Patronage, elections, and constraints: Theory and evidence of political bureaucratic cycles in Brazil

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May 7, 2018

Abstract

Over the past four decades, a vast literature has explored both theoretically and empirically the existence of political cycles in economic outcomes and economic policy tools (political business and political budget cycles, respectively). I advance a related concept, political bureaucratic cycles (namely, cycles in the hiring and firing of bureaucrats and in bureaucratic outputs), and propose a theory at the intersection of the literatures on clientelism and on political business/budget cycles. Empirically, I leverage administrative, micro-level data on the universe of municipal employees in Brazil for the period 2002-2016, which I analyze using quasi-experimental methods. My findings show that, despite (or possibly because of) the legal limits on public hiring around election time, hires and dismissals of municipal personnel show markedly cyclical patterns in the months before and after elections. I then leverage administrative data on the outputs of the healthcare bureaucracy and show that hiring cycles hurt bureaucratic outputs: the number pre-natal check-ups are systematically lower in the months around elections. Finally, I exploit close elections to identify the causal effect of an electoral defeat for the incumbent on political bureaucratic cycles. If the mayor loses, there is a large increase in dismissals under the outgoing administration, a compensating increase in hires under the incoming administration, and a decrease of pre-natal checkups in the months around government turnover. I complement these quasi-experimental findings with insights from in-depth interviews of municipal politicians to better understand the dynamics of political bureaucratic cycles, and how they relate to the political and legal constraints faced by politicians.

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1 Introduction

For decades now, political scientists and economists have studied how politicians' manipulation of economic policy around elections leads to cycles in economic outcomes and/or economic policy. The basic idea is that, in order to increase their chances of re-election, politicians change their economic policy right before the election, in what are often seen as economically suboptimal policy choices that un-smooth government spending and economic activity. This results in what are called political business cycles (related to economic output) or political budget cycles (related to government policy tools like spending).[1] Studying these cycles is useful not only because they have the potential to shape economic policy and outcomes, but also because they provide a window through which to study broader questions of accountability, democracy, and representation.

In this paper, I advance a related concept, namely political bureaucratic cycles (which share the same acronym as political business/budget cycles, PBCs), focused on politically-produced cycles in both bureaucratic inputs (public sector jobs) and bureaucratic outputs (public employees' production). Jobs are one of the most important spending categories for governments around the world, and are indeed one of the main distributive tools used by governments in developing countries. In fact, to try and constrain the use of public jobs as a clientelistic tool in general and especially around elections, several countries (among them Brazil) have imposed legal constraints on governments' hiring decisions.

In this paper, I propose a novel theory of political bureaucratic cycles, with emphasis on three dimensions. First, I retrieve an idea found in early formal models of PBCs but rarely addressed empirically if at all: that fiscal rules designed to contain PBCs may simply displace or even exacerbate them. From that basic idea and from the institutional design of the country I study – Brazil –, I deduce a number of hypotheses about the cyclical behavior of hires and dismissals in electoral years. Second, I leverage the theoretical apparatus of the clientelism literature – which has surprisingly rarely been used in research on PBCs – to propose a theory of patronage (or the clientelistic allocation of public jobs) around elections, with its corresponding testable hypotheses. Third, I build on recent work about the link between political and bureaucratic turnover, and put forward an expanded concept of political turnover that understands it as a phenomenon starting from the moment an incumbent loses the election, rather than when the winner takes office. Doing

[1] The literature and this project focus on cycles caused by politicians' actions around elections. Note there are also electoral cycles that are not driven by politicians' actions (Block and Vaaler, 2004), and political cycles that are not driven by elections (Guo, 2009).

so enables, I argue, a better understanding of the dynamics of political and bureaucratic turnover.

