppers." (2010, p. 1576). This suggests that reforms were highly visible.

While analyzing the impact of land reforms is beyond the scope of this chapter, evidence suggests that they have had both beneficial effects (Besley and Burgess, 2000; Banerjee et al., 2001) as well as mixed outcomes (Bandyopadhyay, 1986; Borgohain, 1992). For instance, ceilings on land ownership have not improved outcomes for the most part (Deininger et al. 2003), and despite the increased participation of the poor in Indian politics, economic inequality tends to persist (Kohli 1980).

Despite the mixed literature on the economic consequences, large numbers of land-poor tenants and farmers, as well as discriminated castes, directly benefitted from the passage of redistributive policies (Bandyopadhyay, 1986; Banerjee, Gertler, and Ghatak, 2002; Bardhan and Mookherjee, 2010). Issues of land were salient to a large number of poor voters, making agrarian topics likely to rise to the top of the political agenda. As evidence of this, Figures 3.1c and 3.1d show the results of two questions asked on the 1971 Indian National Election Survey (Eldersveld, Ahmed, and Marvick, 2011). The first asks if there should be a ceiling on property ownership.[4] Nearly 70 percent of respondents thought that there should be some limit on the amount of land an individual can own. The second question asks if the respondent approves of land grabs—or the taking of property by those who have little to no land themselves.[5] This answer is more split but still highly salient, with about 44 percent approving of land grabs compared with around 45 percent disapproving.

In addition to voter demand, land reforms are ideal policy cycle instruments for governments due to their cost-effectiveness. Reforms are in-kind rather than cash. They impose little cost

[4] "Some people say that the government should pass legislation so that people are not allowed to own and possess large amounts of land and property. Others say that people should be allowed to own as much land and property as they can acquire—what would you say?"

[5] "Some political leaders and parties have been advocating that poor people with no land and property should occupy a part of land and property of those who have a large amount of land and property. Do you approve of this or do you disapprove?"

on the government since revenues to pay for reforms do not have to be generated through taxes or borrowing—instead, the burden falls mostly on landholding elites.[6] This policy is ideal since it benefits supporters and circumvents fixed budget constraints. Freed of this budget constraint, governments are likely to implement land reforms in areas with large amounts of poor voters who own little land, and are thus most susceptible to these promises. Moreover, this may be far more efficient than the cost of large redistributive handouts to win voters.

In India, mandated elections for the state assembly occur every five years.[7] Therefore, policy passage should consistently occur just before an election, and reform should be less likely after an election since the need to win over voters is small.

$\mathbf{H}_1$: Reforms are more likely to occur before an election than after

Since enacting policies such as land reform may be difficult in the state assemblies, and since state elections are typically held early in the year, I expect that governments will aim for the policy to be passed in both the election year as well as the year before the election.

As Chhibber and Nooruddin (2004) emphasize in the Indian context, based on the level of competition, parties in Vidhan Sabha elections face different incentives in their decision to provide public goods. In multiparty systems there may be less *total* voters to buy off, but the margin of victory is much smaller. This makes reform more important in order to win votes. In systems with two or more competitive parties, governments have an incentive to enact land reform in order to beat their opponent in elections. By contrast, in states where a single party is consistently in government, there tends to be less competition in elections, and parties should be less likely to push for reforms, regardless of ideology. Bardhan and Mookherjee (2010) confirm this view in their discussion of competition and leftist party dominance in West Bengal, "once the Left obtains a majority, further increases in its share of local government seats will *decrease* the extent of land

[6] In other cases such as Venezuela, public lands were used, and elites were bought off through payments from oil revenues or encouraged to move to urban centers (Albertus, 2012).

[7] Earlier elections are possible; for instance if a coalition collapses or if "President's Rule" is imposed. I account for this in the robustness section. The state of Jammu and Kashmir has elections every six years according to its constitution.

reforms implemented" (emphasis theirs, p. 1574). Therefore, I hypothesize that land reforms will be more likely during elections where two or more parties are engaging in electoral competition.

$\mathbf{H}_2$: Multi-party competition makes land reform more likely, relative to single-party competition

Ideology is another factor that may determine whether parties implement policy opportunistically or for partisan reasons. Although party ideology could affect the desired extent of land reform, with few exceptions, parties on either side of the ideological spectrum have advocated for reform.[8] For instance, the centrist, "[Indian National] Congress Party advocated the abolition of intermediaries while the growing left and radical movement emphasized the rights of the subtenants and the actual tillers." (Eashvaraiah, 1993, p. 159). While post-election implementation effectiveness of policies may be conditional on ideology, the literature suggests that the passage of reforms is not.[9] Therefore, rather than policies that are implemented based on ideology, I expect that opportunistic parties of all ideologies will take advantage of increased voter support through the passage of reforms. I generate the following hypotheses regarding specific ideologies in India: leftist governments such as the Communist Party of India and the Communist Party of India-Marxist will be most likely to implement land reform. Centrist parties such as the Indian National Congress (INC) have, "traditionally represented the interests of big landowners in rural areas," and so are less likely to implement reform (Bardhan and Mookherjee, 2010, p. 1573). Although early on the INC gave strong support for land reform, over time they slowly backed away from strong redistributive preferences as they began to rely on the support of constituencies other than the poor (Zaidi 1985). This places the INC somewhere between the left and the right. Right-wing parties are expected to implement even less than the INC.

$\mathbf{H}_3$: Left-leaning governments make land reform more likely relative to centrist governments

$\mathbf{H}_4$: Right-leaning governments make land reform less likely relative to centrist governments

[8] In fact, recent evidence suggests strong elite party strategy is used in India to secure votes (Thachil 2011).

[9] The literature has substantial evidence for signals rather than substance. Eashvaraiah believes the Praja Socialist Party-Congress coalition's 1964 Land Reform Act in the state of Kerala was a, "watered down version of the original bill of 1959 passed by the Communist Government." (1993, p. 124) In addition, the 'de facto' level of reform was often much less than what the statute prescribed; in West Bengal the amount actually taken over by government was much less than the land allotted by law. In some instances, land distributed to individuals by government was often only *half* as much as had been agreed to in the land reform bill (Eashvaraiah, 1993).

3.4 Data and methods

To test the hypotheses above I use the dependent variable, land reform, from Besley and Burgess (2000, 2002, 2004), which is coded 1 if a land reform occurred in state i in year t. It is the most comprehensive dataset on land reforms and inequality in the Indian states. The dummy variable encompasses the following four categories: tenancy reform, dissolving intermediaries, consolidating landholdings, or imposing a landholding ceiling on property owners. In the appendix to this chapter, I test the robustness of my findings by disaggregating the four categories.

One drawback to this analysis is the coding of the dependent variable as dichotomous. This does not consider the intensity of each land reform. Despite this, it is justified for three reasons. First, land reform was highly context-specific, which would make coding difficult. For instance, ceilings on property varied, and in some states there were certain exemptions for particular crops such as tea and cardamom (Das, 1995; Bandyopadhyay, 1986). Second, a dichotomous indicator still captures the underlying casual mechanism. Passing land reforms sends a signal to voters that benefits (through redistribution from the reform) are likely to occur in the future. I parse out this out more in the individual-level analysis below. Third, no good measures exist that proxy for intensity of reform.

For party competition and ideology I draw from Chhibber and Nooruddin's (2004) coding of the Indian states. They differentiate between a low-competition state and multi-party contested state assembly elections. I extended their indicators back ten years, lengthening the range of ideological competition from 1957 to 1992, and expanding the analysis for a total of 15 Indian states over the period.[10] Arguably, in a state dominated by a single party, ideology matters less to voters than in multiparty states where ideology could come into play in determining land reform policies. To capture this I employ dummy variables for a variety of competitive and ideological positions. They account for each position's effect on land reform relative to a two-party competitive Parliament

[10]The states are: Andhra Pradesh, Assam, Bihar, Gujarat, Jammu and Kashmir, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, and West Bengal. Although Jammu and Kashmir may be considered a "special category" due to its unique status enshrined in the constitution, results do not change if it is dropped from the analysis.

with two centrist parties. Although coarse, this measure serves to distinguish between the type of competition (single party, two party, multiparty) and the strain of ideology present in each state over time. The resulting model is the following:

$$Pr(Land\ Reform_{it}) = f(Elections_{it},\ Ideology_{it},\ Competition_{it}) \tag{3.1}$$

where the probability of land reform in a given state-year is a function of $Elections_{i,t+s}$, which are a set of dummy variables equal to one if the state i is holding an election in year t—where $s \in [-1,0]$—one year before or the year of an election (Besley and Burgess 2002). *Ideology* and *Competition* are a set of dummy variables to test the effect of ideology and party competition. I also control for the percentage of citizens in each state who do not own any land (Besley and Burgess, 2000). Land reforms were primarily designed to redistribute the property owned by wealthy holders where inequality was large (Banerjee and Iyer, 2005). In the colonial era, the ability to set any rate of taxation antagonized tenant farmers in zamandari systems. Since land reforms have been implemented to benefit the poor, states with large numbers of poor or landless individuals lacking property should make land reform more likely since they favor redistribution. In addition to inequality, signaling to voters through a broad-coverage policy such as land reform may be easier and less expensive than other, more targeted forms of redistribution. Parties in India have a large incentive to target landless and poor voters. The poor in India vote at a similar rate as their wealthier counterparts (Yadav, 1999). The former chief of India's Election Commission sums up the situation, "the poor, the underclass, the uneducated, the former untouchables tend to vote not less but more than others." (Gill, 1998, p. 166). Poor voters are likely to be swayed by promises of land redistribution since a legacy of inequality has caused the poor to, "support political programs that advocate expropriating the assets of the rich." (Banerjee and Iyer, 2005, p. 1198). More broadly, poor voters across the developing world are among those most susceptible to promises of redistribution and patronage (Remmer, 2007). Therefore, demand for reform in states is likely to be greater if there are more land-poor voters. To address those who may be targets of land reform

by politicians, I add *% Owning No Land*. As the percentage of the landless population rises, there should be a greater demand for redistribution.

All descriptive statistics and data sources are available in the appendix to this chapter. To model the probability of land reform in a given state-year, I use a random effects logit model. However, to alleviate any concerns about unobservable heterogeneity across panels, I also present models with state fixed effects.

Another concern is the potential for temporal heterogeneity. Ignoring such dependence may lead to incorrect inferences (Beck, Katz, and Tucker, 1998; Carter and Signorino, 2010). Binary dependent variables in a cross-sectional time-series context are duration models by definition. So accounting for temporal dependence is crucial. This seems plausible in the case of land reform; over time they should become less likely for two reasons. First, if reforms are effective there should be less need for more of them in the future. Second, if reforms are ineffective, politicians will be less likely to use them as a visibility mechanism before elections, since voters start to heavily discount the signal of promised reforms. To account for duration dependence, I add a cubic spline with four knots instead of time fixed effects, since a Wald test cannot reject the null that the parameters are jointly equal to zero. I investigate the robustness of the findings by including time fixed effects as well as a lowess smoother in the robustness section below.

3.5 Results

Turning to Model 1 in Table 3.1, elections appear to affect the propensity for reform. In the year before an election the parameter estimate is statistically significant at conventional levels, and is in the hypothesized positive direction. The election-year dummy is positive, much smaller in magnitude, and insignificant. This lends support to the hypothesis that governments strategically time policies to occur before an election in order to win votes. Moreover, the coefficient remains statistically significant and exerts a similar effect across both random and fixed effects specifications for Model 1.

Table 3.1: Evidence for political policy cycles

	Model 1				Model 2				Model 3				Model 4			
	RE	FE	Alt.-RE	Alt.-FE	RE	FE	Alt.-RE	Alt.-FE	RE	FE	Alt.-RE	Alt.-FE	RE	FE	Alt.-RE	Alt.-FE
Year Before Election *Dummy*	0.91** (0.37)	0.97*** (0.37)			0.98*** (0.37)	1.02*** (0.37)			0.98*** (0.37)	1.02*** (0.38)			0.92** (0.36)	1.01*** (0.37)		
Election Year *Dummy*	0.03 (0.41)	0.04 (0.41)			0.13 (0.41)	0.11 (0.41)			0.12 (0.41)	0.10 (0.41)			0.14 (0.41)	0.14 (0.41)		
Year Before Election *Monthly Weight*			1.10** (0.48)	1.16** (0.48)			1.18** (0.47)	1.18** (0.48)			1.17** (0.47)	1.18** (0.48)			1.10** (0.47)	1.15** (0.48)
Election Year *Monthly Weight*			0.53 (0.93)	0.60 (0.94)			0.75 (0.95)	0.71 (0.95)			0.76 (0.95)	0.73 (0.96)			0.74 (0.93)	0.80 (0.95)
Multiparty: Left-Center-Right	1.62* (0.87)	1.05 (1.04)	1.65* (0.87)	1.11 (1.04)												
Two-Party: Left-Center	0.98** (0.42)	1.36 (1.62)	0.97** (0.42)	1.35 (1.62)												
Two-Party: Center-Right	-0.37 (0.66)	0.98 (0.93)	-0.35 (0.66)	1.00 (0.92)												
Leftist					1.60** (0.64)	1.29* (0.76)	1.61** (0.63)	1.31* (0.76)	1.50** (0.68)	1.28* (0.77)	1.53** (0.68)	1.30* (0.77)	1.80*** (0.65)	1.36* (0.79)	1.79*** (0.64)	1.37* (0.79)
Congress					0.86* (0.49)	0.88* (0.53)	0.86* (0.49)	0.87 (0.53)	0.86* (0.49)	0.88* (0.53)	0.86* (0.49)	0.87* (0.53)	0.92* (0.49)	1.02* (0.54)	0.92* (0.49)	1.01* (0.54)
Effective Number of Parties									0.05 (0.13)	0.15 (0.16)	0.05 (0.13)	0.14 (0.16)	0.07 (0.14)	0.05 (0.16)	0.07 (0.14)	0.05 (0.16)
% Land Owned by Bottom 50%													-0.07 (0.06)	-0.25 (0.20)	-0.07 (0.06)	-0.24 (0.20)
% Land Owned by Top 10%													0.01 (0.03)	0.05 (0.09)	0.01 (0.03)	0.05 (0.09)
% Owning No Land	0.03* (0.02)	0.08*** (0.03)	0.03* (0.02)	0.07*** (0.03)	0.04** (0.02)	0.07*** (0.03)	0.04** (0.02)	0.07*** (0.03)	0.04** (0.02)	0.08*** (0.03)	0.04** (0.02)	0.08*** (0.03)				
Single-Party Dominant	0.39 (0.40)	0.90* (0.50)	0.37 (0.40)	0.88* (0.50)	0.03 (0.38)	0.36 (0.44)	0.00 (0.38)	0.34 (0.44)	0.07 (0.40)	0.42 (0.44)	0.04 (0.40)	0.39 (0.44)	0.14 (0.37)	0.54 (0.44)	0.12 (0.37)	0.51 (0.44)
Constant	-3.78*** (0.67)		-3.76*** (0.66)		-4.47*** (0.78)		-4.42*** (0.78)		-4.66*** (0.93)		-4.59*** (0.93)		-4.48** (1.81)		-4.47** (1.81)	
State FE	–	YES	–	YES	–	YES	–	YES	–	YES	–	YES	–	YES	–	YES
Time Splines	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Obs.	515	515	515	515	515	515	515	515	515	515	515	515	515	515	515	515
Log Lik.	-147.53	-118.84	-148.28	-119.72	-148.27	-118.07	-148.95	-118.99	-148.19	-117.66	-148.89	-118.61	-148.92	-117.64	-149.54	-118.62
χ^2	23.02	22.13	21.87	20.39	19.92	23.67	19.10	21.83	19.47	24.50	18.63	22.59	19.26	24.54	18.21	22.59
Prob $> \chi^2$	0.00	0.01	0.00	0.01	0.11	0.00	0.05	0.00	0.20	0.00	0.11	0.00	0.00	0.00	0.00	0.01

Dependent variable is a dichotomous variable equal to 1 if land reform was passed in state i in year t. Random-effects logit with standard errors in parentheses (unless noted as fixed-effects). Two-tail tests presented despite directional hypotheses. Time-splines not reported but estimated for all specifications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In terms of ideology and competition, both multiparty competition and two-party left-center competition increase the likelihood of land reform passage. However, this effect loses statistical significance in the fixed-effects specification in Model 1. The percentage of the population owning no land also lies in the expected positive direction, and is statistically significant across all models. This provides evidence that poor, landless voters demand reform, and politicians try to win their support through implementing policy.

So far the election indicator has been dichotomous, providing evidence that land reforms are passed in the year before an election. Why does passage occur in the year before an election and not in the election year? Most likely this is because state Assembly elections are typically held early in the year. In fact, the median month of an election in this sample was March. To account for the month of the election, I re-specify the election indicator so that it is equal to $\frac{M}{12}$ in an election year (where M is the month of the election) and $1 - \frac{M}{12}$ in the year before an election (Franzese 2000). All other years equal zero. These monthly weights are shown in the "Alt-RE" and "Alt-FE" columns of Model 1, and show the results from the random effects and fixed effects specifications, respectively. Although the coefficient in the election year remains insignificant, the year before the election effect grows even stronger. In other words, when an election is held in the beginning of the year, the likelihood of land reform in the year before the election increases. The competition variables have similar effects in the alternative models as compared to the dummy variable models. Two-party left-center and multiparty competition are significant only in the random effects specifications, but not when we look within-state. Most likely this is due to the substantial variation across states in regards to electoral competition, but limited within variation.

Since Model 1 did not parse out any effect with ideology and competition, in Model 2 I include *Leftist* and *Congress* indicators through the use of dummy variables. *Leftist* is coded one if a left-wing ideology party is in power in the state, and *Congress* is coded one if the Congress Party is in control. A leftist ideology exerts a strong, statistically significant positive effect on the likelihood of land reform passage. Note that the coefficient on *Year Before Election* remains statistically significant and positive as well, both in the dummy variable and monthly weight specifications. In

addition, while single-party dominant states had a positive effect on the likelihood of land reform passage in the first model (and only in the fixed effects specification), this effect is no longer significant at conventional levels. Model 2 suggests that in addition to opportunistic timing of land reform around elections, leftist parties are more likely to pass land reform than the Congress Party, and that Congress is more likely than other parties to pass reform as well, all else equal. This is consistent with hypotheses H_3 and H_4. Moreover, it confirms Chhibber and Nooruddin's (2004) theory that ideology plays a role in distribution. However competition, as examined in Model 1, seems to matter less than ideology. This suggests that although parties in government may opportunistically time the passage of land reforms, they are more likely to do so along ideological lines.

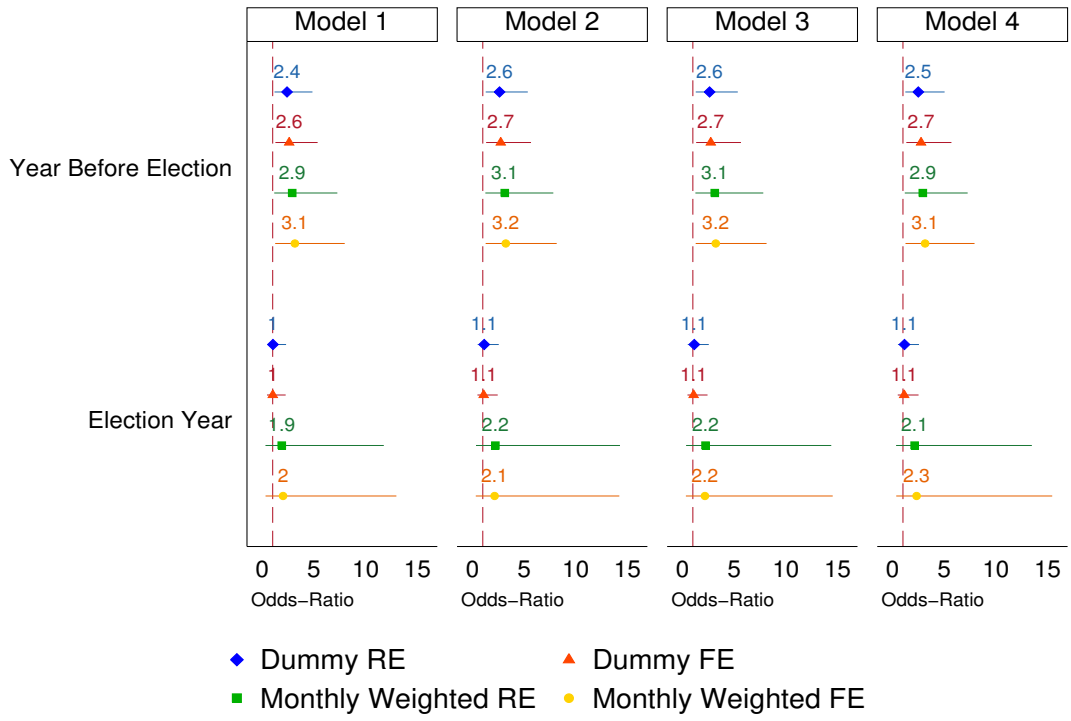
Although the effect of a simple ideology measure was examined in Model 2, it did not account for competition. I add the *Effective Number of Parties* in Model 3, which is coded according to the formula given by Laakso and Taagepera (1979).[11] Even with its inclusion, the year before the election coefficient remains significant and in the same direction as before. The same is true for the leftist and Congress coefficients, as well as the percentage of the population owning no land. However, the effective number of parties does not achieve statistical significance. This provides evidence that the competitive environment does not affect the decision to enact land reform.

In Model 4, I investigate an alternative measure for land-poor voters who increase demand for land reform. Including the percentage of land owned by the bottom 50 percent of the income distribution as well as the percentage of land owned by the top 10 percent captures the inequality of land ownership within a state. Although these variables are in the expected direction—as the poor own more land the need for land reform decreases, as the rich own more land the need for land reform increases—they are not statistically significant at conventional levels. Moreover, including proxies for the inequality of land ownership does not change the earlier results in regards to timing and competition.

[11] This is given by $N_{it} = \frac{1}{\sum_{i=1}^{n} \rho_{it}^2}$, where N_{it} is the number of parties competing in state i in year t with at least one state assembly seat, and ρ_{it}^2 is the proportion of all assembly seats in state i in year t, squared.

To see the robustness of the political cycle of land reform, I plot the odds ratios for the two election variables from Table 3.1 in Figure 3.2. Coefficients to the left of the vertical line (which indicates an odds ratio of one) decrease the likelihood of land reform, while those to the right make reform more likely by a factor of two, four, and so on. Coefficients on the vertical line make land reform no more or less likely to occur. Both the dummy and monthly weighted specifications are shown, along with 95 percent confidence intervals. It is clear that in regards to electoral timing, only the coefficient on the *Year Before Election* is statistically significant. Land reform is about 2.5 times more likely in the year before an election using the dummy variable specification, and around three times more likely when using the monthly weighted specification. Moreover, this result remains robust across all models, even when potentially unobserved state-specific heterogeneity is accounted for using state fixed effects.