Empirically, I study political bureaucratic cycles in Brazilian municipalities, a context where the institutional design and the availability of rich data makes it possible to conduct rigorous empirical tests. To do so, I exploit administrative, micro-level data on the universe of municipal employees between 2002 and 2016, as well as administrative data on bureaucratic outputs in the healthcare sector. My empirical strategy has at its core time-series cross-section regressions of over one million municipality-month observations, which together with the strict exogeneity of municipal elections help me identify the dynamics of political bureaucratic cycles. I then exploit a close-races regression discontinuity design to identify the causal effect of political turnover on political bureaucratic cycles in the six-month period after an incumbent experiences a defeat at the polls. I complement these quantitative strategies with in-depth interviews with municipal politicians and bureaucrats across 4 states in Brazil, which help me develop hypotheses and probe mechanisms about how politicians' incentives and constraints interact to generate political bureaucratic cycles.

The rest of the paper is organized as follows. Section 2 reviews the literature on PBCs, highlighting its evolution over time, achievements, and limitations. Section 3 describes Brazil's institutional context and explains why it is a particularly useful setting in which to study political bureaucratic cycles. Section 4 outlines a theory of political bureaucratic cycles and how it applies to Brazil, and lists a number of testable hypotheses. Section 5 presents an empirical strategy to test those hypotheses, including research design and data analysis plans. Section 6 describes the data, its sources, and its limitations. Section 7 presents my quantitative results, and finally Section 8 concludes by discussing the findings' implications and by outlining the next analytical steps.

2 Literature review

In this section I briefly review the large literature on political business / budget cycles, both formal and empirical, highlighting the most important trends, contributions, and limitations.

2.1 The foundations: Formal models of PBCs

Among the early models of PBCs, perhaps the most influential contribution was that of Nordhaus (1975). His model basically posits that democratically-elected governments will aim at increasing their chances of re-election by manipulating the Phillips curve, with higher unemployment and lower inflation at the beginning of the mandate and a reverse pattern at the end. The result, then, would be political business cycles and deviations from policies along the Phillips curve that are optimal on the long run. In the model, voters are rational in their retrospective assessment of the government, but myopic and ignorant of the macroeconomic trade-off, and thus "fooled" by the government time after time.

Later models questioned this assumption of irrational voters who are not able to anticipate the government's behavior as elections approached, and modeled PBCs as fully rational equilibria. Rogoff and Sibert (1988) modeled electoral cycles in spending, taxes, and the money supply as a signaling equilibrium driven by temporary information asymmetries. In their model, voters observe government competence only with a lag, and thus incumbents of medium level of competency have an incentive to signal the effectiveness of their policies by setting a policy at a suboptimal level. In their model, PBCs occur even with fully rational voters who recognize incumbents' incentive to temporarily distort the economy before elections. Similarly, Rogoff (1990) argues that temporary information asymmetries push incumbents to switch fiscal policies before the election towards easily observed consumption expenditures (and away from investment).

Starting in the late 1980s, a number of models started considering how political and economic factors influence or constrain PBCs. A large number of contextual features have been considered here. Because their treatment has been more empirical than formal, I synthesize this literature on conditional PBCs below. A separate stream of work looked into partisan (as opposed to opportunistic) PBCs driven by differences in government parties' preferences over unemployment and inflation, moving away from the assumption of homogeneous government preferences (Hibbs, 1977; Alesina, 1987).

To sum up, formal models of PBCs generally moved in three directions in the 1970s and 1980s. First, the literature moved from examining cycles in economic outcomes (output, employment, inflation) to examining cycles in economic policy tools (spending, taxes, deficit), thus moving from political business cycles to political budget cycles. Second, scholars went from assuming myopic voters who were fooled by politicians to assuming rational voters and politicians in a situation of

information asymmetry, with PBCs facilitating signaling at a cost in economic distortion (which would be observed by voters, although with a lag). Finally, the literature moved from discussing the rationale for PBCs to examining the institutional and political contexts in which they are likely to arise.

2.2 The empirical approach: Panel-data studies of PBCs

Since the 1970s a vast number of empirical studies examining the existence and drivers of PBCs have been published . These studies typically use time-series cross-section regressions with election-year periods (sometimes together with dummies for pre- and post-electoral periods) to measure the effect of electoral periods on a wide range of economic variables. A first generation of studies examined cycles in unemployment and inflation, in line with the model put forward by Nordhaus (1975), and found mixed results (McCallum, 1978; Beck, 1987; Keil, 1988; Haynes and Stone, 1989).