Figure 3.2: Policies are consistently passed before elections



In the appendix to this chapter I perform a host of robustness checks. I find that the results are robust after accounting for early elections and President's Rule (the suspension and calling of new elections by the Indian President). I also find that the results are robust after accounting for whether the party in control at the state level is the same as the party in government at the national level. Last, I also employ a variety of additional methodological specifications and find that the results remain unchanged.

3.5.1 Individual-level evidence for the use of policies as a strategic tool

The macro-level evidence presented above suggests that land reform is a strategic tool passed before an election in order to gain the support of voters. This supports analyses in other countries that show how land reform policies may increase political support (Albertus, 2012; Boone, 2012). However, this theory raises two concerns at the individual level about the use of policy passage as a strategic tool to win votes. First, does the repeated use of land reforms weaken the signal? In other words, if land reforms are passed but not fully implemented, are voters aware of this? If so, then continuous policy passage may water down the effectiveness it has as a signal to voters. Second, does the effectiveness of land reform make it less likely to be passed in the future? If reforms are effective, a smaller number of voters will be won over by the promise of further reforms. Thus, the signal to voters may weaken over time.

To examine these concerns, I turn to individual-level survey data from the nationally representative Indian National Election Study (Eldersveld, Ahmed and Marvick 2011). Two important questions were included over time in these non-longitudinal studies. First, both the 1967 and 1971 election studies asked respondents an open-ended question about the 'most-important' problem (MIP) facing their village or town. I coded respondents as having *Land/Inequality as the Most-Important Problem* if they voiced concerns over issues of land or issues of inequality.[12] This vari-

[12]Issues of land covers a variety of responses such as land tenure, consolidation of land, protecting the landless, the size of landholdings, and need to get more land. Issues of inequality applied to responses such as increasing economic disparities and the gap between the rich and poor. Since these were open-ended questions, each type of response was grouped into an overall category.

able is a dichotomous indicator where 1 indicates the respondent was concerned with land reform or issues of inequality, and 0 indicates the respondent was concerned with some other issue.

Second, both the 1971 and 1985 surveys ask whether or not the respondent thinks people with no land or property should occupy the land of those who have a large amount of property.[13] I coded *Approve of Land Grabs* as a dichotomous variable with one meaning "approve of land grabs" and zero meaning "disapprove" or "uncertain". Both question types, though imperfect measures, serve as proxies for the saliency of land reform in the minds of voters.

The question core to this theory is whether or not passage of land reform speeds up temporal dependence (i.e. if we see a smaller proportion of respondents that approve of land grabs or think land issues are the most important problem). If land reform is a strategically-timed policy, voters should continue to show a concern for land grabs and land reform *even in states where land reform has already occurred.* Since this is a conditional expectation, I specify an interactive relationship between duration and the number of land reforms passed between the first and second survey. Duration is proxied by a dichotomous variable equal to one for the latter survey—1985 for the land grab question and 1971 for the important problem question. I expect that land reform will lose saliency over time, but that it is not conditional on the passage of new land reforms. In other words, if land reform saliency remains unrelated to the number of land reforms passed in a state, the interaction between land reform passage and duration should remain insignificant.

Table 3.2 shows the results from the individual responses to each question, modeled using a logit with standard errors clustered by state. A basic model as well a model with additional respondent controls are shown, although there is little difference across either in terms of the log-odds coefficients of interest. The respondent controls were dummy variables for social groups at each end of the social hierarchy, gender, religion, occupation, a 5-category education indicator, a rural area dummy, and a trichotomous indicator of political interest. Additional details are in the appendix to this chapter.

[13] "Some political leaders and parties have been advocating that people with no land and property should occupy a part of land and property of those who have a large amount of land and property. Do you approve of this or do you disapprove?"

Table 3.2: The individual saliency of land reform

	Land/Inequality is the Most Important Problem 1967 & 1971 Surveys		Approve of Land Grabs 1971 & 1985 Surveys	
	(11)	(12)	(13)	(14)
ΔLand Reforms	0.032 (0.289)	0.069 (0.225)	-0.029 (0.113)	-0.028 (0.110)
2^{nd} Survey	0.721*** (0.259)	0.753*** (0.244)	-3.787*** (0.780)	-3.652*** (0.763)
ΔLand Reforms∗ 2^{nd} Survey	-0.451 (0.356)	-0.471 (0.299)	0.338 (0.225)	0.357 (0.219)
Dalit		0.563*** (0.200)		0.345** (0.136)
Backwards Caste		-0.147 (0.211)		0.227 (0.148)
Brahmin Caste		0.089 (0.252)		-0.242 (0.237)
Male		0.142 (0.138)		0.128 (0.102)
Hindu		-0.682*** (0.242)		0.347 (0.326)
Muslim		-1.299* (0.667)		0.577 (0.421)
Farm Laborer		0.217 (0.151)		0.486*** (0.174)
Cultivator		-0.032 (0.214)		0.008 (0.126)
Education		-0.170* (0.103)		-0.244*** (0.047)
Rural		0.779*** (0.282)		-0.318* (0.191)
Political Interest		0.111** (0.052)		0.286*** (0.100)
Constant	-3.644*** (0.256)	-3.652*** (0.517)	-0.223 (0.285)	-0.281 (0.400)
N	6752	6752	5417	5417
States	16	16	16	16
Log Lik.	-1088.0	-1053.0	-3258.8	-3164.5
χ^2	13.54	860.3	115.9	166873.8
Prob $> \chi^2$	0.00	0.00	0.00	0.00

Log-odds coefficients with standard errors in parentheses clustered by state. Dependent variables are those choosing issues regarding land reform or economic inequality as the most important issue in the 1967 and 1971 waves, and those who approve of land grabs in the 1971 and 1985 waves. Variable details available in the appendix to this chapter. Two-tail tests, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

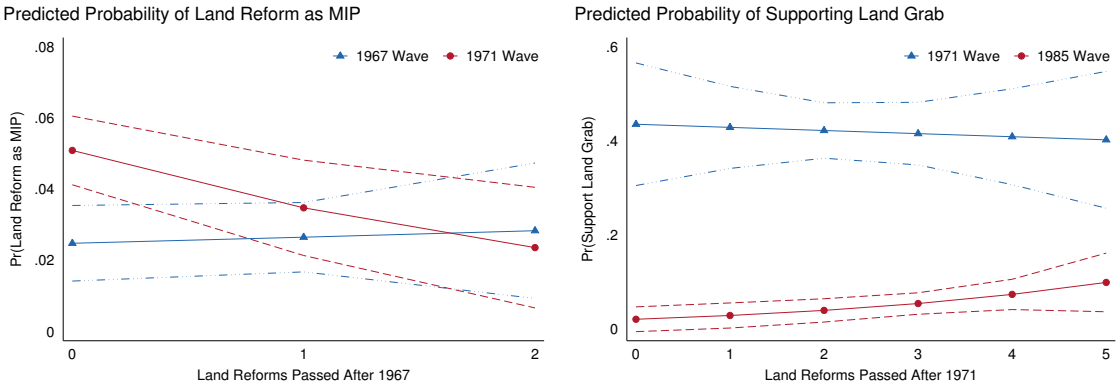
The first two columns of Table 3.2 show the results for a survey respondent choosing land issues or economic inequality as the most important issue facing their town or village. The number of land reforms passed between the 1967 and 1971 waves of the survey is not significant, nor is the interaction between the number of new land reforms passed and the dummy variable indicating the second survey wave. The second survey wave variable is statistically significant, indicating that, as suspected, over time the saliency of land issues decreases.

To help interpret if the over-time decrease in support for land reform differs across the number of reforms passed, I plotted the predicted probabilities from Model 12; this is shown in Figure 3.3a. The vertical axis shows the predicted probability of a respondent selecting land reform as the most important problem. The horizontal axis shows the number of land reforms passed after the first survey wave in 1967. Moving from the 1967 to the 1971 wave, it is clear that the predicted probability actually *increases* if no new land reforms have been passed in the state. Moreover, Figure 3.3a also shows that even in states where land reform was passed, there is no statistically significant difference in the predicted probability between surveys. Taken together, this suggests that although there is a slight decrease in the importance of land reform issues over time, respondents are not affected by the actual passage of reforms. This indicates that land reform policies are not cheap talk, but instead strategic signals.

The second two columns in Table 3.2 show the results for respondents who approve of land grabs. The effect of the change in land reforms between the surveys is small and not statistically significant. Relative to the early survey, the latter has a negative coefficient. As expected, this means that over time the importance of land reform in the eyes of voters diminishes. However, the interaction between time and the number of land reforms passed in a state between waves remains insignificant—indicating that although less respondents approve of land grabs in 1985 than in 1971, this does not depend on the number of land reforms passed between then. As with the previous question, the results remain robust to the addition of the 11 control variables.

The predicted probabilities of supporting land grabs from Model 14 are shown in Figure 3.3b. In contrast to the most important problem question, support for land grabs drops substantially over

Figure 3.3: Previous land reforms do not affect support for future ones



(a) Land reform as most important problem

(b) Support land grabs

time—from nearly 40 percent of respondents approving in 1971 to under 10 percent in 1985. As discussed earlier, the main test in this section is whether or not the salience of land reforms weaken over time. In fact, as shown in Figure 3.3b, the opposite occurs. In the 1985 wave, individuals residing in a state with more land reforms tended to be *more* likely to support land grabs.

The controls in Table 3.2 also shed light on the type of voter that land reform is meant to appeal to. Land reform issues were most likely to be more important to respondents who were of lower caste, had lower levels of education, and strong political interest. Interestingly, rural area, farm laborer, and cultivator dummies tended not to be significant across both models, and in some cases were oppositely signed and significant.

Using individual-level survey data, I have provided further evidence that concern about land remains a salient issue for voters. Although saliency decreases over time, it is not conditional on the passage of further land reforms—if anything the effect became stronger. Thus, both macro- and micro-level evidence suggests that land reforms are policies timed strategically in order to win votes.

3.6 Discussion and conclusion

This chapter theoretically links the research on distributive politics and political budget cycles. Using data on Indian land reform, I find evidence that policy passage is more likely before an election. Policies are a clear signal to voters of incumbent competence, responsiveness to voter demands, and the desire to win over additional supporters. Moreover, individual-level survey evidence confirms that land reform issues remain salient even if reforms have already been passed within a state. In other words, the signal that land reform provides does not seem to diminish over time any faster in states that have passed previous reforms.

Designed to win over rather than reward constituents, land reform is an ideal policy due to its low fiscal cost to governments as well as its high visibility among landless rural voters. This supports earlier findings in Venezuela (Albertus 2012) and Kenya (Boone 2012) that examine the use of land reform as a distributive tool to increase political support. It also supports previous work that finds that reforms are politically salient (Teofilo and Garcia, 2003). This salience is not limited to India; in 2003 an estimated 40 percent of the non-urban population in Asia lived under informal land ownership (Deininger et al. 2003). In Africa this figure is 50 percent. In addition to land reform, other types of legislation may also contain a cyclical component, such as subsidy policies, or the announcement of large make-work projects.

Results in this chapter suggest that competition had only a slight effect on policy passage. However, there was evidence that ideology affects land reforms. Leftist parties were more likely to implement reforms than parties of the center and the right. This suggests that this particular policy may be a partisan good, rather than a purely opportunistic one. To further examine whether opportunistic or partisan behavior is coming into play, a study of the implementation of land reform is needed. Policy implementation, carried out by bureaucratic agents, may differ across ideologies. Or, bureaucracies may be influenced by political considerations in choosing to regulate and enforce certain policies. For instance, in the context of land reform, bureaucratic agents may reward loyal supporters with faster implementation, or they might try to win over swing voters.

In this chapter I have advanced a theory of political policy cycles. Using data on Indian land reform, I find evidence of a policy cycle that is timed to coincide with elections. These findings are complimented by individual-level evidence that suggests that land reforms are precisely the type of issue that matter to voters. Far from being a neutral, demand-based policy, I have argued that governments strategically pass land reforms as a signal to voters, and that they send this signal at a time advantageous to them electorally. Indian land reforms appear to have been constructed to help both the poor and the politicians. As scholars continue to disaggregate macroeconomic indicators into targeted spending areas designed to benefit and attract certain constituencies, the ability to use policy passage around elections as a signal to voters appears to be an important tool of governments.

4. STRONGHOLDS OR ISLANDS? SPATIO-TEMPORAL OPPORTUNISM AND INTERGOVERNMENTAL TRANSFERS

4.1 Introduction

How do politicians reward their political supporters? One line of reasoning holds that they target their core bases of support (Cox and McCubbins 1986; Calvo and Murillo 2004; Nichter 2008).[1] Another finds that constituencies with narrow win margins are rewarded, since swing constituencies are made up of voters who are much easier to "purchase" (Dixit and Londregan 1996; Kwon 2005; Stokes 2005). Both theoretical approaches have led to important discoveries in distributive politics. However, both have tended to focus on a single electoral unit in isolation—such as a town, congressional district, or state—effectively throwing out important information about what is occurring in nearby regions.

In this chapter I build on existing literature in distributive politics by advancing a theory of spatio-temporal opportunism around elections. I extend the core-swing dichotomy that dominates most of the work on intergovernmental transfers around elections in three ways. First, the effects of the core-swing characteristics of a municipality on the level of transfers received may be conditioned by what is occurring in the surrounding constituencies. Despite some literature on temporal opportunism in the swing-voter literature, to the best of my knowledge, no research has considered how spatial patterns of support may affect the distribution of transfers. Second, this is especially likely to matter around elections, when incumbents face strong incentives to reward voters in order to win re-election. In other words, incumbent strategy may shift according to the electoral cycle. Third, I examine if other informational cues about voter support, such as whether or not the municipality elected a mayor of the same party as the national incumbent, factor into their decision to reward certain municipalities.

[1] Throughout this chapter I speak of political benefits in terms of geographic units of support rather than individual supporters, since the vast majority of empirical work in distributive politics focuses on constituencies rather than voters (Golden and Min 2013), but see Stokes (2005), or Nichter (2008).

I use data on federal development transfers to over 5,550 Brazilian municipalities from 2005 to 2012 to test whether opportunism is more likely to occur for municipalities that supported the national incumbent but are surrounded by municipalities less favorable to the incumbent—or "islands" of support—or if instead incumbents tend to reward their regional electoral base, or "strongholds" of support. Brazil is an ideal case for several reasons. It is an open-list system of proportional representation, and existing evidence suggests that pork-barrel spending occurs regularly (Ames 1995). Moreover, research suggests that transfers are channeled to Brazilian municipalities in order to reward voters, and to increase the likelihood of victory for mayors who share the same party as the president (Brollo and Nannicini 2012). In addition, municipalities are highly decentralized and account for around one quarter of all public sector expenditures (Sakurai and Menezes-Filho 2011), making them highly salient political units in the eyes of voters.

Evaluating this "islands versus strongholds" theory of support, I find that incumbents tend to allocate more transfers to municipalities when they are surrounded by municipalities of low support than when they are surrounded by municipalities of high support. Thus, incumbents appear to reward islands rather than strongholds. Although I find no evidence that this pattern changes based on the partisan characteristics of the municipality, I do find that the island-targeting strategy becomes much more broad-based during both municipal and presidential election years.

The rest of this chapter is structured as follows. I first review the existing literature on political opportunism. I then theorize how incumbents may incorporate spatial context when choosing where to strategically place transfers around elections. Next, I test my theoretical expectations and discuss the findings. I conclude with a discussion of the implications of these findings for other scholars of distributive politics.

4.2 Core and swing voters

How does a rational incumbent allocate resources to constituents? Typically, research on this subject has been divided along two theoretical lines: core and swing strategies. Core strategies—largely attributed to Cox and McCubbins (1986)—hold that governments will reward their core

constituencies through targeted provision of goods and services. This has several advantages. For one, parties tend to know their core constituencies well. Continued periods of constituency support and political rewards has the potential to build strong clientelistic relationships (Kitschelt and Wilkinson 2007). In addition, support is "low-risk," since core constituencies typically have a long history of support (Diaz-Cayeros 2008).

A substantial body of empirical evidence supports the core-voter hypothesis. Wilkinson (2007) finds that in India, "politicians are determined to give non-supporters and people whose votes are seen as non pivotal as little as possible" (127-128). Alperovich (1984) finds a similar pattern when looking at earmarked spending by Israel's Likud party. Berry, Burden and Howell (2010) find that congressional districts belonging to the president's party receive almost five percent more expenditures than un-aligned districts. Moreover, benefits are likely to accrue to districts where the previous election was decided by a narrow margin. Examining federal funding to US states, Larcinese, Rizzo and Testa (2006) find that regions of strong presidential support received the largest share of funds, in contrast to swing states. Also focusing on US states, Ansolabehere and Snyder (2006) find support for the core voter hypothesis.

In contrast to rewarding core constituencies, proponents of the swing voter hypotheses argue that since support is already solidified in core districts, politicians will instead target heavily contested areas (Dixit and Londregan 1996; Lindbeck and Weibull 1987; Kitschelt and Wilkinson 2007; Kwon 2005), where there are more voters who are relatively indifferent between parties. One key assumption in swing-voter models is that the pivotal voter is undecided (and thus is already likely to vote); a boost in expenditures can help sway them towards supporting the incumbent (Ansolabehere and Snyder 2006).[2]

Like the core-voter literature, the swing voter hypothesis has substantial empirical evidence to support it. Examining Portugese municipalities, Veiga and Pinho (2007) find that swing municpal-

[2]A closely related literature focuses on the act of getting voters to turnout or abstain from voting, often known as turnout buying (Stokes 2005; Nichter 2008; Bratton 2008). This research tends to focus on the individual, through surveys, formal models, or in-depth interviews. In contrast, much of the core-swing literature that involves geographic constituencies focuses on expenditures and transfers from different levels of government. I focus on this latter literature since it is larger and fits well within a spatial framework.

ities receive more intergovernmental grants. Kwon (2005) finds that districts who are more electorally competitive tend to receive more subsidies in South Korea, while Weghorst and Lindberg (2013) find evidence that clientelistic goods, as well as public goods, sway voters into switching parties in Ghana. All of this suggests that substantial empirical evidence exists for both the core and swing hypotheses.

4.2.1 Distributive politics and vertical intergovernmental opportunism

Much of the core-swing literature has a sub-national focus. As such, vertical opportunism between levels of government—through distributive channels such as grants, fiscal transfers, or expenditures—especially in federal or highly decentralized countries, plays a particularly important role.[3] Transfers from higher to lower levels of government are an important tool used to equal out horizontal inequities between sub-national units (e.g., redistribute revenues to the poorer states), and are especially important in highly decentralized countries that lack the local capacity for revenue raising (Oates 1999; Wallis and Oates 1988). Because of this, transfers often constitute a large proportion of local revenues (Sakurai and Menezes-Filho 2011; Veiga and Pinho 2007).[4] Given that a self-interested national executive is generally considered the main designator of transfers (Berry, Burden and Howell 2010; Brollo and Nannicini 2012), they have the ability to exert substantial influence over sub-national actors. Thus, intergovernmental transfers can become a tool which the incumbent can use for credit claiming, winning re-election, and benefiting co-aligned constituencies while punishing opposition ones.[5]

There is substantial evidence that political favoritism plays a role in intergovernmental redistribution in Peru (Schady 2000), Germany (Dellmuth and Stoffel 2012), Portugal (Veiga and Pinho 2007), Brazil (Brollo and Nannicini 2012), and the US (Levitt and Snyder 1995; Berry, Burden and

[3]Expenditures and transfers are not the only form of pork; alternative strategies include extending access to government employment or increasing public sector wages (Calvo and Murillo 2004).

[4]For instance, 43.1 percent of total municipal revenues in Portugal (Veiga and Pinho 2007).

[5]While co-alignment is often thought of (and operationalized) as being if the party in power at the local government is the same as the national government, this does not have to be the only case. Ethnic, racial, or regional ties, though more difficult to operationalize, could also play a role.

Howell 2010).[6] Often, this involves rewarding co-aligned sub-national units while punishing non-aligned ones, as Larcinese, Rizzo and Testa (2006) find in the case of US state funding, and Brollo and Nannicini (2012) when looking at close municipal elections in Brazil. However, it is not always the case that solidly aligned sub-national units are always rewarded. Looking at Australian state elections, Worthington and Dollery (1998) find only modest support for the co-alignment hypothesis, and also find that marginal federal seats are punished, rather than solidly opposition-controlled regions.

4.2.2 The timing of opportunism

While the work above largely focuses on the determinants of expenditures and transfers within a sub-national unit, a vast literature focuses on when spending is most likely to occur (Franzese 2002; Cole 2009; Drazen and Eslava 2010*a*; Sáez and Sinha 2010).[7] Given that incumbents desire re-election, there is often an incentive to time intergovernmental transfers around elections, so as to maximize the marginal returns of their effort. In fact, in a meta-analysis of the political budget cycle literature, Philips (2016) finds that intergovernmental grants show some of the strongest evidence for manipulation around elections. For instance, Schady (2000) finds that Peruvian social expenditures tend to increase during elections, going to high-poverty areas as well as areas with heavy support for the governing party. Kwon (2005) finds that subsidies to voters in South Korea increase around elections, especially for highly-competitive districts. Expenditures that are more visible to voters are more likely to be manipulated during elections, although there is mixed evidence as to whether current expenditures such as public sector wages (Akhmedov and Zhuravskaya 2004; Kwon 2005), or capital expenditures such as road construction (Drazen and Eslava 2010*a*), are more likely to be manipulated (Philips 2016).[8]

[6] Schady (2000) finds that Peruvian social expenditures are channeled to areas that support the governing party. Examining the distribution of EU structural funds in German districts, Dellmuth and Stoffel (2012) find that electoral considerations tend to distort the allocation of funds; supporting the party of the prime minister is associated with an increase in EU structural funds. Veiga and Pinho (2007) find that municipalities that elected mayors of the same party as the Prime Minister tend to receive more grants. Levitt and Snyder (1995) show that federal spending tends to favor heavily Democratic districts, especially during the 1970s when Democrats had strong control at the federal level, and Berry, Burden and Howell (2010) find a similar pattern looking at US congressional districts.

[7] For an excellent review see Dubois (2016).

[8] In a meta-analysis of these two expenditure categories, Philips (2016) finds no evidence that either form of expenditure is consistently manipulated around elections.

4.3 A theory of spatio-temporal opportunism

As discussed above, the existing literature in distributive politics has made substantial inroads as to how factors such as political competition and partisan alignment affect the distribution of benefits. However, with few exceptions—and despite a substantial literature on swing-voter politics—there is surprisingly little about spatio-temporal effects on intergovernmental transfers. On the temporal side, Diaz-Cayeros (2008) focuses on the volatility of electoral regions; regions that shift support from one party to another are risky to target, but offer a large potential gain in support. Albertus (2012) finds that governments may pursue a strategy whereby short-term payoffs are channeled to core supporters while long-term structural policies are created in order to win over swing supporters. In fact, party strategies in general may become more short-sighted as electoral competition increases, especially in the developing country context (Lupu and Riedl 2013).