A second generation of dozens (if not hundreds) of papers has examined PBCs in economic policy instruments, generally finding evidence of cycles although with significant heterogeneity. In the words of a recent review on the subject, "empirical tests of the PBC in instruments are much more convincing so that nowadays, tests in relation to outcomes are scarce" (Dubois, 2016, 242). The most common policy tools examined by these studies are government spending and revenue variables, such as overall level of spending (Besley and Case, 1995; Block et al., 2003; Khemani, 2004; Alt and Rose, 2009), level of spending by category (Akhmedov and Zhuravskaya, 2004; Veiga and Veiga, 2007; Saez and Sinha, 2010; Pierskalla and Sacks, 2018), share of government spending by category (Khemani, 2004; Drazen and Eslava, 2010; Brender and Drazen, 2013), tax rates (Besley and Case, 1995; Alesina and Paradisi, 2017), government deficit (Brender and Drazen, 2005; Shi and Svensson, 2006; Veiga and Veiga, 2007; Veiga et al., 2017), government revenue (Brender and Drazen, 2005; Alt and Lassen, 2006; Veiga and Veiga, 2007; Covre and de Mattos, 2016), and government debt (Khemani, 2004).[2] A third generation of studies has started to look into economic outputs such as road construction (Khemani, 2004), electricity provision (Baskaran et al., 2015) and even non-investment GDP (Canes-Wrone and Park, 2012).

Public employment has also received some empirical attention, but results are pretty mixed.

[2]Some studies have looked at cycles in other policy tools such as the money supply and exchange rates (Block et al., 2003).

Drazen and Eslava (2010) find that Colombian municipalities contract payments to temporary workers in election years. Similarly, Tavares (2017) finds that non-tenure public employment and personnel expenditures fall in electoral years in Brazilian municipalities. On the other hand, Dahlberg and Mörk (2011) show that Finish and Swedish municipalities increase employment in electoral years. Labonne (2016) finds that both private and public sector employment increase in pre-electoral quarters in the Philippines. Pierskalla and Sacks (2017) find that Indonesian municipalities inflate the teaching force in election years.

A significant part of the recent literature is concerned with identifying the institutional, political, and economic contextual features that mediate PBCs.[3] This has generally been done either by splitting the sample by a variable of interest, or by interacting the election-period dummies with indicators for such contextual features. The proliferation of papers using one of these empirical strategies may be understood as a way of exploiting PBCs to address issues of representation and accountability. As expressed by a recent review of this literature, "context-conditional political budget cycles provide a lens through which to study the extent to which voters are able to select, monitor, sanction, and control politicians – and the extent to which politicians serve their own interests at the expense of voters' interests – in different political and institutional environments" (Alt and Rose, 2009).

Some of the contextual variables that have received theoretical and/or empirical attention in the conditional PBCs literature are electoral competitiveness (Schultz, 1995; Block et al., 2003), level of development (Shi and Svensson, 2006), level of democracy (Gonzalez, 2002), parliamentary versus presidential system (Persson and Tabellini, 2004), age of democracy and voters' experience with elections (Brender and Drazen, 2005), media freedom (Veiga et al., 2017), transparency (Alt and Lassen, 2006; Vicente et al., 2013), partisanship (Kneebone and McKenzie, 2001; Sakurai and Menezes-Filho, 2011), partisan polarization (Alt and Lassen, 2006; Canes-Wrone and Park, 2012), party characteristics (Hanusch and Keefer, 2014; Pierskalla and Sacks, 2017), the electoral system (Persson and Tabellini, 2004), term limits (Klein and Sakurai, 2015), fiscal rules (Rose, 2006; Alt and Rose, 2009; Cioffi et al., 2012),[4] alignment with other levels of government (Chortareas et al., 2017; Sakurai and Menezes-Filho, 2011), the flexibility of different policies (Beck, 1987), and the returns to manipulating a given policy tool (Schultz, 1995; Treisman and Gimpelson, 2001).

Econometrically, this literature uses time-series cross-section datasets of either countries or

[3] For a review of this literature on conditional PBCs see De Haan and Klomp (2013).