Despite some literature on temporal opportunism in the swing-voter literature, to the best of my knowledge no research has considered how spatial patterns of support may affect the distribution of transfers. Why might electoral characteristics of surrounding localities factor in to how an incumbent allocates transfers? Consider an incumbent executive at the national level. In many countries such as the US, Brazil, or India, incumbents control a large amount of discretionary transfers to sub-national jurisdictions. They are opportunistic in that they desire to win re-election (by seeking out new voters), as well as trying to reward those in their own party. Therefore, they tend to strategically allocate transfers.[9] This is a common assumption in distributive politics, one which has empirical support (Brollo and Nannicini 2012; Berry, Burden and Howell 2010; Dellmuth and Stoffel 2012; Veiga and Veiga 2007; Levitt and Snyder 1995), although as discussed above, it is less established whether principals tend to target core constituencies, swing constituencies, or both.[10]

[9]This assumes that transfers are not limited by budgets, and that incumbents do have some discretion over their placement. Discretionary transfers in some countries such as Brazil constitute a large proportion of municipal revenues but a relatively small proportion of the national budget, thus can be seen as largely discretionary (Brollo and Nannicini 2012).

[10]I am careful to distinguish between geographic constituencies that are either swing or core regions rather than individual voters, since the latter is almost never tested in the literature (Golden and Min 2013). For an exception see (Stokes 2005).

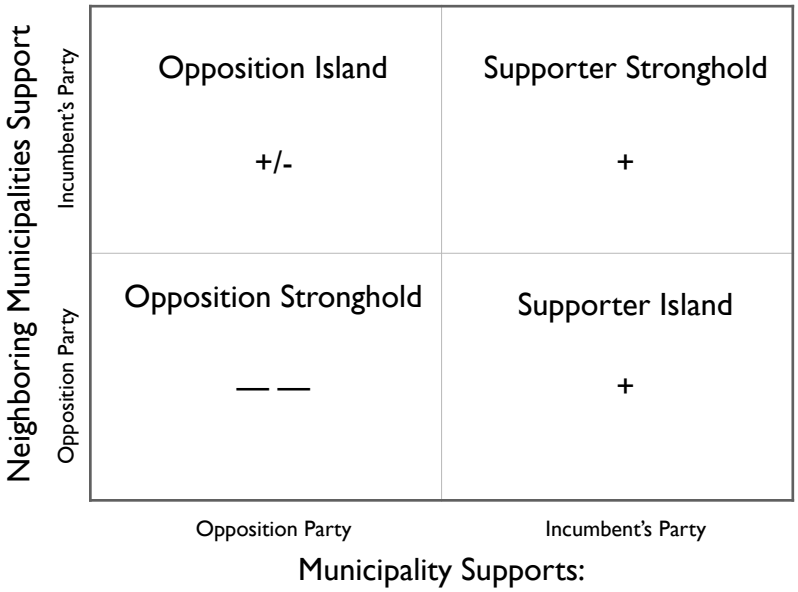
I argue that opportunistic targeting of intergovernmental discretionary transfers may occur not only based on the electoral results in a locality—as current theories of distributive politics would predict—but also based on the level of support of the surrounding localities. There are a number of reasons why the incumbent would want to incorporate surrounding information when deciding how to reward a municipality.

First, assuming that the instrument of manipulation has spillover effects, it may be wise for an incumbent to reward core regions (i.e. concentrations of municipalities of high support) rather than a municipality surrounded by weak or opposition supporters. Evidence suggests that certain government expenditures have spillover effects to proximate spatial units (Murdoch, Rahmatian and Thayer 1993; Baicker 2005). For instance, Solé-Ollé (2006) finds that local government expenditures in areas such as radio, museums, and parks in Spain have beneficial spillover effects to neighboring municipalities. By taking into account information about neighboring municipalities, an incumbent can strategically place transfers so as to avoid rewarding opposition municipalities through potential spillovers.

Second, a single election result may be a one-off event and not represent the true characteristics of a municipality. That is to say, a particular election result could be stochastic and not representative of the underlying amount of support for the incumbent or the incumbent's party. Incorporating surrounding information allows for a more detailed picture of true regions of support. An incumbent may discount a particularly poor performance in a municipality if surrounding municipalities are strong supporters; likewise, they may not reward a municipality that strongly supported the incumbent if its neighbors offered only marginal support. Thus, the amount of intergovernmental transfers may not be entirely a function of municipal support, but instead moderated by the performance of the geographic region as a whole.

Third, the incumbent can strategically punish or reward districts, since—assuming they have similar economic and demographic characteristics as their surrounding neighbors—it should be low-cost to entice voters in these municipalities. Thus, swing regions of support may be particularly likely to be targeted if they are surrounded by regions of incumbent support.

Figure 4.1: Strongholds and islands: A theoretical figure



The effect that these compositions have on the allocation of intergovernmental transfers is shown as a simple theoretical diagram in Figure 4.1, using an example of municipal support in during national elections. The columns of Figure 4.1 show the previous results during the last election; either the municipality has strong support for the incumbent's party, or they have weak support. This is the extent of existing theories on geographic core and swing districts; opposition districts receive the least amount of intergovernmental transfers, while those of strong support are rewarded.

The theoretical expectations shown in Figure 4.1 add nuance to this argument. It is not only the amount of support *within* a district, but also the amount of support in the *surrounding* districts that determines the level of intergovernmental grants allocated. As shown in Figure 4.1, incumbents may also take into account the level of support in neighboring municipalities when deciding how much to reward districts. This leads to the following theoretical expectations: municipalities that are strong supporters of the incumbent's party *and surrounded by similarly inclined municipalities*

(regions that are "supporter strongholds" in Figure 4.1) will receive a larger amount of transfers, all else equal. Municipalities that supported the incumbent's party, but that are surrounded by municipalities that support the opposition party ("supporter islands"), will also receive a larger amount of transfers. If theories of core-swing politics are extended to spatial units, diverging expectations emerge as to the amount of expected transfers in supporter strongholds relative to supporter islands. The core voter hypothesis might lead us to expect that geographic strongholds of support receive the largest amount of transfers. In contrast, the swing voter hypothesis may lead to the expectation that supporter islands of support, or even "opposition islands"—where support for the incumbent is low but not as low as the surrounding municipalities—are likely to be targeted with an increase in transfers. I expect municipalities that strongly support the opposition to receive the least amount of transfers, especially if they are surrounded by similar municipalities, or regions of "core opposition".

In addition to taking into account the spatial context of intergovernmental transfers, the literature on political budget cycles suggests that political opportunism is most likely to occur in the run up to an election. Thus, the spatial opportunism shown in Figure 4.1 may be strongest before an election. Or, incumbents may shift opportunistic strategies in election years. I illustrate this theory in the context of Brazil below.

4.4 Strongholds and islands in Brazil

I use data on 5,550 Brazilian municipalities from 2005 to 2012 to test whether spatio-temporal opportunism exists. Brazil is an ideal test of whether incumbents reward strongholds or islands for several reasons. It is a relatively young democracy. Moreover, is highly decentralized, and evidence suggests that opportunism takes place (Brollo and Nannicini 2012; Fried 2012). Another advantage is that voting is mandatory in Brazil; this mitigates some concerns about incumbents spending for "turnout buying" rather than "conversion buying" or rewarding core supporters (Ansolabehere and Snyder 2006; Nichter 2008).

Recent electoral outcomes in Brazil for the time period used in my analysis are shown in Table 4.1. Presidential elections have been held every four years, in 2002, 2006, 2010, and 2014. Municipal elections are always staggered two years after presidential ones: 2004, 2008, and 2012. The four right-most columns show the results of the national elections. The Brazilian Worker's Party, or PT, has consistently won the presidency from 2002 to 2014, moving from Lula da Silva to his predecessor, Dilma Rousseff. The Brazilian Social Democracy Party, or PSDB, has always been the largest opposition party in recent elections. Therefore, the consistency of the party in government and the main opposition party at the national stage makes examining Brazil ideal.[11]

Table 4.1: Brazilian electoral outcomes

Year	Election Type	Winning Party	Coalition Members	Opposition Party	Opposition Coalition Members
2002	Presidential	Lula da Silva (PT)	PL PC do B, PMN, PCB, PV	José Serra (PSDB)	PMDB, PP, PFL
2003					
2004	Municipal				
2005					
2006	Presidential	Lula da Silva (PT)	PC do B, PRB, PMDB, PL, PSB, PP, PMN	Geraldo Alkmin (PSDB)	PFL, PPS
2007					
2008	Municipal				
2009					
2010	Presidential	Dilma Rousseff (PT)	PMDB, PC do B, PDT, PRB, PR, PSB, PSC, PTC, PTN	José Serra (PSDB)	DEM, PTB, PPS, PMN, PT do B
2011					
2012	Municipal				
2013					
2014	Presidential	Dilma Rousseff (PT)	PMDB, PSD, PP, PR, PROS, PDT, PC do B, PRB	Aécio Neves (PSDB)	PMN, SD, DEM, PEN, PTN, PTB, PTC, PT do B

[11] Both the PT as well as the opposition PSDB have had slight changes to the makeup of their coalitions over time.

The dependent variable in my analysis is per capita infrastructure transfers to municipalities. These are, "related to budget items that involve the construction of buildings or bridges, the paving of roads, the improvement of water and sewer systems, the purchase of ambulances, and so on," and evidence already suggests that these transfers are distributed primarily by the president for opportunistic reasons (Brollo and Nannicini 2012, p. 749).[12] Using a regression discontinuity design, Brollo and Nannicini (2012) find that mayors that are aligned with the ruling PT government receive about one-third greater infrastructure transfers than municipalities that narrowly elected a mayor of the opposition.

4.4.1 Hypotheses

Based on the theoretical argument discussed above, I have a number of expectations as to the direction and relative size of the coefficients in the context of Brazil. First, I expect that greater municipal support for the president's party in the previous election will lead to larger transfers:

H_1**:** *As electoral support for the president increases, municipal transfers increase*

The first hypothesis focuses only on how within-municipality factors might affect the level of transfers from the national government. Taking into account the electoral support of neighboring municipalities leads to two competing theoretical expectations. On the one hand, incumbents may reward stronghold regions. Thus, we might expect that increases in electoral support for the president will lead to larger transfers, but only when surrounding support is also strong:

H_2a**:** *Increasing electoral support leads to increased municipal transfers, especially when a municipality is surrounded by strong incumbent support*

On the other hand, if we take the view that it is not strongholds, but islands, that the incumbent should attempt to reward, we should observe the opposite effect. Greater electoral support for the incumbent should still lead to increased infrastructure transfers, especially when a municipality is surrounded by weak support for the incumbent:

[12] Moreover, most of the other types of transfers from the national level to municipalities are non-discretionary (Brollo and Nannicini 2012).

*H_2b***:** *Increasing electoral support leads to increased municipal transfers, especially when a municipality is surrounded by weak incumbent support*

Last, the literature on temporal opportunism posits that there are strong incentives to time budgetary changes to coincide with the election. Thus, transfers around the election should be larger.[13] I expect that political budget cycles may take place for both presidential and municipal elections, since an incumbent wants to ensure re-election in the former, and tends to support co-aligned incumbents in the latter (Brollo and Nannicini 2012). In addition, this pattern of support may take place along the strongholds versus islands divide discussed above. Perhaps transfers are timed around elections, but only to those regions of core support (strongholds) or instead to supporters surrounded by municipalities of weak support (islands):[14]

*H_3a***:** *Evidence for hypothesis H_2a will be strongest in an election (municipal or presidential) year*

*H_3b***:** *Evidence for hypothesis H_2b will be strongest in an election (municipal or presidential) year*

4.5 Data and methods

As discussed above, I use data on 5,550 Brazilian municipalities from 2005 to 2012 to test whether incumbents pursue a strategy of rewarding strongholds or islands of support. The dependent variable is the per capita infrastructure transfers to municipality i in year t. I use a random-effects estimator for all models. To test if political transfers are a function of presidential support both within-municipality as well as based on the surrounding municipalities, I use the following model:

$$Transfers_{it} = \beta_0 + \phi Transfers_{it-1} + \beta_1 Vote_{it} + \beta_2 \mathbf{W} \cdot Vote_{it} + \beta_3 Vote_{it}\ x\ \mathbf{W} \cdot Vote_{it} + \beta_4 PT/Coal\ Mayor_{it} + \gamma \mathbf{Controls} + \varepsilon_{it} \quad (4.1)$$

where $Transfers_{it}$ is the level of intergovernmental discretionary transfers per capita for municipality i in year t, β_0 is a constant, $Transfers_{it-1}$ is the previous year's per capita transfers to municipal-

[13] Municipal elections are held in October in Brazil; political budget cycles are likely to occur in the election year, given that the election is not held until 10 months into the year. Previous scholars have accounted for different months of elections in a number of ways, including the method used by Franzese (2000) which weights the month of the election.

[14] I remain ambivalent as to whether these effects should be largest during municipal or presidential elections

ity i, and $Vote_{it}$ is the vote share of the president (always the PT) in the last presidential election. To take into account the electoral performance outside a municipality, I add, $\mathbf{W} \cdot Vote_{it}$, which is the vote share of the surrounding municipalities. This is also interacted with the vote share of the municipality. The measure of the vote share of the surrounding municipalities, $\mathbf{W} \cdot Vote_{it}$, is known as a spatial-X variable, with a weights matrix, $\mathbf{W}$, specified by the analyst. This specification is seldom straightforward, and often best-guided by theory (Neumayer and Plümper 2012; Beck, Gleditsch and Beardsley 2006). In this case, I use an inverse distance connectivity matrix, given as $w_{ij} = \frac{1}{d_{ij}}$, where each entry for row i, column j, is w_{ij}, and d_{ij} is the centroid distance between municipalities i and j. Therefore, if municipality j is far away from municipality i, it will have only a small effect. I then row standardized the matrix by dividing each entry by the largest row sum and column sum in the matrix (min-max row standardization). Next, I premultiplied presidential vote by the $\mathbf{W}$ matrix. This creates a new vector, $\mathbf{W} \cdot Vote_{it}$, where higher values indicate that more surrounding municipalities support the president, while low values indicate that the surrounding municipalities have weak support.

In Equation 4.1, I also include $PT/Coal\ Mayor_{it}$ to control for partisan co-alignment between the national and subnational levels (Khemani 2004; Brollo and Nannicini 2012), which is a dichotomous variable equal to one if the mayor of the municipality belongs to either the PT or a coalition member.[15] I also include log total expenditures, the percentage of extremely poor residents, the percentage of residents living in rural areas, the age of the mayor, and a dummy variable for if the mayor is in their second—and final—term as a set of controls.

4.6 Results

The first set of results are shown in Table 4.2. Model 1 shows the results from Equation 4.1 without the interaction between a municipality's presidential vote and the surrounding presidential vote. As expected, a one percentage point increase in presidential vote in a municipality is associated with a 0.12 Brazilian Real (R$) increase in per capita transfers.[16] In addition, the statistical significance

[15] Municipalities held by a PT mayor constituted about 9 percent of the sample; for those held by the PT or a coalition member, this is around 46 percent.

[16] In mid-2010 the exchange rate was about R$1.8 per US dollar.

of the lagged dependent variable suggests that transfers change annually at a moderate pace; on average less than half of a municipality's level of transfers can be attributed to the prior level of transfers alone.

Model 1 shows support for the hypothesis that stronger support for the president leads to larger subnational transfers. Yet does the surrounding support for the president affect how much the incumbent distributes to a municipality? In Model 1, the coefficient on the spatial-X variable, **W**·Presidential Vote, is negative and statistically significant. This suggests that all else equal, a municipality that is surrounded by strong incumbent-supporting municipalities receives less transfers than one that is surrounded by municipalities with weak support.

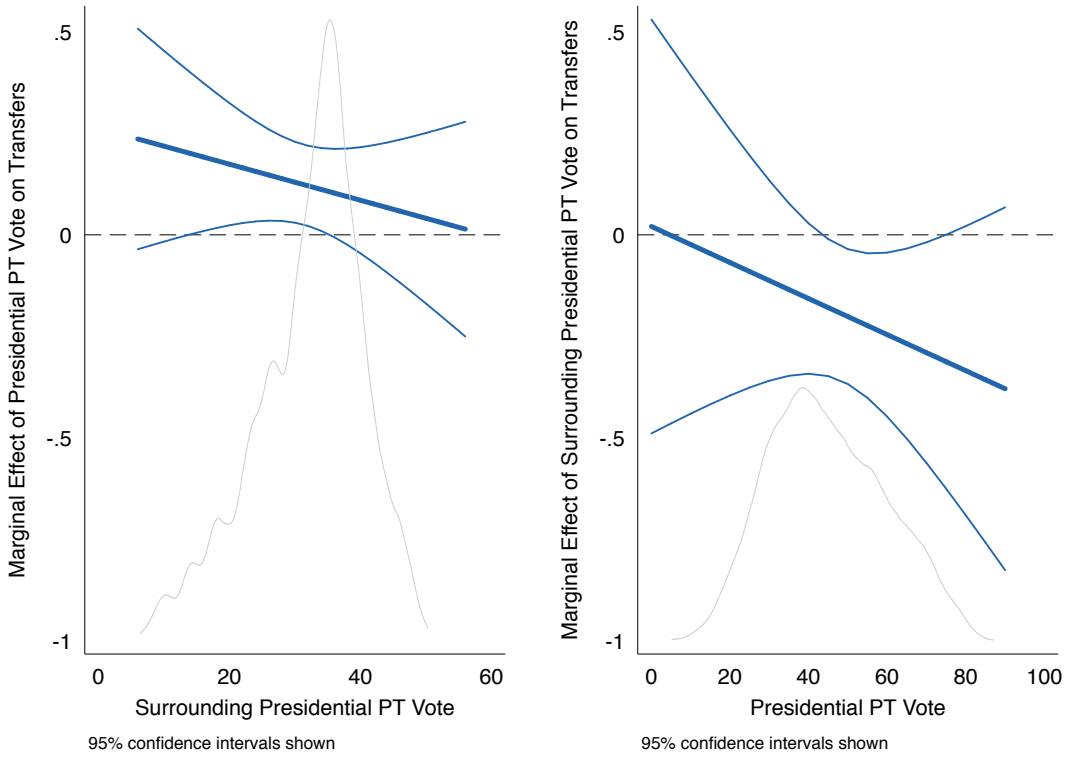
While Model 1 shows evidence for the first hypothesis, it cannot test the second two hypotheses on whether or not surrounding municipalities' support for the incumbent conditions the effect that a municipality's own presidential support has on transfers. To do so I turn to Model 2, which interacts presidential vote with the spatial-X variable, surrounding presidential vote. Since interpreting interactive effects is not always straightforward, I use marginal effects plots, which are shown in Figure 4.2. The plot on the left in Figure 4.2 shows the marginal effect of support for the PT in the last presidential election, across the level of support of the surrounding municipalities (where larger values indicate stronger support for the PT). This result clarifies the findings in Model 1; increases in a municipality's support for the PT has the largest positive effect on transfers when surrounding municipalities are less supportive. While the marginal effect is not statistically significant at either the very low- or high-end range of surrounding presidential PT vote, this finding suggests that incumbents tend to favor islands of support rather than strongholds. This can also be seen in the plot on the right in Figure 4.2, which shows the marginal effect of the surrounding municipalities' support for the PT, across the level of presidential vote for the PT. Again, while the results are not statistically significant across the entire range, it shows that the marginal effect of increasing surrounding support for the PT is always negative across the level of a municipality's own presidential support, and becomes more negative as surrounding support for the incumbent party increases. The results from the first two models in Table 4.2 suggest that incumbents take

Table 4.2: Evidence of spatio-temporal opportunism in Brazilian municipalities

	(1)	(2)	(3)
Transfers_{it-1}	0.458***	0.458***	0.458***
	(0.005)	(0.005)	(0.005)
Presidential Vote	0.123**	0.263	0.299
	(0.050)	(0.168)	(0.226)
W·Presidential Vote	-0.194**	0.021	0.209
	(0.084)	(0.260)	(0.336)
PT Mayor/Coal. Won	6.131***	6.146***	16.066
	(1.223)	(1.223)	(16.196)
W·Presidential Vote *x* Presidential Vote		-0.004	-0.007
		(0.005)	(0.007)
PT Mayor/Coal. Won *x* **W**·Presidential Vote			-0.481
			(0.500)
PT Mayor/Coal. Won *x* Presidential Vote			-0.111
			(0.335)
PT Mayor/Coal. Won *x* **W**·President. Vote *x* President. Vote			0.007
			(0.010)
Municipal Election Yr.	52.356***	52.358***	52.397***
	(1.463)	(1.463)	(1.463)
Presidential Election Yr.	37.516***	37.656***	37.594***
	(1.517)	(1.526)	(1.534)
ln(Expenditures)	-7.077***	-7.100***	-7.109***
	(0.676)	(0.676)	(0.676)
% Extremely Poor	-1.310***	-1.301***	-1.303***
	(0.068)	(0.069)	(0.070)
% Rural Population	0.375***	0.377***	0.377***
	(0.036)	(0.036)	(0.036)
Mayor Age	-0.654***	-0.656***	-0.655***
	(0.064)	(0.064)	(0.064)
Second Term	3.407***	3.401***	3.398***
	(1.261)	(1.261)	(1.261)
Constant	176.309***	169.934***	166.420***
	(12.610)	(14.572)	(16.081)
N	37540	37540	37540
Municipalities	5549	5549	5549
R^2	0.22	0.22	0.22
χ^2	10710.99***	10711.69***	10714.47***

Note: Dependent variable is per capita infrastructure transfers. Random-effects regression with standard errors in parentheses. Two-tailed tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4.2: Incumbents reward islands, not strongholds

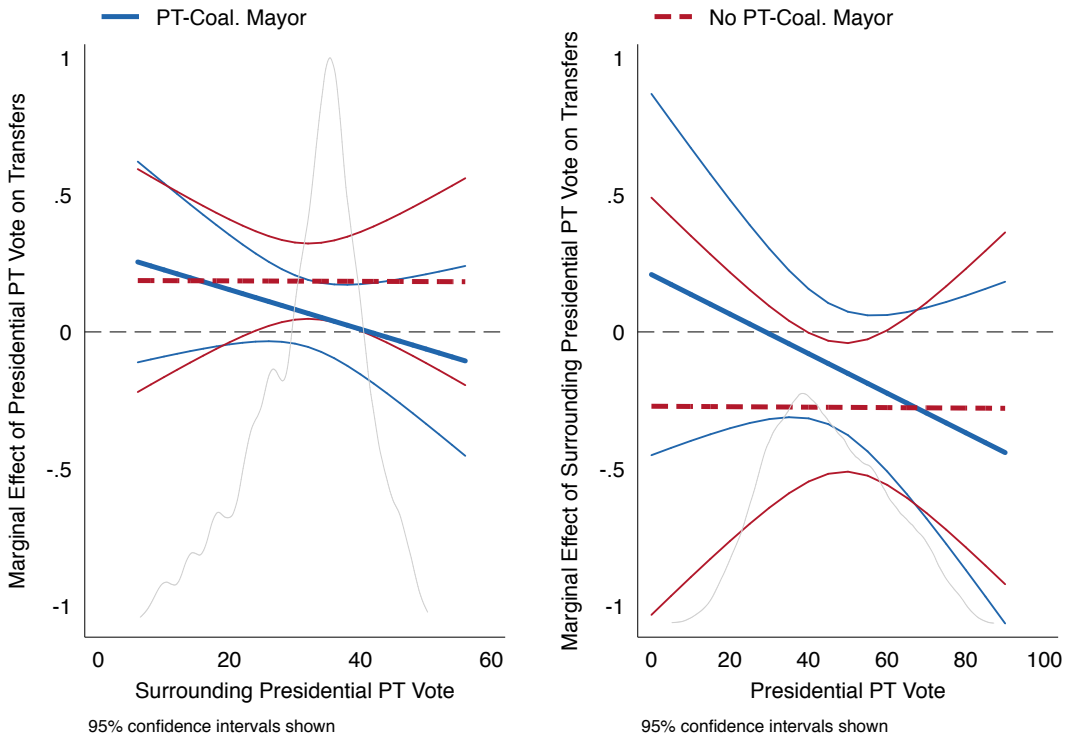


into account their performance in the surrounding municipalities when choosing how to allocate discretionary transfers.

4.6.1 Incorporating the sub-national context

So far, the results suggest that incumbents incorporate information about previous national PT support of the surrounding geographic area when allocating transfers to a given municipality. However, the incumbent also has information on support at the local level; this was accounted for in models 1 and 2 in Table 4.2 with the dummy variable for whether or not a the municipality elected a PT mayor (or mayor of a party in the PT's governing coalition). In both models, this variable was positive and statistically significant, confirming previous findings that co-aligned sub-national units are often rewarded by their national counterpart (Levitt and Snyder 1995; Veiga and Pinho

Figure 4.3: Sub-national partisanship affects transfers but it does not condition islands vs. strongholds



2007; Dellmuth and Stoffel 2012). Yet does such co-alignment affect the findings that incumbents provide transfers to islands of support rather than strongholds? To test this, in Model 4 in Table 4.2 I interact the mayoral dummy with both presidential vote and surrounding presidential vote, as well as the interaction between the two.

To better interpret these results, I plot the marginal effects in Figure 4.3. Overall, there are no statistically significant differences between municipalities that elected a PT or coalition mayor and those that did not. This suggests that although PT or coalition mayors tend to receive more transfers than other municipalities, such partisanship is unassociated with the effect between presidential electoral support and surrounding presidential support.

4.6.2 Taking elections into account

The results in Table 4.2 indicate that incumbents reward supporter municipalities, especially when they are surrounded by municipalities of low support. Yet incumbents also have an incentive to increase these transfers at key points in the electoral cycle. As shown in Table 4.2, the coefficients on the municipal and presidential election year dummy variables are always positive and statistically significant. This is evidence of a political budget cycle effect for transfers. In addition, it may be the case that the targeting of islands of support may be greatest during elections.

To see if elections condition the previous finding that the national incumbent targets islands of support and not stronghold regions, in Table 4.3 I interact the municipal election dummy variable—in Model 4—and national election dummy variable—in Model 5—with the percent of vote for the incumbent and the surrounding vote for the incumbent, and the interaction between them.[17] Again, marginal effects plots offer the easiest way to interpret the interactive results in Table 4.3. The results from Model 4 are depicted in Figure 4.4, where in the left plot I plot the marginal effect of presidential vote on transfers across surrounding presidential vote. The solid blue line is the marginal effect in a municipal election year, while the red dashed line is the marginal effect in a non-municipal election year.

The results in Figure 4.4 show two vastly different incumbent strategies. In a non-municipal election year, the marginal effect of a municipality's presidential support is positive and significant only when they are surrounded by weak supporting municipalities. When surrounding support is high, stronger municipal support is associated with less transfers, all else equal. In contrast, in a municipal election year the marginal effect of previous PT presidential vote is always positive across the range of surrounding municipal support, albeit not statistically significant when surrounding support is very low. The marginal effect for surrounding PT presidential support shown on the right of Figure 4.4 supports these findings; in a non-election year, incumbents tend to reward weak PT supporters in strong PT-supporting areas. In an election year, weak supporters are

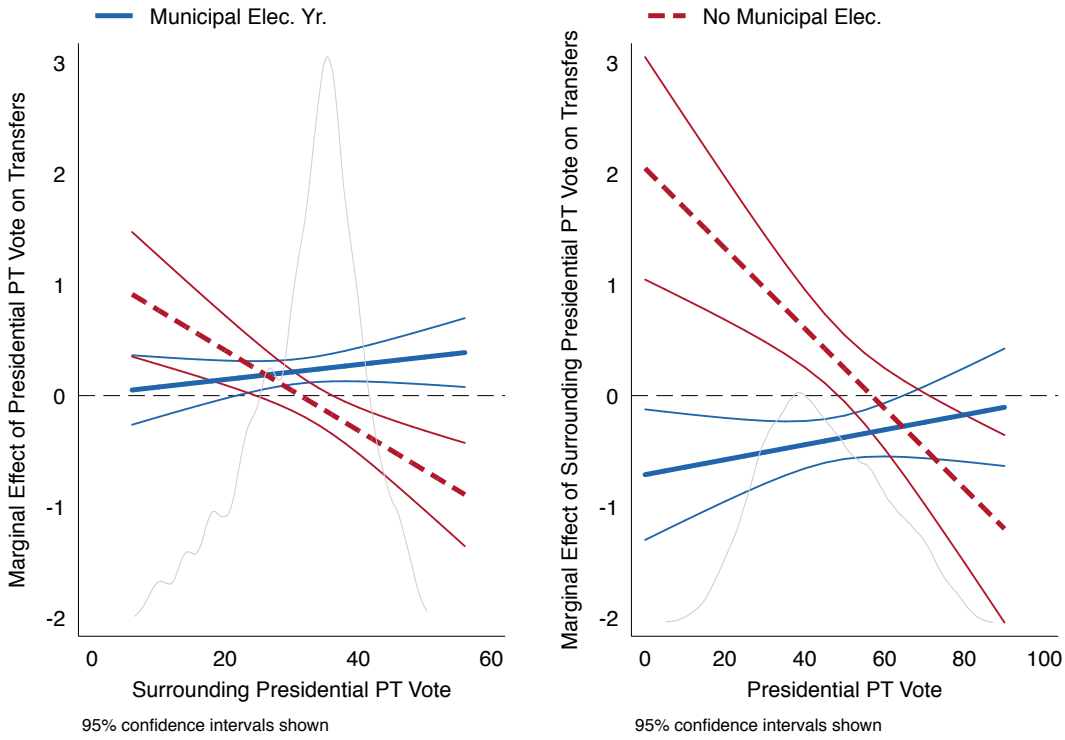
[17] Recall from Table 4.1 that municipal and presidential elections are staggered, the former being held in 2004, 2008, and 2012, and the latter in 2002, 2006, 2010, and 2014.

Table 4.3: Incumbent strategy shifts during municipal elections

	(4)	(5)
Transfers_{it-1}	0.458***	0.458***
	(0.005)	(0.005)
Municipal Election Yr.	-25.049	52.378***
	(19.796)	(1.464)
Presidential Election Yr.	37.147***	23.558
	(1.537)	(17.340)
President. Vote	0.010	0.192
	(0.194)	(0.215)
W·President. Vote	-0.713**	-0.150
	(0.300)	(0.322)
W·President. Vote *x* President. Vote	0.007	-0.002
	(0.006)	(0.006)
Municipal Election Yr. *x* **W**·President. Vote	2.763***	
	(0.588)	
Municipal Election Yr. *x* President. Vote	1.120***	
	(0.395)	
Municipal Election Yr *x* **W**·President. Vote *x* President. Vote	-0.043***	
	(0.012)	
Presidential Election Yr. *x* **W**·President. Vote		0.614
		(0.551)
Presidential Election Yr. *x* President. Vote		0.221
		(0.376)
Presidential Election Yr. *x* **W**·President. Vote *x* President. Vote		-0.011
		(0.011)
PT Mayor/Coal. Won	6.092***	6.186***
	(1.223)	(1.227)
ln(Expenditures)	-7.145***	-7.065***
	(0.676)	(0.677)
% Extremely Poor	-1.334***	-1.295***
	(0.069)	(0.072)
% Rural Population	0.383***	0.374***
	(0.036)	(0.036)
Mayor Age	-0.657***	-0.656***
	(0.064)	(0.064)
Second Term	3.307***	3.434***
	(1.260)	(1.261)
Constant	189.559***	173.825***
	(15.280)	(16.154)
N	37540	37540
Municipalities	5549	5549
R^2	0.22	0.22
χ^2	10755.92***	10713.82***

Note: Dependent variable is per capita infrastructure transfers. Random-effects regression with standard errors in parentheses. Two-tailed tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4.4: Incumbents shift to rewarding supporters



rewarded less as the surrounding level of PT support is stronger. This suggests that incumbents pursue a strategy of rewarding islands of support and punishing non-supporting municipalities in stronghold regions in off-election years, and shifting towards rewarding supporter municipalities, no matter the level of surrounding support, during municipal elections.

To see if this effect holds in presidential election years, in Figure 4.5 I plot the marginal effects of presidential support and surrounding presidential support from Model 5 in the left and right plots, respectively. Note now that the solid blue line shows the marginal effect in a presidential election year, while the dotted red line shows the marginal effect in a non-presidential election year. Unlike the results for municipal election, there appears to be no difference between presidential election and non-election years in terms of affecting the relationship between municipal support and surrounding support. The marginal effect of municipal support for the PT is generally positive (though not statistically significantly different from zero) across the range of surrounding support for the PT. Therefore, incumbents appear to shift their strategy during municipal election years, not presidential ones.

4.6.3 Robustness

I investigate the robustness of the findings above by examining the vote share in the second round of presidential elections. In Brazil, these are held if no candidate secures a majority vote. Second rounds were held in each presidential election in the sample. If a second round is held, only the top two candidates face off against each other. If voters use the first round as a "protest vote," results may be more clear when examining the second round of presidential election results.

As with the first-round results, I estimate the same five models on the second round. These are shown in Table 4.4. Across the first four models, the results are very similar to those using the first round of presidential election results. The exception is Model 10, which examines whether incumbent strategy of rewarding strongholds or islands shifts during presidential elections. While none of the interaction terms in this model were significant in Table 4.3, many of them are in Table 4.4.

Figure 4.5: No shifting strategies during presidential elections

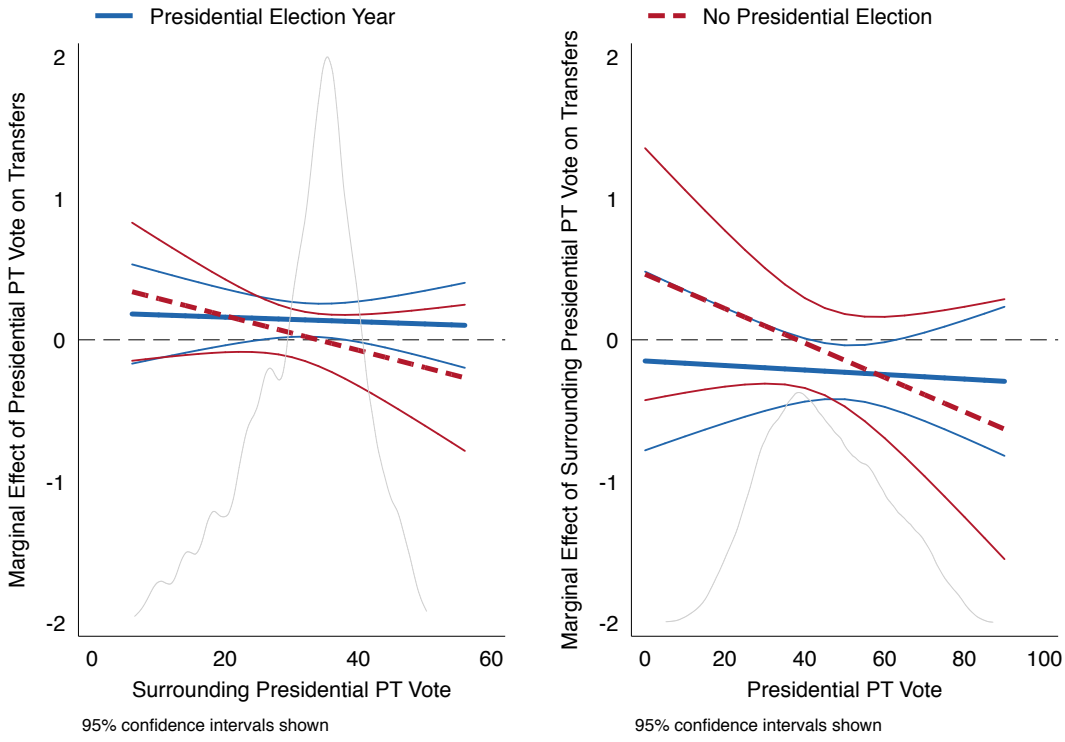
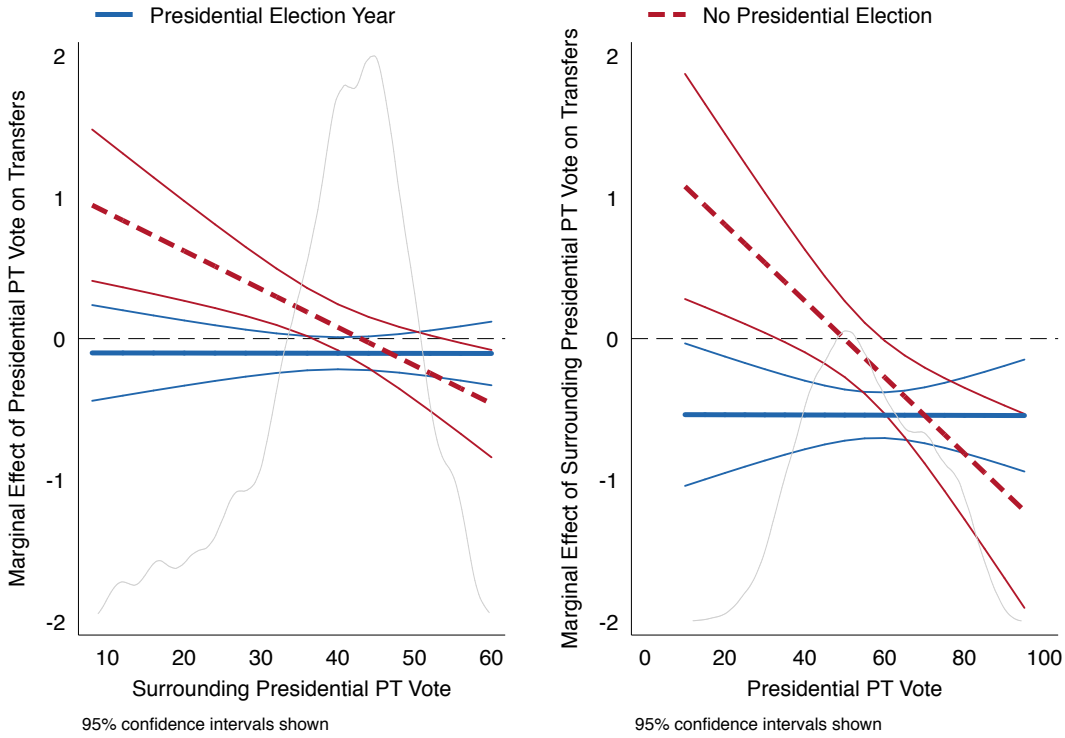


Table 4.4: Spatio-temporal opportunism in the second round of voting

	(6)	(7)	(8)	(9)	(10)
Transfers_{t-1}	0.456***	0.456***	0.456***	0.456***	0.456***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$\mathbf{W}\cdot 2^{nd}$ Round	-0.443***	0.049	0.142	-0.646**	-0.537*
	(0.071)	(0.260)	(0.348)	(0.305)	(0.305)
2^{nd} Round	-0.015	0.321*	0.332	0.032	-0.101
	(0.049)	(0.177)	(0.244)	(0.208)	(0.211)
Municipal Election Yr.	52.323***	52.314***	52.339***	-9.881	52.361***
	(1.461)	(1.461)	(1.462)	(23.172)	(1.461)
Presidential Election Yr.	36.535***	36.634***	36.590***	36.850***	-48.199**
	(1.487)	(1.488)	(1.492)	(1.489)	(22.810)
PT Mayor/Coal. Won	6.301***	6.360***	12.188	6.075***	6.140***
	(1.221)	(1.222)	(20.678)	(1.222)	(1.224)
$\mathbf{W}\cdot 2^{nd}$ Round x 2^{nd} Round		-0.008**	-0.010	0.003	-0.000
		(0.004)	(0.006)	(0.005)	(0.005)
PT Mayor/Coal. Won x $\mathbf{W}\cdot 2^{nd}$ Round			-0.243		
			(0.512)		
PT Mayor/Coal. Won x 2^{nd} Round			-0.043		
			(0.354)		
PT Mayor/Coal. x $\mathbf{W}\cdot 2^{nd}$ Round x 2^{nd} Round			0.003		
			(0.009)		
Municipal Election Yr. x $\mathbf{W}\cdot 2^{nd}$ Round				2.364***	
				(0.568)	
Municipal Election Yr x 2^{nd} Round				0.974**	
				(0.394)	
Muni. Elec. Yr. x $\mathbf{W}\cdot 2^{nd}$ Round x 2^{nd} Round				-0.038***	
				(0.009)	
Presidential Election Yr. x $\mathbf{W}\cdot 2^{nd}$ Round					1.883***
					(0.570)
Presidential Election Yr x 2^{nd} Round					1.261***
					(0.399)
Pres. Elec. Yr. x $\mathbf{W}\cdot 2^{nd}$ Round x 2^{nd} Round					-0.027***
					(0.010)
ln(Expenditures)	-7.381***	-7.407***	-7.419***	-7.514***	-7.534***
	(0.679)	(0.679)	(0.679)	(0.679)	(0.680)
% Extremely Poor	-1.261***	-1.254***	-1.256***	-1.249***	-1.185***
	(0.070)	(0.070)	(0.070)	(0.070)	(0.072)
% Rural Population	0.359***	0.363***	0.364***	0.366***	0.354***
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Mayor Age	-0.647***	-0.650***	-0.650***	-0.648***	-0.645***
	(0.064)	(0.064)	(0.064)	(0.064)	(0.064)
Second Term	3.608***	3.594***	3.595***	3.403***	3.573***
	(1.259)	(1.259)	(1.259)	(1.259)	(1.259)
Constant	199.397***	180.486***	178.579***	200.541***	210.215***
	(12.837)	(16.035)	(18.504)	(17.227)	(17.521)
N	37540	37540	37540	37540	37540
Municipalities	5549	5549	5549	5549	5549
R^2	0.22	0.22	0.22	0.22	0.22
χ^2	10757.90***	10762.60***	10763.06***	10833.94***	10787.32***

Note: Dependent variable is per capita infrastructure transfers. Random-effects regression with standard errors in parentheses. Two-tailed tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4.6: National elections do shift incumbent strategy (second round results)



To better visualize these differences, I plot the marginal effect of second round presidential vote for the PT across surrounding second round vote (on the left-side plot), and the marginal effect of surrounding municipalities' second round presidential vote across a municipality's second round vote (on the right-side plot) in Figure 4.6. In a non-presidential election year, increased support for the incumbent's party is associated with increased transfers to a municipality, but only when the municipality is surrounded by fairly weak support. In a presidential election year, there appears to be no conditioning relationship between municipal and surrounding incumbent support; in fact, as shown in Figure 4.6, the effect of previous incumbent support on transfers is not statistically significantly different from zero, no matter the level of surrounding support.

Overall, the results using the second round vote in presidential elections are largely similar to the first round results. I find that incumbents pursue a strategy of rewarding islands of support

rather than strongholds. However, unlike the first round results, I find that, when using the second round votes, the incumbent party does tend to shift its opportunistic strategy both during municipal as well as presidential elections, moving from rewarding islands to a much broader flow of transfers to municipalities that does not appear to be conditional on the level of support in the surrounding municipalities.

4.7 Discussion and conclusion

A number of important findings are worth discussing from the results above. First, as the literature on opportunism predicts, I find evidence that municipalities are rewarded for their support for the incumbent in the previous presidential election. Expanding extant models by incorporating surrounding patterns of municipal support, I also find that incumbents pursue a strategy of favoring islands of support rather than strongholds. That is, incumbents tend to shift more transfers to supporter municipalities surrounded by areas of weak support than to those surrounded by strong supporters. This suggests that incumbents are taking into account external factors—surrounding regional support—when considering how to allocate resources to a particular municipality.

I also found evidence of a political budget cycle effect. Across all model specifications, there was a marked increase in infrastructure transfers to municipalities in both municipal and presidential election years. This is consistent with a large literature suggesting that incumbents may opportunistically time transfers (Schady 2000; Akhmedov and Zhuravskaya 2004; Kwon 2005). Even more important, I find that the incumbent strategy shifts during municipal and presidential elections; incumbents reward islands of support and tend to punish non-supporting municipalities surrounded by strong supporters in off-municipal and off-presidential election years.[18] However, during elections, the effect that previous PT support has on transfers does not depend on a municipality's neighbors. That is to say, incumbents appear to not take surrounding geographic context into account during elections.

[18]Recall that the presidential election findings were only present when using election results from the second round of voting.

While an important step forward, there are a number of directions that could be investigated in future work. For one, I assumed that the spatial weights matrix was an inverse distance, such that spatially proximate municipality regions matter more than ones further away. While this is a common assumption in most applied work, there are reasons why spatial distance may not be the only driver of the placement of transfers.[19] In addition, while I operationalized party support through the use of the previous PT vote share in presidential elections, perhaps a measure of competition—such as win margin—is a better reflection of the way in which the national incumbent rewards or punishes sub-national municipalities.[20] Last, while I found evidence that the incumbent incorporates previous presidential support, mayoral partisan characteristics, and surrounding municipal support, there may be other factors that play into the incumbent's strategy.

The existing literature in distributive politics has tended to focus on geographic units in isolation. In this chapter, I expanded upon this by looking both across space and over time to argue that incumbents may strategically time where and when transfers are likely to occur. Pitting a "strongholds" hypothesis versus a "islands" one, I find evidence that incumbents tend to reward islands of support rather than regional strongholds. Moreover, I find evidence that this strategy switches in municipal and presidential election years. Taken together, these results suggest the need to take into account both neighboring spatial units, as well as the time relative to the election, when examining opportunistic transfers.

[19] As the title of Beck, Gleditsch and Beardsley (2006) suggests, "Space is more than geography." In the case of Brazilian municipalities, perhaps the relative size of the municipality (e.g. population, wealth) matters more for connectivity between municipalities than geographic distance alone.

[20] Of course, using second round election results is essentially a win margin measure, since there are only two parties.

5. CONCLUSIONS

5.1 Introduction

It has been over four decades since Nordhaus (1975) wrote what is largely considered to be the first quantitative article on political cycles. This subject has remained an important area of interest ever since. At its core lies a paradox regarding democratic accountability; on the one hand, elections are designed to hold politicians to account for their performance over their term in office. As the economic voting literature suggests, incumbents are largely evaluated on how well the macroeconomy performed, or how well-off an individual voter perceives they themselves have become. On the other hand, the very act of holding elections, coupled with the fact that voters tend to remember the more recent past, creates an incentive for the incumbent to manipulate various instruments just before an election to make voters feel that the economy is doing better than it actually is. Thus, the very elections used to hold politicians accountable create perverse incentives to change behavior during elections.