[4] The fiscal rules that have received most attention are balanced budget requirements, budget targets, and specific rules on spending around elections.

subnational units, typically with country-year observations. The last decade has seen a marked move from national to subnational units, which allows researchers to keep many institutional features fixed. It has become increasingly common to use quarterly or monthly data, since – as noted by Labonne (2016)– using yearly data makes it harder (and may even obstruct) cyclical patterns.[5] With regards to causal identification, researchers typically assume that the timing of elections is exogenous. This assumption is reasonable in cases where the electoral schedule is enshrined in the constitution, but remains problematic in cases (such as parliamentary democracies) where governments decide on the timing of elections. There is a whole literature on the endogenous timing of elections and PBCs,[6] but since the work of Khemani (2004) it has become commonplace to instrument for election timing with the originally scheduled calendar. A couple of recent studies however take issue with the assumption of exogeneity even in the case of constitutionally scheduled elections, and have exploited the phase-in of elections (Pierskalla and Sacks, 2017, 2018) or special elections held after the death of an incumbent (Baskaran et al., 2015). In general, the identification of causal effects from the contextual variables has not been dealt with, and researchers often uncritically split their sample by, or interact the election-period dummies with, theoretically relevant variables. As for the estimation strategy, most studies include in their regressions lagged dependent variables, unit fixed effects, time-period fixed effects, and some control variables. To avoid Nickell bias (Nickell, 1981) resulting from the inclusion of fixed effects and lagged dependent variables, many papers use a GMM estimator (Arellano and Bond, 1991). Standard errors are usually corrected for serial and spatial correlation with one- or two-way clustering.[7]

To sum up, the last few decades have seen an explosion of empirical studies of PBCs, using national and subnational time-series cross-section data from around the world. This has resulted in an impressive accumulation of evidence about the existence of PBCs around the world, and the conditions under which they are more likely to emerge. All in all, the literature suggests that politicians manipulate economic policy variables in advance of elections, at least under certain conditions. The Brazilian context, to which I turn now, has a number of institutional and data features that make it an unparalleled environment in which to study political bureaucratic cycles, how they relate to the fiscal constraints faced by politicians, and what consequences they have for citizen welfare.

[5] I take this logic further by using contract-level data, aggregated at the month level.

[6] I do not review these debates here because the timing of elections is not endogenous in Brazil.

[7] Some papers have also tried to explicitly model the spatial dynamics between municipal governments in Brazil, be them driven by either spillovers or yardstick competition (Videira and Mattos, 2011; Covre and de Mattos, 2016).

3 Institutional setting

Brazil returned to democracy in the 1980s after a two-decade military dictatorship. The 1988 Constitution established a three-level federal system and gave significant fiscal autonomy to municipal governments. Among other things, municipalities are responsible for providing primary education and healthcare, two areas in which they are obliged to spend at least 40% of their revenue. Municipal governments, however, depend heavily on inter-governmental transfers and raise only a small fraction of the revenue they spend (Arretche, 2004). The country has 27 federated units (26 states plus a federal district) and a large number of municipalities which has grown from 4,491 in 1991 to 5,570 today.[8] As per the constitution, municipal, state and federal elections are held every four years on the first Sunday of October, with municipal elections taking place two years after state and federal elections.[9] Importantly, this means that the timing of municipal elections is strictly exogenous. Until the 1997 constitutional amendments, mayors were ineligible for re-election, but since then they are eligible for a second term. The electoral system for mayoral elections is majoritarian, with a runoff election if no candidate obtains an absolute majority of the valid votes only in municipalities with over 200,000 inhabitants.[10]

In a context of increasing pressures for monetary and fiscal discipline, and in response to problems of fiscal asymmetry between the three levels of the federation, Congress approved in 2000 a highly consequential Fiscal Responsibility Law (LRF, *Lei de Responsabilidade Fiscal*). The LRF aimed at clarifying responsibilities between all levels and branches of government, and at increasing accountability and reducing moral hazard between them (Loureiro and Abrucio, 2004). The most important aspects of the law with regards to political cycles are the limitation of government revenue that can be spent on personnel; the establishment of stricter fiscal limits for electoral years; and the establishment of targets and reporting mechanisms.[11]

Specifically, the LRF includes seven main rules designed for controlling personnel expenses and

[8] The splitting of municipalities, which was particularly intense during the 1980s and 1990s, has been partially driven by electoral and political factor. For an empirical study of the matter see Tomio (2005).