Much ink has been spilt about the various tools incumbents might use, such as monetary or fiscal policy—as well as other routes—such as entering into conflicts (to create a "rally-round-the-flag" effect), boosting employment, speeding along government contracts, and so on. Ultimately, whether one feels such manipulation is "good" or "bad" depends on context; passing land reform policies, as shown in the third chapter, may truly have been in the voters' interests, but it was in the incumbent's interests to time their passage strategically. On the other hand, boosting the money supply (and therefore increasing price rises) probably benefits very few voters over the long-term.

In addition to the context on the normative implications on political cycles, a substantial body of literature now exists on the contextual factors that make political cycles more or less likely. For example, institutional constraints on fiscal behavior, the transparency of government, and the level of development are just three examples of important conditioning effects. Despite such a large body

of literature, in this dissertation I have addressed three significant gaps. Below, I briefly summarize each of the substantive chapters. I conclude by offering ideas on fruitful areas of future research.

5.1.1 What do we really know about political budget cycles?

In the second chapter, I used a comprehensive meta-analysis of the political budget cycle literature in order to establish whether or not budgetary instruments such as expenditures, revenues, debt, and fiscal balance, increase or decrease around elections. This was motivated by the need to better summarize the literature, which is very large, and nearly impossible to summarize in a single literature review. Although a number of scholars have written excellent qualitative literature reviews, even these articles face a number of issues. For one, they face a difficulty aggregating the results and accounting for the "quality" of a study. If one article finds statistically significant evidence for political budget cycles but another does not, what can we substantively conclude? Is one article more convincing (theoretically, methodologically) than another, and how can we take this into account? In other words, given numerous studies on political budget cycles, what is the best way to aggregate the results of individual studies to make broad statements about what we know? The second issue with qualitative literature reviews is that it is difficult to make an "all else equal" comparison between articles, given that they are often conducted on different data, use different control variables, and might even use a different operationalization of the dependent variable.[1] How can we account for these differences when comparing studies?

To better quantify the literature on political budget cycles, I used a meta-analytic approach, popular in other fields, and growing in prominence in political science and economics. Taken as a whole, evidence suggests that there is statistically significant evidence that expenditures and debt increase during elections, while revenues and fiscal surpluses decrease. However, the magnitude of this effect, judged against other meta-analyses in the social sciences, is small. I also documented evidence of publication bias in the literature, which suggests that higher-ranked (by impact factor)

[1] Although these are probably the two most important issues, a number of others exist. There is no way to identify, or account for, publication bias. Study-specific heterogeneity cannot be identified or characterized. In addition, it is hard to be comprehensive; given journal page limits, one cannot write about every article in the literature. Thus, subjective author biases might also play a role in which articles are appearing in a qualitative meta-analysis.

journals tend to publish "more significant" findings. However, a statistically significant political budget cycle effect remains even after accounting for such bias.

In the second chapter I was also able to characterize how much a particular decision about data, variables, or methods in a particular article influences the relationship between budgets and elections. Using Bayesian model averaging in the context of a meta-regression analysis, I found that incumbents tend to consistently manipulate aggregate expenditures—and not, as much of the literature contends—highly disaggregated expenditures such as education or health. I also found that political budget cycles are robust to many methodological and data-specific choices, such as employing fixed or random effects, or more fine-grained coding of the election year. Last, in line with numerous studies, the results in the second chapter suggest that a number of factors, such as democracy and economic development, probably condition the relationship between elections and budgets.

5.1.2 Are there budget cycle alternatives?

In the third chapter, I proposed a theory of political policy cycles. If we expect that fiscal and monetary manipulation takes place, why not other policies that matter to voters? I specifically focused on the passage of land reform legislation in the context of Indian states. This case was selected since it should be one of the most likely contexts in which to observe political policy cycles, should they exist. There are several reasons for this. Land reform has been an important redistributive policy in India that is salient to voters, attributable to the government in power, and low-cost to the incumbents, since wealthy landowners bear the brunt of the costs. Using aggregate data, I found that land reform policies are consistently passed in the year before a state election. This finding remained robust to a variety of alternative specifications, such as controlling for ideology, political competition, proxies for landless voters, and accounting for states that have had historically left-leaning policies. I also used historical survey evidence to show that land reform has remained a salient issue to individuals, even in states that have passed previous reforms. This suggests that redistributive policies are a tool used by incumbents during elections, and an important alternative to fiscal manipulation.

5.1.3 Does space matter in addition to time?

In the fourth chapter, I used theories from distributive politics to help explain why sub-national transfers may shift around elections not only in time, but also in space. The literature on distributive politics has debated on whether incumbents reward their political bases of support, or instead target geographic areas of marginal support in order to win the next election. Adapting these two competing theoretical claims, I have argued that this literature has left out two important components. First, incumbents targeting localities for distributive benefits may focus not only on the electoral performance of that locality, but also the electoral performance of the surrounding localities in deciding how to reward them with opportunistic goods. Second, not only are these goods more likely to be provided during elections (as the political budget cycle literature suggests), the spatial targeting strategy used by incumbents might shift during elections.

Using data on intergovernmental transfers to Brazilian municipalities, in the fourth chapter I found that incumbents appear to take into account the electoral performance of surrounding municipalities when handing out transfers. Greater support for the incumbent's party in the previous presidential elections is associated with larger transfers, especially when a municipality is surrounded by municipalities that have not supported the incumbent. In line with the political budget cycle literature, I also found that transfers to municipalities increase during both presidential and municipal election years.[2] Last, the findings suggest that incumbent strategy shifts during elections. In off-election years, incumbents reward "islands" of support (supporter municipalities surrounded by low-supporting municipalities). However, during election years, especially municipal ones, incumbents shift their opportunistic strategy towards rewarding past political support, regardless of whether a municipality is surrounded by strong supporters or weak supporters. This suggests that incumbents are aware of both time and space during elections, and shift their strategy accordingly.

[2]Recall that these were staggered so that municipal elections occur every four years, two years after a presidential election.

5.2 Future directions

Despite the three contributions herein, there still remain a number of other areas for future research on political cycles. I address five different areas below. For some proposed research areas, such as cross-governmental linkages and compositional spending, data already exist and a few scholars have made important inroads in these areas. Others, such as elite behavior or the impact of political cycles on elections, may be highly endogenous, or data may be difficult to gather. Regardless, these five areas represent some of the most promising avenues of future research on political cycles.

5.2.1 Compositional spending

As described in the meta-analysis chapter, much of the recent work on political budget cycles has shifted from aggregate indicators, such as total expenditures or fiscal balance, to disaggregated ones such as capital, current, health, and education expenditures. Such disaggregation is a welcome improvement, since it helps narrow down exactly where increases in expenditures (or decreases in revenues) might occur during elections. Yet selecting a particular category may miss important changes in other, overlooked categories. As Kramon and Posner (2013, p. 467) contend when discussing the field of distributive politics more broadly, the conclusions reached by a particular scholar, "will be as much a function of the outcome they happen to select as of the general patterns of political behavior that they are trying to understand."[3] In other words, picking specific categories may either downplay or—more likely given author subjectivities and bias towards significant findings—emphasize sizable movements in single categories that are of little substantive significance when looking at the entire budget.

A study examining *all* budgetary components would get around these critiques by analyzing how all allocations change simultaneously around elections. Although scholars have looked at the total change of budgetary categories around elections (Brender and Drazen 2013), they have not modeled simultaneous changes in the composition of budgets during elections (i.e., modeling

[3]The authors examine four dependent variables across studies examining six African countries, finding that the conclusions reached about whether ethnic favoritism exists depends to a large part on which country is being examined and which outcome (e.g., water provision, infant life expectancy) is used as the dependent variable.

how health, education, welfare, defense spending, etc., change around elections). Recently, a number of methodological approaches have been designed to handle this type of data (Philips, Rutherford and Whitten 2015, 2016*a*,*b*; Lipsmeyer, Philips and Whitten Forthcoming). Examining how budgets change around elections, not just particular categories, would be a welcome addition to the literature.

5.2.2 Cross-national linkages

Are political budget cycles more likely, or larger in magnitude, at the regional or local level? As discussed in the meta-analysis chapter, there has been no theorizing as to why this may be the case. One reason might be due to linkages (partisan or some other type) between the local and national incumbents. This might provide an incentive for further manipulation; as Dubois (2016, p. 249) points out, "one might think that [political budget cycles] at the local level would be reinforced if the local government shared the same political opinions as the national government." That is to say, linkages allow the national government to channel funds to choice localities, and localities use these funds around elections to appeal to voters. In federal systems such as the US and Brazil, there are multiple levels of government with the potential to create political cycles (e.g., national, state, local), and important relationships between each level that may make these cycles more or less likely. Although there has been some work on accounting for such linkages (e.g., Khemani 2004; Pepinsky 2007; Rumi 2014; Ribeiro and Jorge 2015)—and I control for them in the third and fourth chapters—this phenomenon remains understudied.

5.2.3 Elite behavior

Although the workhorse competency model of political budget cycles assumes that the incumbent is either competent or incompetent, much more remains to be done in terms of how elites behave in terms of their decision to manipulate. Are certain types of incumbents more likely to manipulate, such as men versus women incumbents, new leaders, incumbents in new democracies, or partisan differences? In addition, when in the election cycle does the incumbent decide that they need to manipulate, and what goes into their strategic decision-making about how much to manipulate?

Other literatures focus on the forces that change elite behavior. For instance, in the work on economic voting, there is evidence that parties shift their strategies in response to external stimuli (Green and Hobolt 2008; Green-Pedersen and Mortensen 2010; Adams and Somer-Topcu 2009), and that parties change their economic emphasis during campaigns in response to changing economic conditions, as well as the campaign strategies of other parties (Williams, Seki and Whitten 2016). Such strategic behavior could be further incorporated into the work on political cycles.

5.2.4 Individual-level evidence

Articles analyzing disaggregated expenditures have argued that incumbents will try to manipulate certain, "visible" policies that appeal to voters (Gonzalez 2002; Khemani 2004; Chang 2008; Aidt, Veiga and Veiga 2011; Katsimi and Sarantides 2012; Enkelmann and Leibrecht 2013). However, as shown in the meta-analysis chapter, there is very little evidence in support of a particular visible category of revenues or expenditures that incumbents consistently manipulate during elections. Moreover, there is no research in the political cycle literature on the type of fiscal categories voters are most likely to focus on during elections. Are voters more likely to notice an increase in transfer payments than an increase in road construction, for instance? More importantly, is it likely to influence their vote? While numerous scholars have staked out a theoretical expectation on this without much evidence, survey evidence—perhaps even a survey experiment—of particular policies that become more salient to voters around elections would help shed light on the type of policies that are truly visible and likely to be manipulated.

5.2.5 Impact of political cycle on elections

While it is clear that incumbents manipulate policies around elections, does such manipulation have an effect on electoral outcomes? That is to say, does fiscal or monetary intervention make enough of a difference in elections to turn an incumbent defeat into a victory? A few scholars have flipped the causal arrow around in this manner. For instance, Klomp and De Haan (2013*c*) examine if political budget cycles increase the probability of re-election, while Brender and Drazen (2008)

find no relationship between increases in fiscal deficits and re-election prospects (see also Veiga and Veiga 2013).

Despite some research into the effect that political cycles have on re-elections, a number of issues remain. First, there may be non-linear or threshold effects, such that a large enough change in a fiscal category, such as expenditures, has the ability to move election results. Second, as discussed above, certain visible categories might be more efficient at shifting votes for a given amount of manipulation. There is a clear need for further investigation into the electoral "boost" that political cycles provide.

5.3 Manipulating the masses

> "*You can fool all the people some of the time, and some of the people all the time, but you cannot fool all the people all the time*" —Abraham Lincoln

Are politicians "manipulating the masses?" As evidenced by the substantive chapters in this dissertation, the answer appears to be yes. As shown in the second chapter, taking the literature as a whole, there is statistically significant (though substantively small) evidence that expenditures and debt rise during elections, while revenues and fiscal surpluses fall. All of this suggests that incumbents use policies at their disposal to increase their likelihood of re-election.

However, far from being a blunt tool, I have shown that incumbents are often sophisticated in their varied use of tools of manipulation. As discussed in the third chapter, incumbents may pursue alternative policies to fiscal or monetary manipulation, such as passing redistributive policies around elections that appeal to voters. In the fourth chapter, I showed that incumbents do not simply allocate transfers to municipalities during elections, but follow a careful strategy of targeting supporter municipalities surrounded by non-supporters in off-election years, and move towards broad-based rewards (based on previous presidential support) in election years. Not only do politicians appear to use political cycles in order to enhance their re-election prospects, they appear to adapt their strategy in order to continue to do so.

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Yadav, Yogendra. 1999. "Electoral politics in the time of change: India's third electoral system, 1989-99." *Economic and Political Weekly* pp. 2393–2399.

Yoshino, Naoyuki and Tetsuro Mizoguchi. 2010. "The role of public works in the political business cycle and the instability of the budget deficits in Japan." *Asian Economic Papers* 9(1):94–112.

Zaidi, A Moin. 1985. *Not by a class war: A study of Congress policy on land reforms during the last 100 years*. India: Publication Dept., Indian Institute of Applied Political Research.

APPENDIX A

A.1 Studies included in meta-analysis

Table A.1 shows the studies included in the main meta-analysis.

Table A.1: Included studies

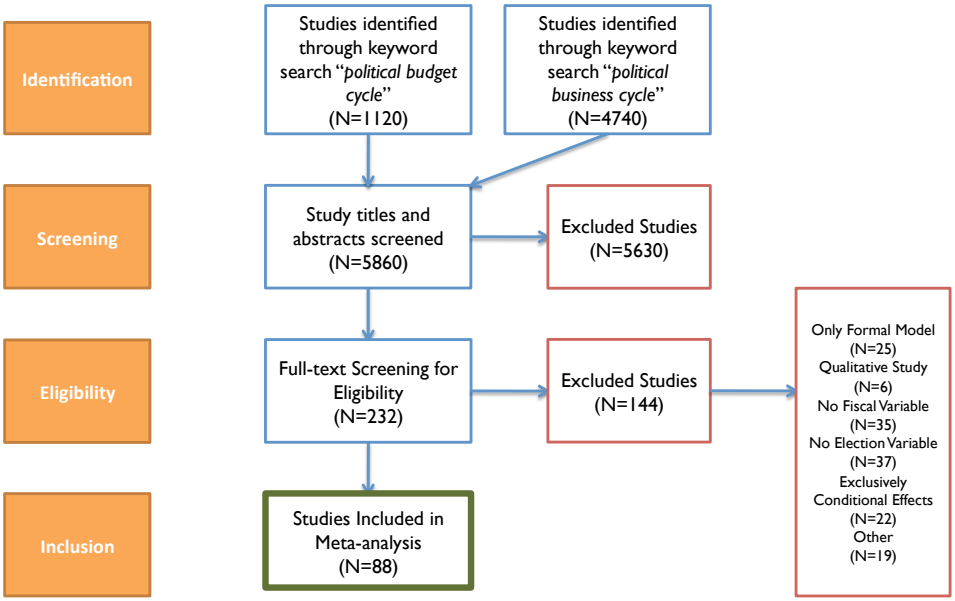
Schady (2000); Schuknecht (2000); Wibbels (2000); Harrinvirta and Mattila (2001); Kneebone and McKenzie (2001); Neck and Getzner (2001); John and Ward (2001); Goff and Tollison (2002); Rodden and Wibbels (2002); Svorny and Marcal (2002); Galli and Rossi (2002); Block (2002); Gordin (2002); Gonzalez (2002); Akhmedov and Zhuravskaya (2004); Baleiras and Costa (2004); Binet and Pentecôte (2004); Khemani (2004); Lambrinidis et al. (2005); Brender and Drazen (2005); Kwon (2005); Chaudhuri and Dasgupta (2005); Ames et al. (2005); Alt and Lassen (2006); Chaudhuri and Dasgupta (2006); Haber and Neck (2006); Mink and de Haan (2006); Rose (2006); Shi and Svensson (2006); Geys (2007); Remmer (2007); Tujula and Wolswijk (2007); Pepinsky (2007); Veiga and Pinho (2007); Veiga and Veiga (2007); Chang (2008); Klašnja (2008); Bercoff and Meloni (2009); Chang et al. (2009); Streb et al. (2009); Vergne (2009); Sáez and Sinha (2010); Drazen and Eslava (2010); Hagen (2010); Hyde and O'Mahony (2010); Krishnakumar et al. (2010); Peters (2010); Potrafke (2010); Barberia and Avelino (2011); Efthyvoulou (2011); Dahlberg and Mörk (2011); García-Sánchez et al. (2011); Jochimsen and Nuscheler (2011); O'Mahony (2011); Park (2011); Sakurai and Menezes-Filho (2011); Sedmihradská et al. (2011); Bartolini and Santolini (2012); Benito et al. (2012); Efthyvoulou (2012); Hanusch (2012); Katsimi and Sarantides (2012); Padovano (2012) Streb et al. (2012); Veiga (2012); Benito et al. (2013); Bonfiglioli and Gancia (2013); Enkelmann and Leibrecht (2013); Dash and Raja (2013); Guillamón et al. (2013); Klomp and De Haan (2013*c*,*b*,*a*); Sjahrir et al. (2013); Wehner (2013); Aidt and Mooney (2014); Amable and Azizi (2014); García-Sánchez et al. (2014); Mačkić (2014); Nyblade and O'Mahony (2014); Padovano (2014); Petrarca (2014); Shelton (2014); Tepe and Vanhuysse (2014); Bee and Moulton (2015); Getzner (2015); Houlberg and Pedersen (2015); Neck, Haber and Klinglmair (2015); Ribeiro and Jorge (2015)

A.2 Details on research design

As discussed in Chapter 2, a specific set of criteria had to be met for a study to be included in the meta analysis. This can be grouped into four general stages, as shown in Figure A.1. In the identification step, searches for "political budget cycle" and "political business cycle" were

performed using both Google Scholar and Web of Science. A total of 1120 were identified for political budget cycles, and 4740 for political business cycles. Next, study titles and abstracts were screened. This excluded the vast majority of search results, since a substantial portion of Google Scholar results are often citations to unpublished papers, or conference papers that became articles, were not in English, or overlapped with the other search term.

Figure A.1: Decision tree for inclusion



Of the 232 studies that remained, a full-text screening for eligibility was performed. This involved reading the text and ensuring that the article: 1.) contained an empirical test, 2.) used a fiscal measure as the dependent variable, 3.) included some form of election variable 4.) did not

exclusively report an interaction between elections and some other variable. The studies that did not meet these criteria are in the "Excluded Studies" section below.

One interesting question is how the inability to get *all* available studies might affect the results.[4] We might expect that if working papers were to be included, the effect size might be smaller (towards zero). This is because of the tendency for journals to publish significant findings. In line with other meta-analyses, (Doucouliagos and Ulubaşoğlu 2008), I did not include working papers. There were several reasons for this. First, working papers have not yet gone through the peer review process, so we might expect these studies to be of lower quality. Second, with nearly 1200 estimates of the political budget cycle effect, the dataset I constructed was large by meta-analytic standards (Stanley and Doucouliagos 2012); this reflects the substantial size of the literature, both in number of articles and length of time studies have been published. Were the literature on political budget cycles less developed, considering working papers might be a good strategy. Last, unless the overlooked studies were non-randomly distributed along the funnel plot (which plots the calculated partial correlation against precision), they should not exert much influence on the average effect. Again, while we might expect working papers to be centered around zero effect, it is hard to see how other overlooked studies—for instance, if a public finance article simply controlled for elections—would be anything except random. In fact, Stanley, Jarrell and Doucouliagos (2010) show that keeping only the top 10% most precise estimates often leads to better estimates of the true effect size. As shown in Table A.2, the trimmed estimated effect size is fairly close to the full-sample estimate. The average effect is slightly larger for expenditures and debt and smaller for revenues and fiscal balance. Thus, the effect sizes seen in Chapter 2 appears to be robust; it would take a very precise overlooked study to change the results in a significant way.

In Chapter 2 I opened discussion of the results from the effect size calculation through the use of a funnel plot. Another way to visualize the effect size, and to test if the empirical findings remain constant over time, is to regress the publication year on the partial correlations. This is weighted by the precision of the estimate. This is shown in Figure A.2. More precise estimates are indicated

[4] I thank an anonymous reviewer for posing this question.

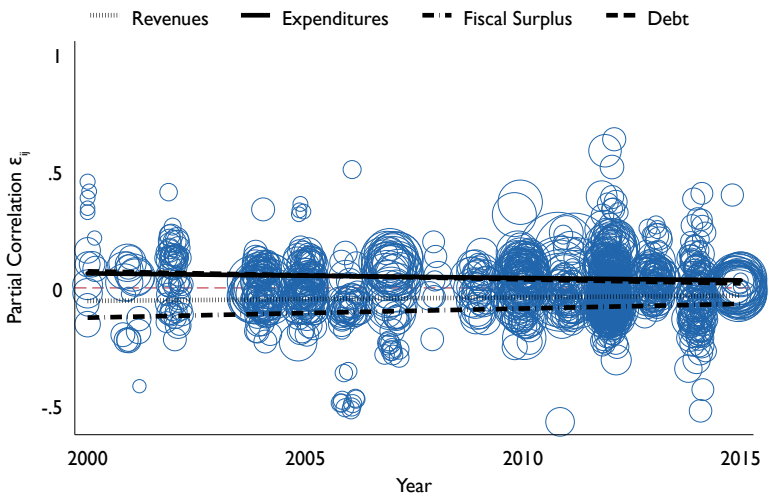
Table A.2: Does trimming the bottom 90% change the effect size?

Fiscal Variable	Full Sample (Obs.)	Top 10% Precise (Obs.)
Expenditures	0.046 (699)	0.060 (70)
Revenues	-0.047 (243)	-0.019 (25)
Fiscal Balance	-0.107 (234)	-0.073 (24)
Debt	0.011 (22)	0.082 (3)

Notes: Table shows unweighted means of calculated partial correlations, with number of observations in parentheses.

by larger circles. A number of interesting characteristics of the results can be gleaned from Figure A.2. The first is that extreme estimates of the effect size tend to lack precision. In addition, the variation in effect size appears to increase for more recent publications. As mentioned earlier, the average effect size is relatively small. Moreover, it does not appear to change much over time, although all effects look like they are converging on zero.

Figure A.2: Effect size over publication year



Note: Larger circles indicate increased model precision. Regression lines weighted by precision.