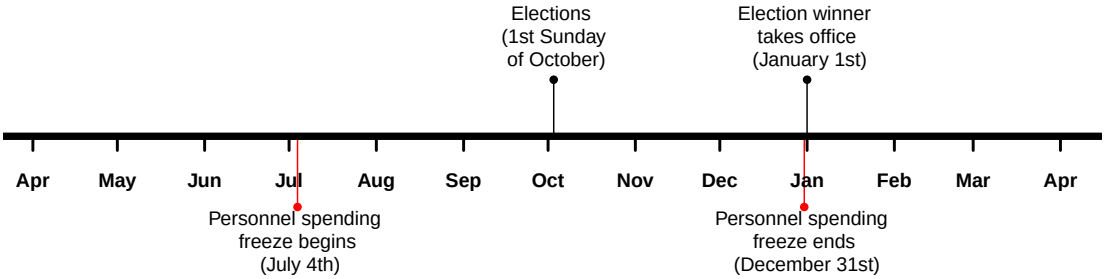
[9] The constitution of 1988 established that elections had to take place in the last 90 days of a mayor's mandate, but a 1997 constitutional amendment imposed the rule of first Sunday of October. In practice, the 1996 and 1992 elections were also held in early October. The first municipal elections under the new constitution were held on November 15, 1988.

[10] Only 92 municipalities (less than 2%) reached that population threshold in the 2016 municipal elections. When runoff elections do take place, they are held on the last Sunday of October, as per the 1997 constitutional amendment.

[11] While the LRF became an inflection point in the fiscal control of state and municipal governments, some of its rules, especially those concerning the control of personnel expenses were already enshrined in federal legislation (Kerches and Peres, 2010).

their use as patronage in electoral years. First, no municipal government can spend more than 60% of the net liquid revenue in personnel expenses, with 6 points being reserved for the legislative and 54 for the executive (article 20). Second, personnel expenses cannot increase during the 180 days before the end of the government's mandate (article 21), i.e., roughly three months before and after municipal elections (see Figure 1). Third, compliance with this limit is verified at the end of every quadrimestre. If personnel expenses are over 95% of the limit (i.e., over 51.3% for the executive), the municipality cannot create new posts or give out salary increase (article 22). Fourth, if the limits are surpassed, the government must comply in the next two quadrimestres, with at least one third of the reduction in the first quadrimestre. However if the limits are surpassed during an electoral year, the government cannot receive so-called voluntary transfers,[12] or get credit or guarantees (article 23). Fifth, up to 30 days after the end of every quadrimestre the government must issue a Fiscal Management Report (RGF, *Relatório de Gestão Fiscal*), which must be open to the public and contain a comparison of actual personnel expenses and the legal limits (articles 54 and 55). Sixth, if personnel expenses reach 90% of the limit (i.e., 48.6% for executive governments), audit courts will alert the legislature and the prosecutor's office (article 59). Finally, municipalities with less than 50,000 inhabitants can issue their RGFs every semester instead of every quadrimestre, and were only obliged to issue some of the other fiscal reports starting 2005 (article 63).

Figure 1: Timeline of election cycles in Brazil



Not only did the LRF establish clear rules for municipal governments, it also established clear procedures for its enforcement through prosecutors, courts, and audit courts. A few other pieces of legislation[13] establish tough penalties on mayors for non-compliance. For example, mayors who

[12]Voluntary transfers are transfers from other levels of government that are not related to the healthcare system or mandated by the constitution.