A.3 Addressing potential publication bias

In Chapter 2, the results of the meta-regression analysis suggested that publication bias may exist. To test whether the quality of a journal affects the precision of the estimates, I ran the following regression:

$$Precision_{ij} = \beta_0 + \beta_1 Impact\ Factor_{ij} + \varepsilon_{ij} \tag{A.1}$$

testing the hypothesis that journal quality for study i, model j (as proxied by the impact factor) is not related to the precision of the partial correlation (one over the standard error), $H_0 : \beta_1 = 0$. As shown by Table A.3, the journal impact factor is positive and statistically significant for expenditures and revenues, but not for fiscal surplus or debt. This suggests that higher quality journals publish studies of expenditures and revenues with higher levels of precision. To put these effects in perspective, consider that moving from an unranked journal (with an impact factor of zero) to the Quarterly Journal of Economics, the highest-ranked journal in the sample, would result in the standard error of the effect size decreasing from about 0.049 to 0.017 (when using expenditures). Given that the average partial correlation was near-zero, this means moving from an effect that is not statistically significant to one that is. In contrast, neither fiscal surplus or debt appear to suffer from publication bias according to Table A.3, since the impact factor is not statistically significant.

Table A.3: Publication bias: Journal quality on precision

Variable	**Expenditures** Coef. (Std. Error)	**Revenues** Coef. (Std. Error)	**Fiscal Surplus** Coef. (Std. Error)	**Debt** Coef. (Std. Error)
Journal Impact Factor (β_1)	6.701*** (0.939)	3.167** (1.578)	-1.490 (1.257)	-9.002 (7.769)
Constant (β_0)	20.595*** (1.388)	21.330*** (2.095)	25.425*** (1.459)	41.493*** (6.317)
Observations	699	243	234	22
R^2	0.07	0.02	0.01	0.06

Notes: Dependent variable is precision. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Next, I follow the suggestion of Stanley and Doucouliagos (2012) to test for publication bias using the following regression:

$$t_{ij} = \beta_0 \frac{1}{SE_{ij}} + \beta_1 + v_{ij} \tag{A.2}$$

where the t-statistic of the partial correlation for model i and study j is regressed on the coefficient on the standard error of the effect size divided by itself, β_1, a constant term, β_0, and an error term, $v_{ij} = \frac{\varepsilon_{ij}}{SE}$. These results are shown in Table A.4. Also known as the funnel asymmetry test (FAT), it is used to examine asymmetries in the t-statistic across the precision of the estimates. Such asymmetry is indicative of publication bias. As shown in the table, the coefficient on the standard error, β_1, is negative and statistically significant for expenditures and debt. Both revenues and fiscal surplus have positive standard error estimates, although this effect is statistically significant for only fiscal surplus. This indicates that expenditures, fiscal surplus, and debt appear to have effect sizes that may be distorted by publication bias.

Table A.4: Publication bias: FAT-PET

	Expenditures	**Revenues**	**Fiscal Surplus**	**Debt**
	Coef.	Coef.	Coef.	Coef.
Variable	(Std. Error)	(Std. Error)	(Std. Error)	(Std. Error)
Standard Error$_{ij}$ (β_1)	-60.455***	0.624	20.246***	-121.969***
	(7.579)	(5.910)	(4.269)	(31.055)
Constant (β_0)	4.261***	-1.007***	-3.246***	5.873***
	(0.318)	(0.262)	(0.202)	(1.005)
Observations	699	243	234	22
R^2	0.08	0.00	0.09	0.44

Notes: Dependent variable is the t-statistic of the model-study partial correlation. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table A.4 we can also test for the significance of of the constant, which is known as the precision effect test (PET). Rejection of the null hypothesis that $\beta_0 = 0$ indicates that despite publication bias, there exists a true underlying effect. Or, in other words, when publication bias equals zero, there is still an effect statistically significantly different from zero. The null hypothesis can

be rejected for all four dependent variable categories, in the expected theoretical directions; despite publication bias, there appears to be a genuine increase in expenditures and debt during elections, and a decrease in revenues and fiscal surplus.

Since publication bias and an underlying effect appear to exist, as well as an underlying effect, I present the precision-effect estimate with standard error (PEESE) test in Table A.5. It provides a better estimate of the underlying effect in the presence of publication bias (Stanley and Doucouliagos 2012). Weighting by the standard error as with the FAT-PET test, but replacing the standard error on β_1 with the variance yields:

$$t_{ij} = \beta_0 \frac{1}{SE_{ij}} + \beta_1 SE_{ij} + v_{ij} \tag{A.3}$$

A significant β_0 provides evidence that there is an underlying effect of political budget cycles. As evidenced by the constant, β_0 remains positive and significant for expenditures and debt, and negative and significant for revenues and fiscal surplus. This suggests that the underlying effect seen in the literature remains robust to publication bias. Moreover, this effect remains in the expected theoretical direction and is statistically significant across all models.

Table A.5: Publication bias: PEESE

	Expenditures	**Revenues**	**Fiscal Surplus**	**Debt**
	Coef.	Coef.	Coef.	Coef.
Variable	(Std. Error)	(Std. Error)	(Std. Error)	(Std. Error)
Variance$_{ij}$ (β_1)	-357.239***	14.967	106.617***	-1029.085**
	(72.906)	(63.683)	(33.163)	(416.913)
Constant (β_0)	2.775***	-1.011***	-2.647***	3.627***
	(0.220)	(0.166)	(0.125)	(0.769)
Observations	699	243	234	22
R^2	0.03	0.00	0.04	0.23

Notes: Dependent variable is the t-statistic of the model-study partial correlation. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.4 Excluded studies

Table A.6 documents the studies that failed to make it past the eligibility stage, along with the reason for exclusion.

Table A.6: Studies excluded from analysis

Formal model
Tuinstra (2000); Baleiras and Santos (2000); Ghate and Zak (2002); Gavious and Mizrahi (2002); Economides et al. (2003); Dhami (2003); Streb (2005); Kayser (2005); Sieg (2006); Aidt and Dutta (2007); Beniers and Dur (2007); Candel-Sánchez (2007); Saporiti and Streb (2008); Biswas and Marjit (2008); Martinez (2009); Bonomo and Terra (2010); Garrí (2010); Yoshino and Mizoguchi (2010); Gersbach (2004); Hanusch (2012); Streb and Torrens (2013); Ales et al. (2014); Ferré and Manzano (2014); Hanusch and Magleby (2014); Findley (2015)
Qualitative
Franzese (2002); Shi and Svensson (2003); Eslava (2011); Lupu and Riedl (2013); Percic and Apostoaie (2014); Halász (2014)
No fiscal variable

Table A.6 Continued

Kiefer (2000); DeRouen and Heo (2000, 2001) [no. of defense contracts]; Toma and Cebula (2001); Patterson and Beason (2001) [announcement of stimulus package]; Heckelman (2001); Heckelman (2002); Erlandsson (2004); Krause (2005); Heckelman and Wood (2005); Andrikopoulos et al. (2006); Berlemann and Markwardt (2006); Heckelman (2006); Sadeh (2006); Özatay (2007); Miliauskas and Grebliauskas (2008); Tepe and Vanhuysse (2009) [no. new teachers]; Milani (2010); Ferris and Voia (2011); Helland (2011); Potrafke (2012); Canes-Wrone and Park (2012); Ahlquist (2010) [social pacts]; Coelho et al. (2006) [local employment]; Rose and Smith (2012) [revenue forecast bias]; Brogan (2012) [forecast errors]; Brender and Drazen (2013) [change of leader's post-election effect on spending]; Geys (2013) [employment]; Mechtel and Potrafke (2013) [growth in job-creation scheme enrollment]; Anessi-Pessina and Sicilia (2015) [revenue misrepresentation] Baskaran, Min, and Uppal (2015) [electricity provision]; (Benito, Guillamón and Bastida 2015) [deviation from expected]; Chiripanhura and Niño-Zarazúa (2015) [GDP growth]; Katsimi and Sarantides (2015) [probability of re-election]; Konstantakis et al. (2015) [GDP cycle]
No election variable

Table A.6 Continued

Ergun (2000) [1 and 2 periods before election]; Esaw and Garratt (2000); Padovano and Venturi (2001) [election variable is last term]; Reddick (2002) [1 and 2 periods before election]; Remmer (2002); Matschke (2003) [countdown variable for election]; Easaw and Garratt (2006); Cerda and Vergara (2008); Guo (2009); Benito and Bastida (2009); Balassone et al. (2010); Bogdan et al. (2010) [election variable is for term continuation]; Luo et al. (2010); Klein (2010) [diff. in diff.]; Tepe and Vanhuysse (2010) [no election variable, hazard model]; Albuquerque (2011) [election variable is decadal election count]; Alt and Lassen (2006); Tellier (2006) [election variable is time elapsed since last election]; Fujii (2008); Brender and Drazen (2008) [examine re-election prospects]; Schneider (2010) [only pre-election]; Javid et al. (2011); Bröthaler and Getzner (2011); Aidt et al. (2011) [dependent variable is election year distortion from trend]; Tutar and Tansel (2012) [dummy equals -1, 0, 1, in year before, year of, year after election, respectively]; Shahor (2013) [dummies for each election]; Benito et al. (2013) [no election period variable]; Bertelli and John (2013); Fiva and Natvik (2013); Veiga and Veiga (2013) [election dummies included but not shown]; Franklin et al. (2013); Cassette and Farvaque (2014); Kim and Kwon (2014); García-Sánchez et al. (2014) [election variable is integer count-down to next election]; Bodea and Higashijima (2015) [election variable is presidential or parliamentary dummy]; Cabaleiro-Casal and Buch-Gómez (2015)
Only conditional effects (i.e., interactions)
Clark and Hallerberg (2000); An and Kang (2000); Hallerberg et al. (2002); Block et al. (2003); Blomberg and Hess (2003); Buti and Noord (2004); Golinelli and Momigliano (2006); Rose (2008); Hiroi (2009); Mourão (2011); Gan et al. (2012); Vicente et al. (2013a, 2013b); Hanusch and Vaaler (2013); Bastida, Beyaert and Benito (2013); Klomp and de Haan (2013); Rumi (2014); Tsai (2014); Hanusch and Keefer (2014); Bojar (2015); Haga (2015); Klein and Sakurai (2015)
Other

Table A.6 Continued

Thames (2001) [no measure of uncertainty]; Treisman and Gimpelson (2001) [no regression, only F-test results]; Andrikopoulos et al. (2004) [no effect size reported]; Mierau et al. (2007) [logit]; Malley et al. (2007) [state-space]; Donahue and Warin (2007) [no measure of uncertainty, observations]; Lalvani (2008) [no regression]; Karagol and Turhan (2008) [VAR]; Brender and Drazen (2007) [results identical to Brender and Drazen (2005)]; Doležalová (2011) [unclear]; Kendall-Taylor (2011) [no regression]; Hayo and Neumeier (2012) [no measure of uncertainty]; Karakaş (2013) [cannot obtain journal]; de Haan and Klomp (2013) [results taken from Klomp and de Haan 2013]; de Haan (2014) [results taken from Klomp and de Haan 2013]; Pérez-Forniés et al. (2014) [no regression]; Benazić and Tomić (2014) [no regression]; Citi (2015) [unit of analysis is EU]

A.5 Probing the robustness of the effects

In Chapter 2, plots of the calculated effect sizes were given as:

$$\varepsilon = \frac{\sum(\varepsilon_{ij} N_{ij})}{\sum N_{ij}} \tag{A.4}$$

where the effect size is given by the sum of the study-model partial correlations, ε_{ij}, times the study-model number of observations, N_{ij}, divided by the sum of all observations. The number of observations was chosen as the weighting scheme since it is a commonly-used weight in the meta-analysis literature. In this section I probe the robustness of the results in Chapter 2 by considering alternative weights.

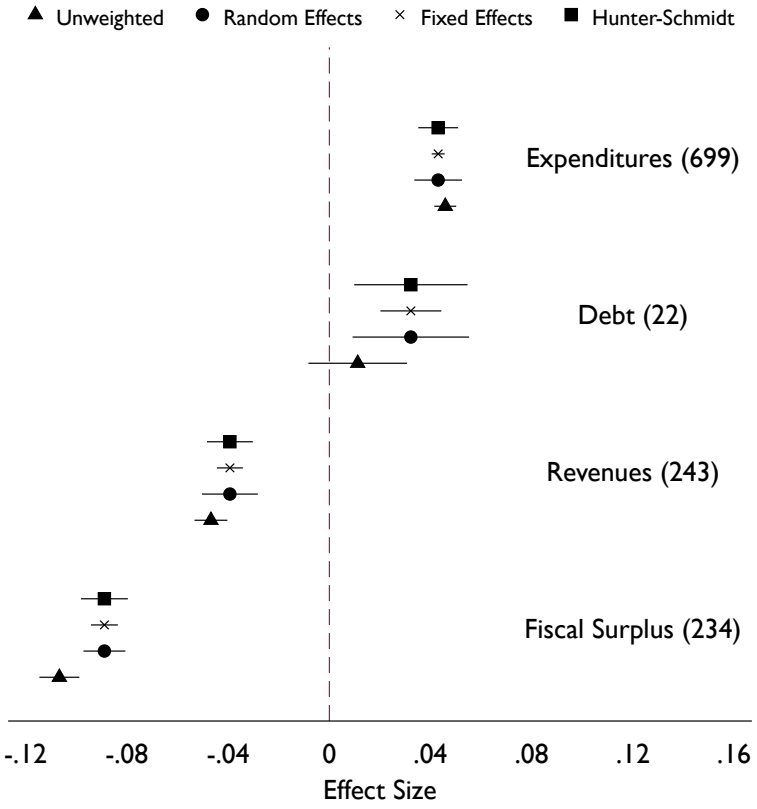
One of the alternative weighting schemes is the precision, or the inverse of the standard error of the calculated study-model effect size.[5] After changing N_{ij} to precision, I then plotted the overall effect size calculations, along with 95% confidence intervals calculated the same way as in Chapter

[5] Recall that the standard error of the partial correlation is given as

$$SE_{ij} = \sqrt{\frac{1-\varepsilon_{ij}}{df_{ij}}} \tag{A.5}$$

2.[6] The results are shown in Figure A.3. It is clear that the results remain robust to weighting by precision.

Figure A.3: The political budget cycle effect across four major categories (precision-weighted)

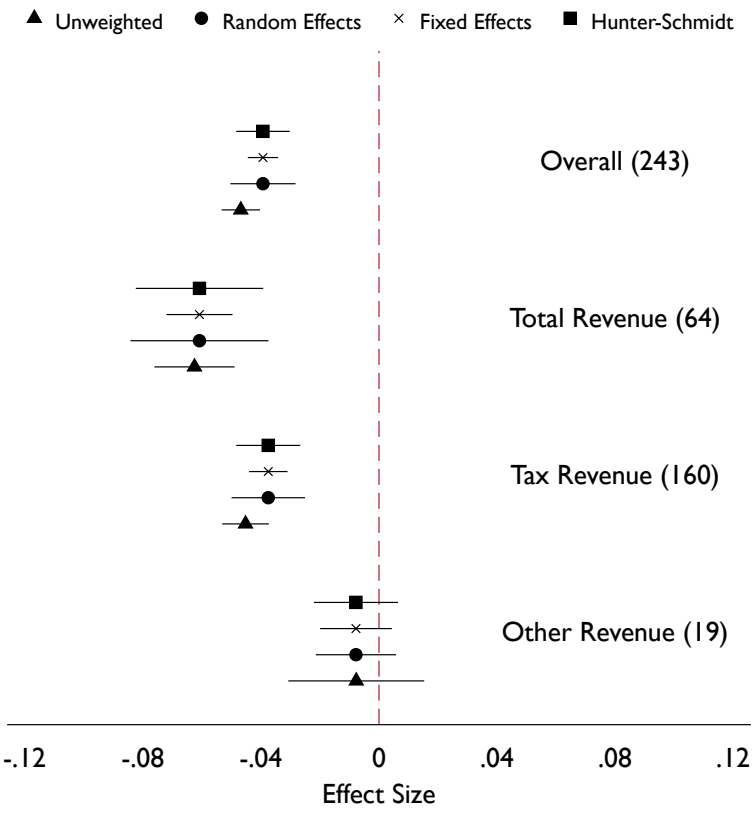


Notes: 95% confidence intervals reported.

I also re-ran the revenue and expenditure disaggregation calculations using precision as the weight. These are shown in Figures A.4 and A.5, respectively. As with the overall categories, the results appear to be robust to weighting by the precision of the partial correlations.

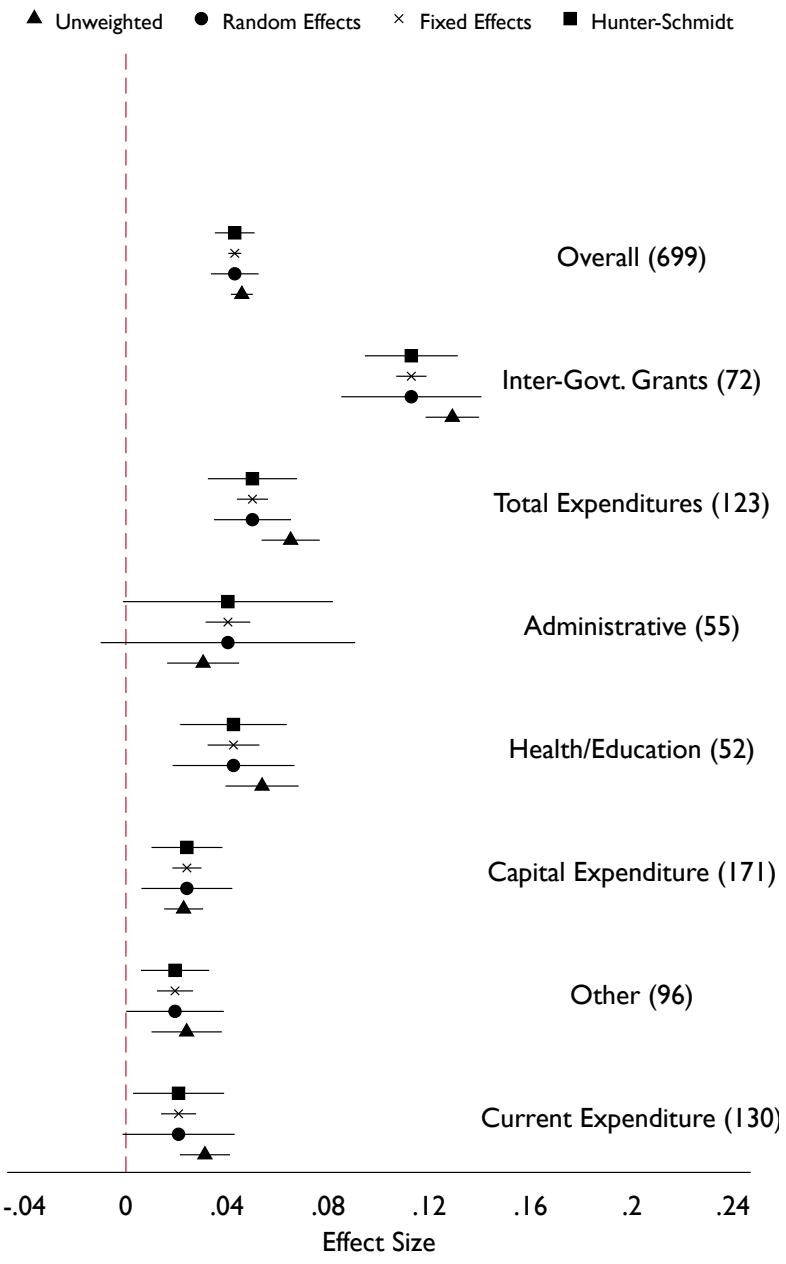
[6]These were unweighted, random effects, fixed effects, and the Hunter-Schmidt calculation.

Figure A.4: The political budget cycle effect: Disaggregation by Revenues (precision-weighted)



Notes: 95% confidence intervals reported.

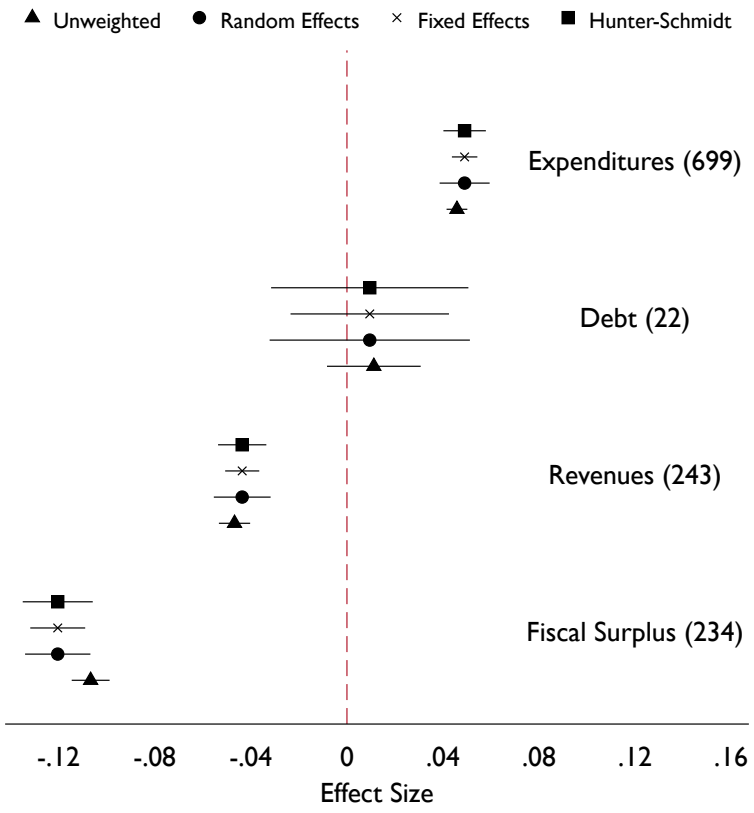
Figure A.5: Disaggregation by Expenditures (precision-weighted)



Notes: 95% confidence intervals reported.

Another alternative weighting scheme uses journal impact factors. Using the impact factors (from 2013) as the new N_{ij}, I recalculated the effect sizes.[7] The results are shown in Figures A.6 (four general categories), A.8 (expenditure disaggregation), and A.7 (revenue disaggregation). Once again, the results remain fairly robust to the alternative weighting scheme.

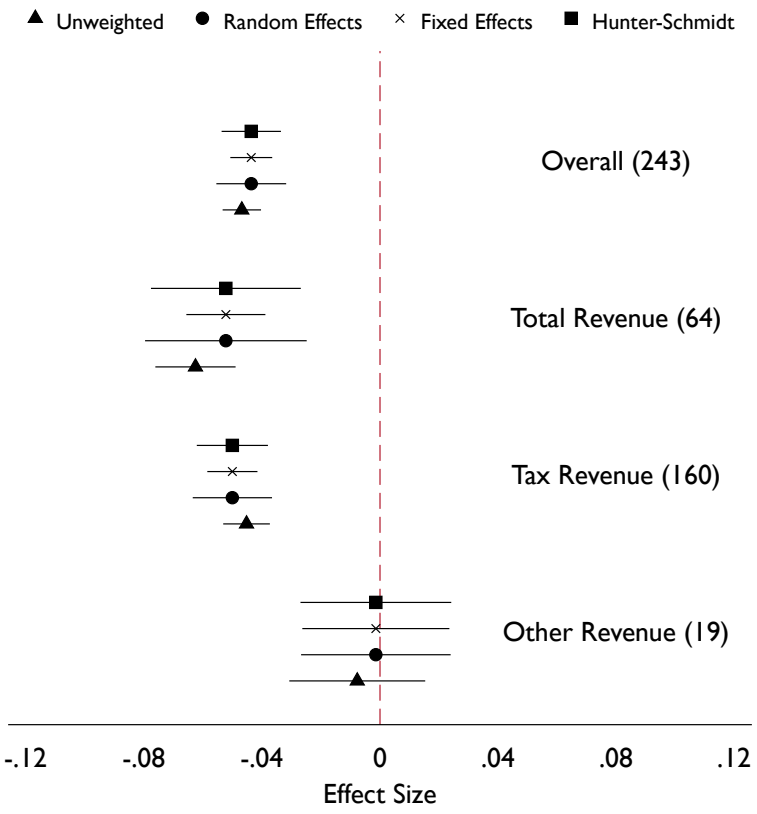
Figure A.6: The political budget cycle effect across four major categories (weighted by impact factor)



Notes: 95% confidence intervals reported.

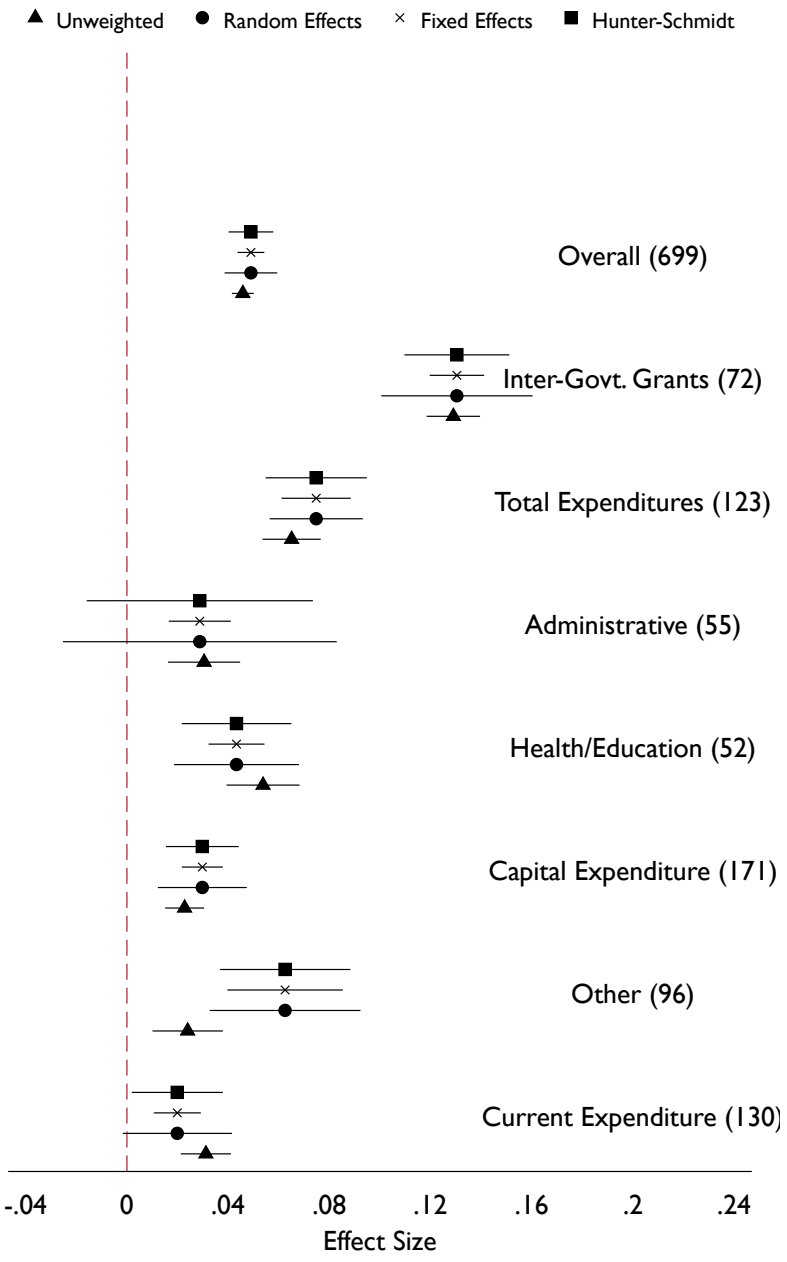
[7]Not all journals had impact factor scores, thus severely downplaying the influence of these partial correlations.

Figure A.7: The political budget cycle effect: Revenue disaggregation (impact factor-weighted)



Notes: 95% confidence intervals reported.

Figure A.8: The political budget cycle effect: Expenditure disaggregation (weighted by impact factor)



Notes: 95% confidence intervals reported.

A.6 Meta-regression analysis and bayesian model averaging

For all models using Bayesian model averaging in Chapter 2, two million draws were taken after a burn-in period of 500,000 draws. I used a beta-binomial model prior, with coefficient priors set to benchmark priors for the hyperparameter in Zellner's g-prior, as given by Fernandez, Ley and Steel (2001)

A graphical depiction of the posterior inclusion probabilities is shown in Figure A.9, A.10, and A.11 for the model of expenditures, revenues, and fiscal surplus, respectively. These are based on the 500 best models chosen via the Markov chain Monte Carlo (MCMC). To swap covariates when conducting the MCMC, a reversible-jump algorithm was used. Each row represents a different covariate eligible for inclusion in the final model. For example, for expenditures, those at the top (where the colored bars span the width of the figure) have the highest posterior inclusion probability. Red bars (lighter in grayscale) indicate that the covariate has a negative coefficient (i.e., the presence of this factor makes the resulting political budget cycle effect *smaller*). Blue bars (darker in grayscale) indicate that the coefficient is positive (i.e., the presence of this factor makes the resulting political budget cycle effect *larger*).

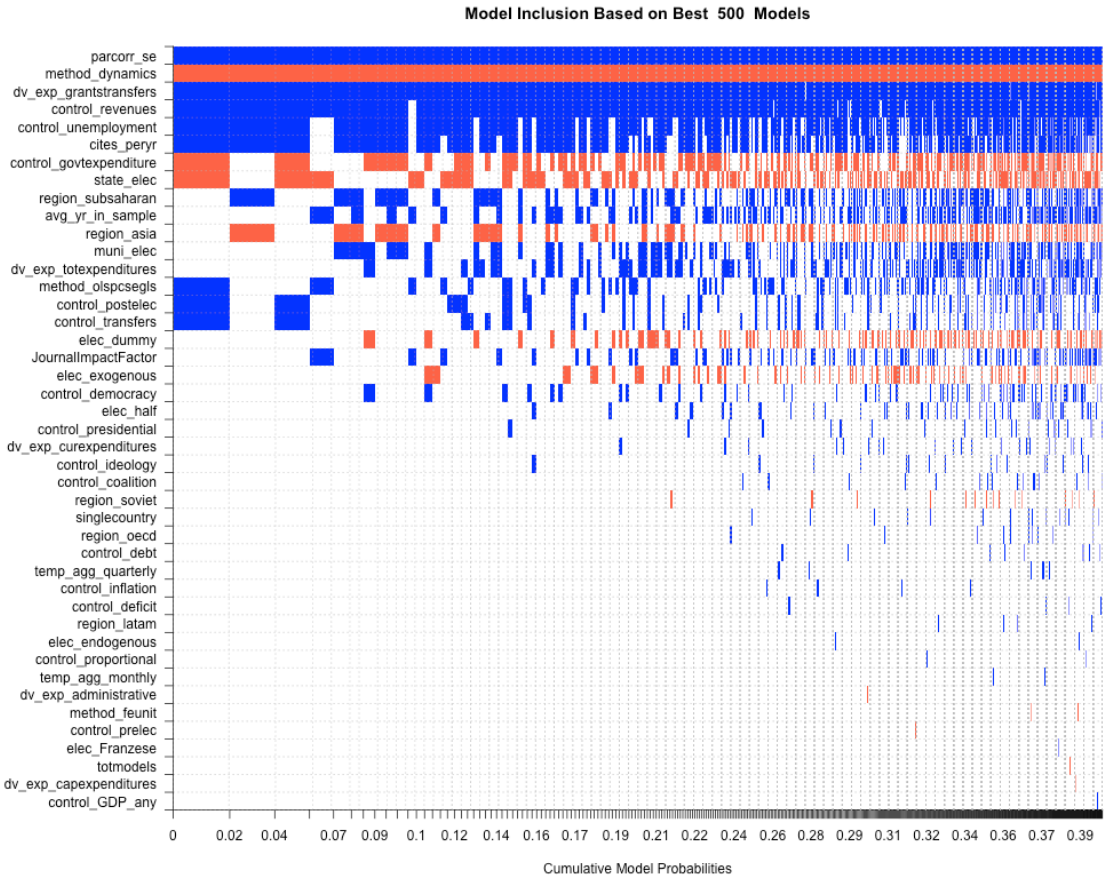
To probe the robustness of the covariate-swapping algorithm, the model was re-run using a birth-death algorithm. I found very similar results when using the alternative algorithm.

A.7 Coding decisions

This section details coding decisions made when coding variables for the calculation of the overall effect sizes, and for the meta-analysis. The first are dependent variables:

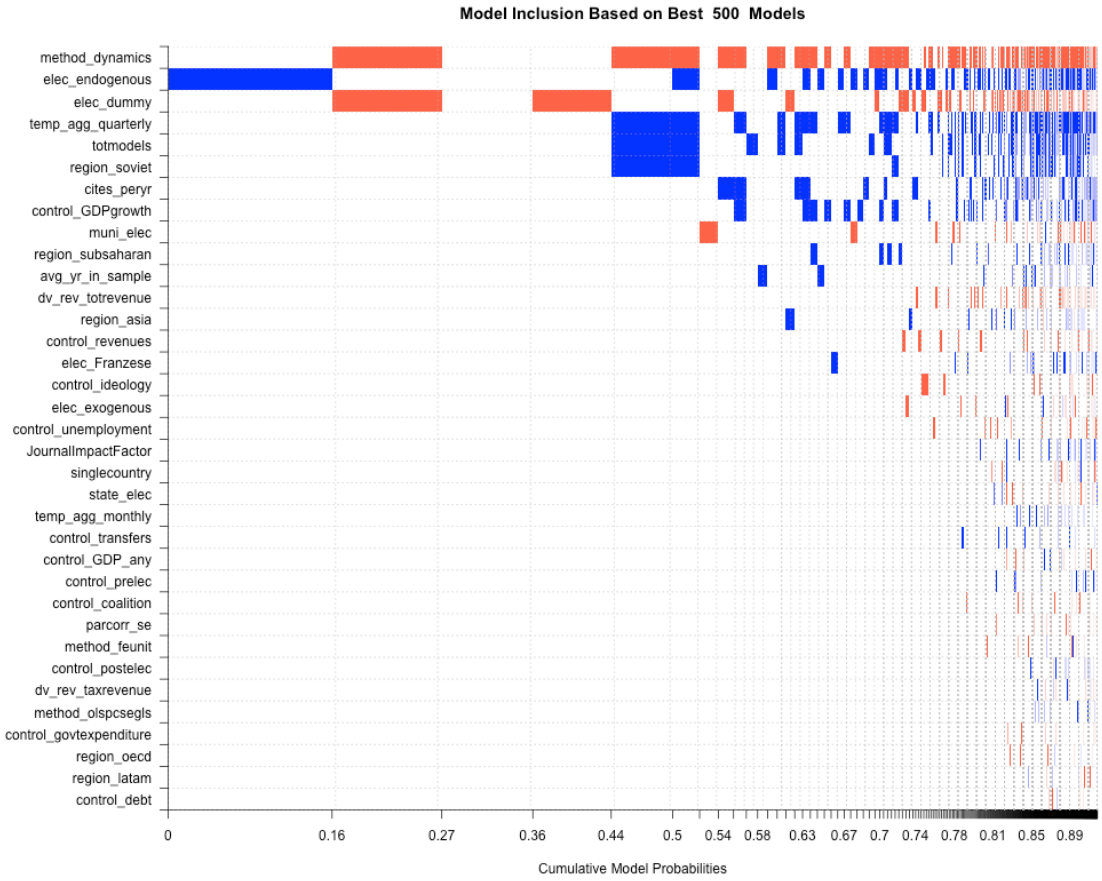
- Fiscal surplus: Dependent variable used in the analysis was deficit spending or fiscal surplus. The signs were reversed on deficits to indicate that increases correspond with an increased budget surplus.
- Debt: Dependent variable used in the analysis was government debt, or net claims on government.

Figure A.9: Posterior inclusion probabilities in model for each variable: Expenditures



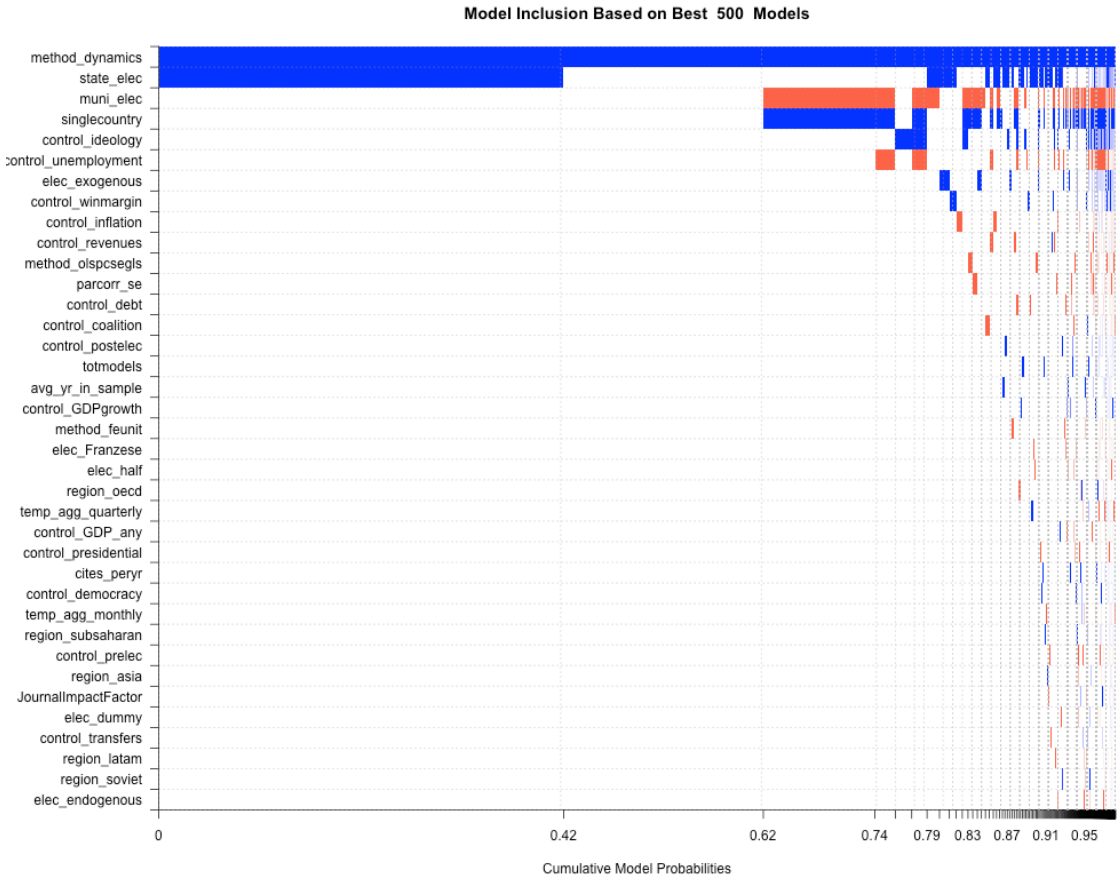
Notes: Figure shows the probability of variable inclusion in the model based on 500 best models for expenditures. Red bars (lighter in grayscale) indicate inclusion, but in the negative direction. Blue bars (darker in grayscale) indicate inclusion in the positive direction. The hyperparameter on Zellner's g-prior for regression coefficients set to a benchmark prior, and the model prior is set to a random prior. A reversible-jump algorithm used to swap covariates.

Figure A.10: Posterior inclusion probabilities in model for each variable: Revenues



Notes: Figure shows the probability of variable inclusion in the model based on 500 best models for revenues. Red bars (lighter in grayscale) indicate inclusion, but in the negative direction. Blue bars (darker in grayscale) indicate inclusion in the positive direction. The hyperparameter on Zellner's g-prior for regression coefficients set to a benchmark prior, and the model prior is set to a random prior. A reversible-jump algorithm used to swap covariates.

Figure A.11: Posterior inclusion probabilities in model for each variable: Fiscal surplus



Notes: Figure shows the probability of variable inclusion in the model based on 500 best models for fiscal surplus. Red bars (lighter in grayscale) indicate inclusion, but in the negative direction. Blue bars (darker in grayscale) indicate inclusion in the positive direction. The hyperparameter on Zellner's g-prior for regression coefficients set to a benchmark prior, and the model prior is set to a random prior. A reversible-jump algorithm used to swap covariates.

- Expenditures: Dependent variable used in the analysis was expenditures, further broken into sub-categories below.

- Revenues: Dependent variable used in the analysis was revenues, further broken into sub-categories below.

- Total Expenditures: Dependent variable used in the analysis was total expenditures. Other names for this were government consumption, consumption expenditures, non-interest expenditures, and general payments.

- Inter-governmental Grants and Transfers: Dependent variable used in the analysis involved inter-governmental grants or transfers. This included loans to other levels of government. Note that this *did not* include transfers to individuals, such as social security payments.

- Capital Expenditures: Dependent variable used in the analysis was capital expenditures. This included capital transfers, construction spending, development expenditures, investment in buildings and construction, and other infrastructure projects.

- Current Expenditures: Dependent variable used in the analysis was current expenditures. This included (current) development expenditures, spending on economic services, (current) grants, transfers to individuals such as social services or social security, payments to retirees, and spending on "culture".

- Administrative Expenditures: Dependent variable used in the analysis was administrative expenditures. This included wages and income to public sector employees, transfers to state-owned enterprises, police expenditures, and other administrative spending.

- Education and Health Expenditures: Dependent variable used in the analysis was education or health expenditures.

- Other Expenditures: Dependent variable used in the analysis was some "other" expenditure not listed above. Care was taken to choose the eight largest categories—this category is a catch-all term for any non-classifiable dependent variable. This includes spending on

agriculture, defense, security, subsidies, economic services, housing, environment, industry, irrigation, leisure, media, net lending, social expenditures, other transfers (to citizens), public services, and spending on water, energy, and communications. Often, these were vaguely defined—so although social expenditures could be capital or current, for instance, it was unclear and so listed as "Other".

- Total Revenues: Dependent variable used in the analysis was described as total government revenues. Often this was expressed as a percent of budget or GDP.
- Tax Revenues: Dependent variable used in the analysis was tax receipts for government. This included income, sales, and property taxes, for instance.
- Other Revenues: Dependent variable used in the analysis was some other type of revenue. This included fees, non-tax revenues, and money income.

Below is a list of other variables coded for the meta-regression analysis.

- Standard Error: The standard error of the calculated effect size for a given study-model.
- OECD: A dummy variable that takes on a value of one if the analysis included at least one country in the OECD.
- Latin America: A dummy variable that takes on a value of one if the analysis included at least one country from Latin America.
- Asia: A dummy variable that takes on a value of one if the analysis included at least one country from Asia.
- Sub-Saharan Africa: A dummy variable that takes on a value of one if the analysis included at least one country from Sub-Saharan Africa.
- Eastern Europe and the Former Soviet Union: A dummy variable that takes on a value of one if the analysis included at least one country from Eastern Europe or the former Soviet Union.

- Average Year: The average year in the sample, calculated as $\frac{MinYear+MaxYear}{2}$.
- Quarterly Aggregation: The temporal aggregation is quarterly. This variable is dichotomous.
- Monthly Aggregation: The temporal aggregation is monthly. This variable is dichotomous.
- Single Country: A dummy variable that takes on a value of one if the analysis is conducted on a single country. Most often, this means that the level of aggregation is either municipal or state.
- Municipal Aggregation: Level of analysis is at the lowest level of government—commonly a municipality. This variable is dichotomous.
- State Aggregation: Level of analysis is at the state or provincial level. This variable is dichotomous.
- Democracy: This is a dichotomous variable equal to one if the analysis controlled for democracy. This included some index of democracy (commonly the Polity score or an equivalent), or a democracy dummy.
- Coalition: This is a dichotomous variable equal to one if the analysis controlled for if the current government was in a coalition, or was a minority or majority government. Typically this was a dummy variable; although some controlled for the overall size of the coalition.
- Debt: This is a dichotomous variable equal to one if the analysis controlled for the debt of the unit of analysis (national government, state/province, or municipality). This was most commonly a continuous variable, although I included those who had budgetary restraint dummy variables or indebted dummy variables.
- Deficit: This is a dichotomous variable equal to one if the analysis controlled for the deficit. If the analysis controlled for the budget surplus (otherwise called the fiscal balance), I included it as well.

- Government Expenditures: This is a dichotomous variable equal to one if the analysis controlled for government expenditures. This could include total spending, non-defense spending, capital spending, or primary sector spending.
- Government Revenues: This is a dichotomous variable equal to one if the analysis controlled for government revenues. This could include total revenues, tax revenues, capital revenues, tax revenues per capita, privatization revenues, local-source revenues, and municipal taxes.
- Transfers: This is a dichotomous variable equal to one if the analysis controlled for grants, subsidies, bailouts, or other transfers from upper- to lower-levels of government.
- GDP: This is a dichotomous variable equal to one if the analysis controlled for economic output. This could include gross domestic product (GDP), GDP per capita, GDP gap between potential and real output, gross national product, as well as lags or leads of GDP.
- GDP Growth: This is a dichotomous variable equal to one if the analysis controlled for GDP growth.
- Ideology: This is a dichotomous variable equal to one if the analysis controlled for political ideology, either through a continuous measure or a dichotomous or trichotomous indicator.
- Inflation: This is a dichotomous variable equal to one if the analysis controlled for inflation, either actual, expected, or the change in inflation.
- Presidential: This is a dichotomous variable equal to one if the analysis controlled for a presidential system.
- Proportional: This is a dichotomous variable equal to one if the analysis controlled for a system of proportional representation.
- Unemployment: This is a dichotomous variable equal to one if the analysis controlled for unemployment.
- Win Margin: This is a dichotomous variable equal to one if the analysis controlled for either the vote share or margin of victory from the previous election.

- Fixed-effects Unit: Analysis used some form of unit fixed effects. In this sample of articles, this included unit fixed effects, regional fixed effects, weighted least squares with regional fixed effects, and a tobit model with fixed effects.

- Dynamics: Analysis used some form of dynamics using a lagged dependent variable. In this analysis, this included GMM and GMM-HLM models, error-correction models, pooled mean-group estimators, and autoregressive distributed lag (ARDL) models.

- OLS PCSE GLS: A catch-all category that included models estimated using ordinary least squares (OLS), OLS with panel-corrected standard errors (PCSE), generalized least squares (GLS), and GLS with autoregression corrected using the Cochrane-Orcutt procedure.

- Election Dummy: This is a dichotomous variable equal to one if the election variable used in the analysis is a dummy variable. This is the most basic election variable since it does not account for *when* the election occurred during the year.