[13]Including a decree from 1967, the penal code, the 1992 administrative dishonesty law, and the 2002 fiscal crimes law.

increase salary expenses in the last 180 days of their mandate are subject to penalties of 1-4 years of prison, losing their post, and being disqualified for election for 5 years. In general, the LRF can be seen as a counter-movement to the wave of legal and fiscal decentralization that took place in the aftermath of the 1988 constitution, and which led to a dramatic increase in municipal governments' budgets and policy responsibilities. If on the one hand municipalities are seen as the locus for participation and democratic control, on the other hand they have traditionally been seen as the stronghold of clientelistic politics and irresponsible fiscal behavior, especially in the smaller municipalities of the interior (Leal, 1948; Colonnelli et al., 2018).

Ultimately, it is an empirical question whether the LRF managed to constrain the fiscal behavior of municipal governments. Some studies have found that the LRF made municipalities more disciplined in their revenue-spending and balance (Linhares et al., 2012), and that it reduced municipalities' spending on personnel or at least contained its growth (Santolin et al., 2009; de Menezes and Júnior, 2009). On the other hand, the LRF may have fostered suboptimal strategic fiscal behaviors that may be inefficient and/or that may obstruct accountability. For example, Kerches and Peres (2010) note that some municipal governments have chosen a strategy of contracting out social services to firms, in order to reduce personnel expenses by counting those contracts as mere service expenses. They note that while the Treasury has requested that those contracts be included under personnel expenses whenever those contracts aim at substituting public employment, in practice this has not been enforced. In fact, data on personnel expenditures for 2015 show that compliance with the LRF is far from perfect, with over 25% of the municipalities that submit data to the Treasury reporting spending over 60% of their net revenue on personnel. A related, interesting question is how politics influences the enforcement of the LRF rules. Experts report there is significant heterogeneity in the enforcement done by state audit courts and state prosecutors (Kerches and Peres, 2010), and Hidalgo et al. (2016) have shown that the enforcement of accounting rules by state audit courts is politically biased, with auditors showing a bias in favor of those politicians who appointed them.

4 Theory and hypotheses

In this section I propose a theory of political bureaucratic cycles, which I see as resulting of the strategic calculations of politicians who are election-motivated, who operate in a context of limited economic development in which public jobs are a critical distributive resource, and who have their

choices constrained (significantly but not completely) by fiscal and legal limits. In low- and middle-income contexts, the returns to the political use of public employment may be higher than in high-income environments for two main reasons. First, with less job opportunities in the private labor market, the value of public jobs increases. Second, with less state capacity distributing other benefits that may be valued by citizens (such as infrastructure works) require more planning, more implementation capacity, and more coordination with higher levels of government and with the private sector. In these settings, therefore, jobs are more likely to become a political currency.[14] The theory and hypotheses are based on my reading of the literature and my ongoing interviews with municipal politicians and bureaucrats across Brazil.

My theory is innovative in three respects. First, I hypothesize (following an early contribution of the formal literature) that legal limits aimed at constraining PBCs may in fact simply displace them or even exacerbate them. Second, I bring in insights and concepts from the literature on clientelism, which has surprisingly rarely been linked to research on PBCs. Third and last, I propose an expanded theory of the links between political and bureaucratic turnover, building on the one put forward by Akhtari et al. (2018). I develop each of these points below, while spelling out testable hypotheses for the empirical analysis.

4.1 Legal constraints on politicians' ability to manipulate policy ahead of elections displace, rather than eliminate, PBCs

Most if not all of the literature on political business/budget cycles assumes that they are negative, and that fiscal constraints imposed to contain them are welfare enhancing. However, I hypothesize that these limits may simply displace or even exacerbate PBCs, at least in contexts where politicians have both the incentive and the ability to manipulate policy in the months around elections. Interestingly, this hypothesis was already present in one of the most cited papers in the literature about PBCs. Rogoff (1990) notes that PBCs may be a socially efficient mechanism for diffusing information about the government's competence. In his own words, "efforts to curtail the cycle can easily reduce welfare, either by impeding the transmission of information or by inducing politicians to select more socially costly ways of signaling" (Rogoff, 1990, 22).[15] Rogoff goes on to note (and argue formally) that "in practice, an incumbent has a wide array of fiscal actions with which he can

[14] Note however that my theory does not rule out political bureaucratic cycles in high-income countries, and there is indeed anecdotal evidence of the political use of public employment in OECD countries.