- Election Half-Year: This is a dichotomous variable equal to one if the study accounted for the election year in the following way: the election variable equals one in the year of an election *only* if the election took place after June 30. If not, the year before the election is coded as the election variable. This indicator is not as coarse as the simple dummy variable, but not as specific as the Franzese indicator.

- Franzese: This is a dichotomous variable equal to one if the election variable used in the analysis uses the method of Franzese (2000). The resulting variable is equal to $\frac{M}{12}$ in an election year, where M is the month of the election.[8]

- Election Pre-Determined: This is a variable equal to one if the author(s) election variable was only for pre-determined...i.e., exogenous. This only applies to cross-national analyses.

- Election Early: This is a dichotomous variable equal to one if the author(s) election variable was only for elections called early...i.e., endogenous. This only applies to cross-national analyses.

[8]The period-before-election indicator, if included, is then equal to $1 - \frac{M}{12}$.

- Election$_{t+1}$: This is a dichotomous variable equal to one if the analysis controlled for the period after an election.

- Election$_{t-1}$: This is a dichotomous variable equal to one if the analysis controlled for the period before an election. To keep the comparison similar in terms of functional form (since this is the key independent variable of interest), I only included indicators the are bounded by zero and one. This excluded "counter" variables that equal 0, 1, 2,... for one year after, two years after,... the election. It also excluded a number of articles that created an indicator that could take on negative and positive values.

- Total Models: The total number of models (not including the appendix) that appear in the article.

- Cites per Year: The total number article cites (collected at the time of the meta-analysis) divided by the number of years since publication.

- Impact Factor: The impact factor of the journal (using 2013 impact factors).

APPENDIX B

B.1 Summary statistics and robustness tables

In Chapter 3, robustness checks were conducted on the monthly weighted coding. The results of the same robustness check using the alternative dummy variable coding for time relative to the election are shown in Table B.1. As in Chapter 3, the *Year Before Election* variable remains positive and significant throughout the various specifications. So too does the percentage of those owning no land, suggesting that larger numbers of land-poor citizens prompt the government to pass land reforms.

In Table B.2 I explore the effects of a number of context-specific variables to check the robustness of the findings above. All models now use the weighted monthly coding as well as state fixed effects unless noted otherwise (though results using the dummy variable coding are above). To investigate the effect of early elections, I add a dummy variable that equals one if there were *Early Elections* in that year. Early elections most commonly occur due to a vote of no confidence or a collapse of a majority coalition.[9] As shown in Model 7, accounting for early elections does not affect the previous findings. Model 8 controls for *President's Rule*, which occurs when, "the president of India, upon receipt of a report by the governor of the state or otherwise, may be satisfied that constitutional breakdown has occurred at the state level. This leads to the temporary imposition of President's Rule and, eventually, fresh elections" (Arulampalam et al., 2009, p. 10). Inclusion of this control does not change the results either.

Another control variable important to the Indian context is based off Khemani's (2007) *Strictly Affiliated* indicator, and shown in Model 9. This dummy variable equals one if the subnational party in government is the same as the party in government at the national level. Existing literature suggests competing expectations in regards to political control in India's federal system. On the one hand, a coattails effect may exist whereby state parties aligned with the central government benefit

[9] It can also be caused by the imposition of President's Rule.

Table B.1: Robustness: Dummy variable coding

	(1)	(2)	(3)	(4)	(5)	(6)
Year Before Election	0.92**	0.92**	0.94**	0.92**	0.94**	1.12**
Dummy Variable	(0.37)	(0.38)	(0.38)	(0.37)	(0.37)	(0.53)
Election Year	-0.30	-0.30	-0.31	-0.30	-0.31	-0.46
Dummy Variable	(0.52)	(0.52)	(0.52)	(0.53)	(0.52)	(0.88)
Single-Party Dominant	0.94*	0.89*	0.80	0.29	0.80	0.77
	(0.49)	(0.49)	(0.50)	(0.42)	(0.50)	(0.65)
Multiparty: Left-Center-Right	0.79	0.85	0.88	1.65*	0.87	-0.46
	(1.03)	(1.05)	(1.05)	(0.90)	(1.05)	(1.31)
Two-Party: Left-Center	1.17	1.22	1.09	0.64	1.13	0.60
	(1.62)	(1.61)	(1.62)	(0.59)	(1.61)	(1.73)
Two-Party: Center-Right	0.82	0.74	0.78	-0.40	0.78	0.45
	(0.92)	(0.92)	(0.93)	(0.66)	(0.93)	(1.21)
% Owning No Land	0.08***	0.08***	0.07**	0.04**	0.07**	0.12**
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.05)
Early Elections	0.79	0.81	0.92	0.90	0.91	1.15
	(0.70)	(0.70)	(0.71)	(0.71)	(0.71)	(1.08)
President's Rule		-0.55	-0.56	-0.58	-0.55	-0.58
		(0.68)	(0.69)	(0.67)	(0.69)	(0.86)
Strictly Affiliated			0.56	0.61	0.55	0.40
			(0.44)	(0.40)	(0.43)	(0.59)
West Bengal				0.81		
				(0.67)		
Constant				-4.02***		
				(0.77)		
State FE	YES	YES	YES	–	YES	YES
Time Splines	YES	YES	YES	YES	–	–
Lowest Smoother	–	–	–	–	YES	–
Year FE	–	–	–	–	–	YES
N	515	515	515	515	515	515
States	15.00	15.00	15.00	15.00	15.00	15.00
Log Lik.	-119.33	-118.98	-118.13	-145.13	-118.23	-88.79
χ^2	21.16	21.86	23.55	26.02	23.35	82.24
Prob $> \chi^2$	0.03	0.04	0.04	0.03	0.02	0.00

Dependent variable is a dichotomous variable coded 1 if land reform was passed in state i in year t. Logit with standard errors in parentheses. Two-tail tests presented despite directional hypotheses. Time-splines, lowess smoother, and fixed effects included but not reported where noted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Land reform remains robust

	(7)	(8)	(9)	(10)	(11)	(12)
Year Before Election	1.12**	1.10**	1.08**	1.06**	1.09**	2.14***
Monthly Weight	(0.48)	(0.48)	(0.48)	(0.48)	(0.48)	(0.82)
Election Year	0.53	0.60	0.62	0.61	0.63	2.35
Monthly Weight	(1.28)	(1.28)	(1.28)	(1.25)	(1.28)	(1.94)
Single-Party Dominant	0.96*	0.92*	0.83	0.28	0.79	0.80
	(0.50)	(0.50)	(0.51)	(0.42)	(0.51)	(0.65)
Multiparty: Left-Center-Right	0.96	1.06	1.10	1.79*	1.12	-0.07
	(1.04)	(1.07)	(1.07)	(0.92)	(1.06)	(1.32)
Two-Party: Left-Center	1.11	1.16	1.08	0.68	1.07	0.64
	(1.61)	(1.61)	(1.61)	(0.59)	(1.62)	(1.75)
Two-Party: Center-Right	0.80	0.74	0.78	-0.38	0.77	0.53
	(0.90)	(0.91)	(0.92)	(0.67)	(0.92)	(1.21)
% Owning No Land	0.08***	0.07**	0.07**	0.04**	0.07**	0.12**
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.06)
Early Elections	0.26	0.27	0.34	0.30	0.29	0.06
	(0.74)	(0.74)	(0.74)	(0.72)	(0.73)	(0.89)
President's Rule		-0.54	-0.56	-0.60	-0.57	-0.71
		(0.69)	(0.70)	(0.68)	(0.70)	(0.87)
Strictly Affiliated			0.51	0.57	0.54	0.34
			(0.43)	(0.40)	(0.43)	(0.59)
West Bengal				0.75		
				(0.67)		
Constant				-3.24***		
				(0.67)		
State FE	YES	YES	YES	–	YES	YES
Time Splines	YES	YES	YES	YES	–	–
Lowest Smoother	–	–	–	–	YES	–
Year FE	–	–	–	–	–	YES
N	515	515	515	515	515	515
States	15.00	15.00	15.00	15.00	15.00	15.00
Log Lik.	-119.21	-118.87	-118.15	-145.16	-118.68	-87.61
χ^2	21.40	22.07	23.51	25.69	22.46	84.61
Prob $> \chi^2$	0.03	0.04	0.04	0.03	0.02	0.00

Dependent variable is a dichotomous variable coded 1 if land reform was passed in state i in year t. Logit with standard errors in parentheses. Two-tail tests presented despite directional hypotheses. Time-splines, lowess smoother, and fixed effects included but not reported where noted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

from the additional popularity of the national stage, and therefore should be less likely to need to win over voters through land reforms. On the other hand, if we hold the view that the national party determines state policy, there may exist strong pressure for aligned states to implement land reform to benefit the party on the national stage. Although the strictly affiliated dummy lies in the positive direction, it is not statistically significant, and its inclusion has no substantive effect on the political timing variables (although single-party dominant is no longer significant).

As a further check, in Model 10 I add a dummy variable for *West Bengal* since the colonial history of zamandari estates in this region gave rise in the 20^{th} century to leftist and communist groups, who often championed land reforms. Its inclusion has no effect on the substantive impact of the findings. The last two models in Table B.2 probe the sensitivity of accounting for temporal duration. I continue using state fixed effects in Model 11 but substitute out the cubic splines for a single lowess smoother. The results remain unchanged. Finally, including both state and year fixed effects in Model 12 has no substantive impact on the results, although the coefficient on *Year Before Election* roughly doubles. Overall, the main findings are robust to all alternative specifications.

The summary statistics for the aggregate level results are shown in Table B.3. Data sources are from Besley and Burgess (2000,2002, 2004) and the EOPP Indian States Data Base, as well as Chhibber and Nooruddin (2004). Data sources not listed were coded by the author.

In Chapter 3, the dependent variable in the aggregate-level analysis was land reform, which was a combination of four categories: tenancy reform, the abolishing of intermediaries, ceilings on landholdings, and the consolidation of landholdings. In Table B.4 I parse out the dependent variable further. One dependent variable is created that equals 1 if a tenancy reform is passed in the state-year, and 0 otherwise. The other dependent variable is a combination of intermediary abolition, landholding ceilings, and consolidation—done so since these were the three least-common categories. The results from Table 1 in Chapter 3 are shown here in Table B.4, although I restrict the analysis to only using the monthly weighted coding of elections and use random effects.

As clear from Table B.4, even disaggregating land reform out further, substantive results remain identical to those in Chapter 3. Land reform is likely in the year before the election, and left-leaning

Table B.3: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N	Source
Effective Number of Parties	2.633	1.373	1.146	9.138	515	EOPP
% Owning No Land	13.483	8.175	0.96	41.58	515	EOPP
% Land Owned by Bottom 50%	5.648	4.235	0.2	20.68	515	EOPP
% Land Owned by Top 10%	49.366	7.029	29.3	66.92	515	EOPP
Dummy Variables (State-Years)	**Number of Occurrences**					
Land Reforms	48				515	EOPP
Elections Held	126				515	EOPP
Multiparty: Left-Center-Right	10				515	C&N (2004)
Two-Party: Left-Center	75				515	C&N (2004)
Two-Party: Center-Right	74				515	C&N (2004)
Leftist	33				515	
Congress	348				515	
Single-Party Dominant	146				515	C&N (2004)
Early Elections	48				515	
West Bengal	35				515	
President's Rule	60				515	C&N (2004)
Strictly Affiliated	348				515	

Data on land reform: Besley and Burgess (2000,2002,2004) EOPP Indian States Data Base. EOPP = EOPP Indian States Data Base. C&N (2004) = Chhibber and Nooruddin (2004). Data without sources listed are coded by the author.

governments are the most likely likely to carry out reforms. Interestingly, the Congress party seems to have favored reforms other than tenancy reform, as evidenced by the significance of the Congress dummy variable for the "Other" three types of land reform.

B.2 Details on individual-level analysis

The individual-level analysis in Chapter 3 used survey results from the Indian National Election Study (Eldersveld et al., 2011), and available at the ICPSR website (No. 25402). Interviews were fact-to-face and followed national elections in 1967, 1971, 1979, and 1985.

Only the 1967 and 1971 surveys contained questions about the most-important problem facing an individual's village, including a specific category for issues of land reform and issues of inequality. Issues of land included a variety of responses such as land tenure, consolidation of land, protecting the landless, the size of landholdings, and the desire to get more land. Issues of inequality could include responses such as an increase in economic disparities, or the gap between the rich and poor. Since these were open-ended questions, each type of response was grouped into

Table B.4: Parsing out the type of land reform

	Tenancy	Other	Tenancy	Other	Tenancy	Other	Tenancy	Other
	(1)		(2)		(3)		(4)	
Year Before Election	1.47***	1.81***	1.57***	1.92***	1.57***	1.93***	1.56***	1.81***
Monthly Weighted	(0.52)	(0.55)	(0.52)	(0.55)	(0.52)	(0.55)	(0.52)	(0.54)
Election Year	0.88	1.54	0.95	1.84	0.96	1.83	1.00	1.73
Monthly Weighted	(1.03)	(1.06)	(1.06)	(1.13)	(1.06)	(1.13)	(1.06)	(1.12)
Single-Party	0.13	0.43	-0.06	0.05	-0.06	0.03	-0.02	0.18
Dominant	(0.48)	(0.49)	(0.47)	(0.45)	(0.50)	(0.47)	(0.46)	(0.47)
Multiparty:	0.72	1.21						
Left-Center-Right	(1.13)	(1.15)						
Two-Party:	1.15**	0.97*						
Left-Center	(0.47)	(0.51)						
Two-Party:	-0.67	-1.05						
Center-Right	(0.79)	(1.08)						
Leftist			1.87***	2.63***	1.86**	2.68***	2.12***	2.89***
			(0.69)	(0.87)	(0.74)	(0.92)	(0.68)	(0.90)
Congress			0.68	1.58**	0.68	1.58**	0.71	1.64**
			(0.55)	(0.78)	(0.55)	(0.78)	(0.55)	(0.78)
% Owning No	0.01	0.04*	0.02	0.05**	0.02	0.05**		
Land	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)		
Effective No.					0.01	-0.03	0.06	0.05
of Parties					(0.15)	(0.15)	(0.15)	(0.17)
% Land Owned,							-0.10	-0.08
Bottom %							(0.07)	(0.07)
% Land Owned,							-0.00	-0.00
Top 10%							(0.04)	(0.04)
Constant	-2.62***	-3.26***	-3.26***	-4.55***	-3.29***	-4.47***	-2.47	-3.48
	(0.68)	(0.74)	(0.81)	(0.98)	(0.97)	(1.06)	(2.05)	(2.25)
N	515	515	515	515	515	515	515	515
States	15	15	15	15	15	15	15	15
Log Lik.	-116.80	-102.66	-116.60	-100.30	-116.60	-100.29	-115.57	-101.76
χ^2	22.11	26.12	19.89	28.81	19.87	28.80	23.80	26.73
Prob $> \chi^2$	0.00	0.00	0.35	0.00	0.31	0.00	0.00	0.00

Random-effects logit with standard errors in parentheses. Two-tail tests. Time-splines included but not reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

an overall category. As mentioned in Chapter 3, this was turned into a dichotomous indicator with 1 indicating the respondent thought inequality or issues of land reform were the most important issue facing their village, and 0 indicating some other issue was the most important.

Only the 1971 and 1985 surveys asked the following question, "Some political leaders and parties have been advocating that people with no land and property should occupy a part of land and property of those who have a large amount of land and property. Do you approve of this or do you disapprove?" Respondents could choose either "approve", "disapprove", or "uncertain". I recoded this into the *Approve of Land Grabs* indicator, where 1 means a respondent "approves of land grabs" and 0 means they either "disapprove" or are "uncertain".

Although most of the control variables are dichotomous, there were two multi-category variables used. Education is an 8-category indicator coded as follows. 1 is illiterate, 2 has some primary education, 3 has some middle-level education, 4 is some high school, 5 is a high-school graduate, 6 is some college, 7 is full college, and 8 is a post-graduate degree. The political interest variable asks about the respondent's interest in politics between campaigns. This is a trichotomous indicator with 0 indicating no interest, 1 indicating some political interest, and 2 indicating a lot of political interest.

APPENDIX C

C.1 Robustness section figures

In Table 4.4 I re-ran the models using the second round of voting in the presidential elections. Recall that in these second rounds, only the top two candidates face off against each other. I presented the results from Model 10 in Chapter 4. The marginal effects plots for the other models are presented below.

In Figure C.1 I plot the marginal effect of second round presidential vote across the level of surrounding municipal PT support (in the second round as well) on the left-side, and the marginal effect of the surrounding municipalities' support for the incumbent across the level of a municipality's vote for the PT. These come from Model 7 in Chapter 4. Similar to results in Chapter 4, I find that, if anything, incumbents seem to be targeting supporters surrounded by weak supporters rather than core regions of support. Overall, the results are not statistically significantly different from zero on the left-side plot in Figure C.1.

In Figure C.2 I plot similar marginal effects as in Figure C.1, although the interaction is further conditioned by whether the municipality in question is held by a mayor belonging to the PT or a party in the national coalition. The results come from Model 8 in Chapter 4. As with the results in the chapter, there does not appear to be any conditioning effect based on local-level partisanship. Nor does there appear to be much of a conditioning effect of surrounding municipalities on the level of transfers a given municipality receives.

In Figure C.3 I plot the marginal effect of PT vote in the second round of elections across the level of surrounding PT vote in the left-side plot, and the marginal effect of surrounding PT presidential vote across a municipality's PT vote on the right-side plot. These results come from Model 9 in Chapter 4. The results are very similar to Figure 4.4 in Chapter 4; incumbents move from targeting islands of support in off-election years to a very broad-based strategy in municipal election years, one that does not appear to be conditioned by surrounding municipalities.

Figure C.1: Some support for targeting islands over strongholds: Second round vote results

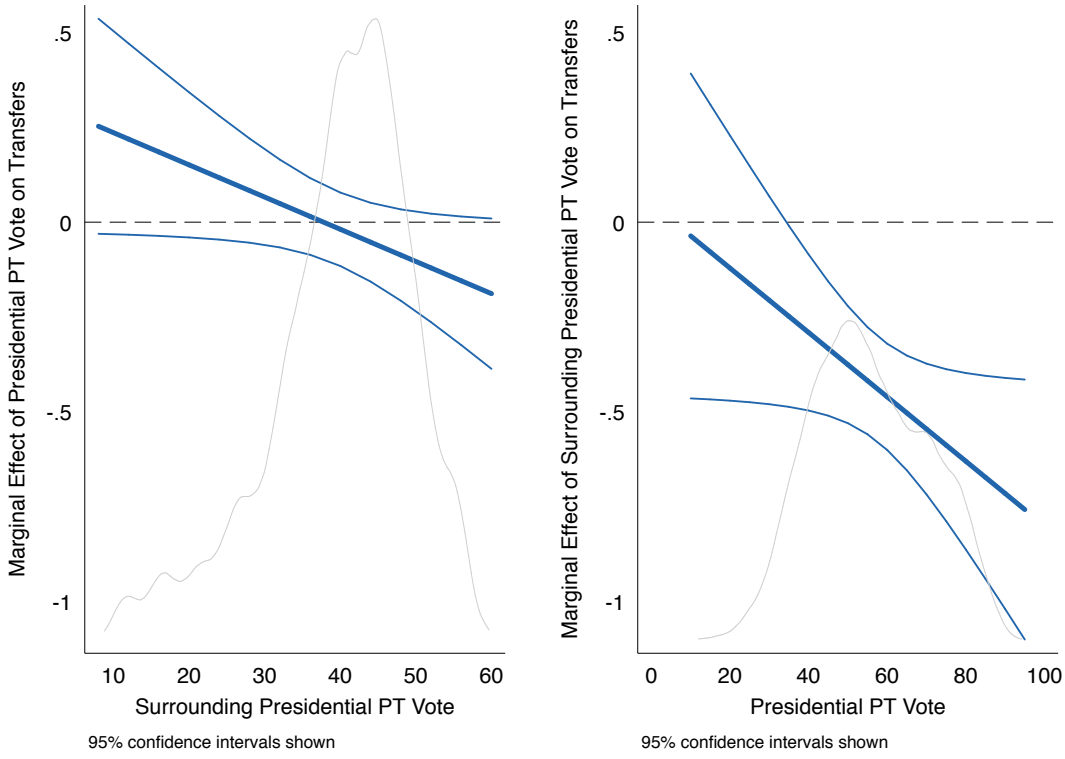


Figure C.2: Local-level partisanship does not affect spatial opportunism: Second round vote results

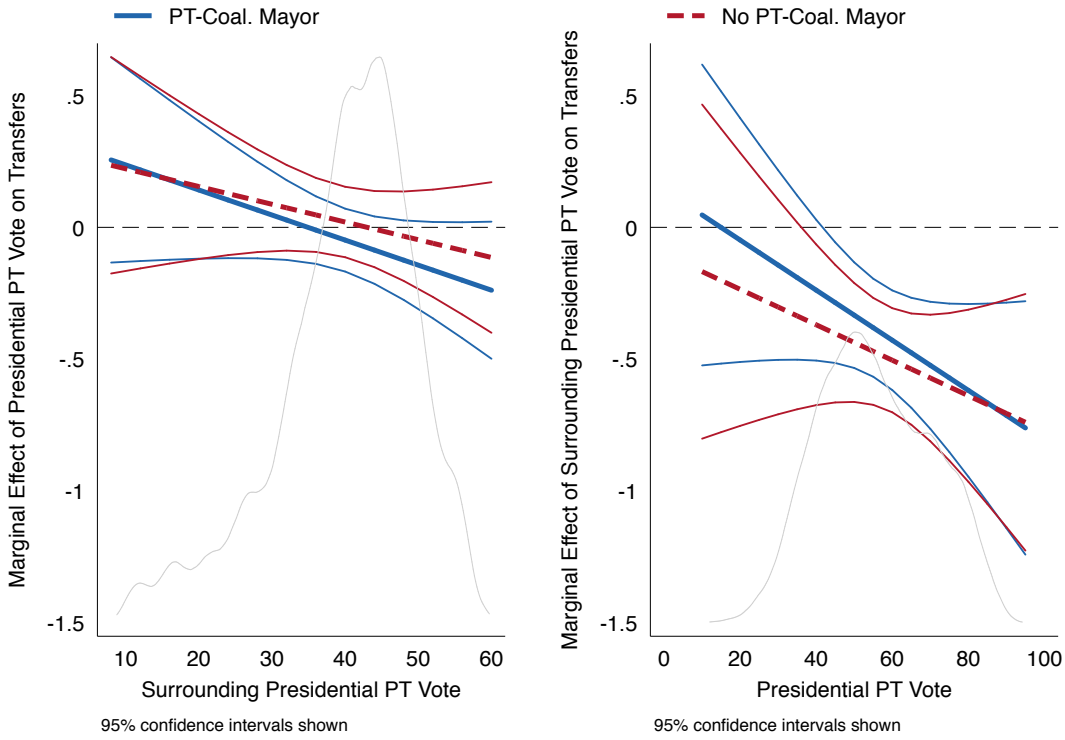


Figure C.3: Shift from favoring islands to broad-based support in municipal election years: Second round vote results