[15] A similar idea was put forward by Tufte (1978, 149).

signal, and it is not realistically possible to constrain him in all dimensions. If this is the case, then attempts to block signaling in one set of fiscal policy instruments will tend to exacerbate distortion in others. Indeed, attempts to suppress the political budget cycle may actually reduce the welfare of the representative citizen by inducing competent types to signal inefficiently" (Rogoff, 1990, 31). While I do not directly test the effects of the LRF on political bureaucratic cycles, I do test a number of hypotheses about them that do stem from this idea that rules to control PBCs can have unintended consequences.[16]

The first hypothesis is that, despite the strong constraints imposed by the LRF, there are political bureaucratic cycles in Brazilian municipalities, and that these cycles are in fact shaped by such limits. In particular, I expect to find that hiring expands in the second quarter of an electoral year (*hypothesis 1a*); stabilizes in the third quarter, after the beginning of the LRF's hiring freeze in early July (*hypothesis 1b*); contracts in the last quarter, after the elections are held (*hypothesis 1c*); and expands again in the first quarter of the following year (*hypothesis 1d*). I expect these political cycles in bureaucratic inputs to have a correlate in bureaucratic outputs (*hypothesis 1e*), with the direction of the association being related to whether politicians' decisions follow a clientelistic or a programmatic rationale, as explained below.

Second, I expect the political bureaucratic cycles to be most pronounced where politicians have both the ability and the incentives to manipulate hiring. In particular, I hypothesize that cycles will be most pronounced for temporary than for tenured contracts (*hypothesis 2a*), and in situations where the incumbent mayor is re-eligible for re-election (*hypothesis 2b*) and exposed to higher electoral competition (*hypothesis 2c*).[17]

4.2 Political bureaucratic cycles through the clientelistic lens

A second area of focus in my theory of political bureaucratic cycles is the connection to clientelism. Surprisingly, the empirical and theoretical apparatus of the clientelism literature has rarely been

[16] I am working on processing data on municipal personnel expenses, which will allow me to study the consequences of some of the stricter rules imposed by the LRF for municipalities above certain thresholds. I am also working on processing the municipal employment data for the 1990's, which will potentially allow me to assess the effect of the LRF through a difference-in-differences design.

[17] In future versions of the paper I will test the hypotheses that PBCs are more pronounced when municipal finances are not close to or above the limits on personnel expenses imposed by the LRF (*hypothesis 2d*), and where municipal employment represents a larger share of total formal employment (*hypothesis 2e*). I expect these two variables to increase politicians' maneuver for engaging in cyclical policies and the returns to doing so, respectively.

leveraged on studies of PBCs.[18] Scholars of clientelism have long studied the critical role that jobs pay in clientelistic equilibria (Wilson, 1961; Chubb, 1982; Golden, 2003). In fact, jobs may play a unique role in clientelistic arrangements since they are "a credible, selective, and reversible method of redistribution, which ties the continuation utility of a voter to the political success of a particular politician", and hence solve a two-way commitment problem between the patron and the citizen (Robinson and Verdier, 2013). In interviews, it is common for actors in the field to spontaneously express views of municipal jobs as a clientelistic resource. The adviser to a judge in a municipality of Ceará told me that "at the time of elections, these small municipalities of the interior turn into two sides, A and B, because of people's need for municipal contracting of services and hiring of personnel." A secretary of education in another municipality of the state said "in municipalities in this area, politics is very influential [on hiring], since [the municipal] government is the largest employer."

I build on efforts by Pierskalla and Sacks (2017) at bridging the gap between theories on PBCs and on clientelism, a cross-fertilization that I argue is useful in four fronts. First, examining political bureaucratic cycles can help us understand the reasons for the persistence of patronage systems (Grindle, 2004). Second, the conceptual and theoretical tools of the clientelism literature can be mobilized for examining who benefits from PBCs, an issue that has rarely been addressed in the literature, the only exception I found being Khemani (2004). In particular, it is possible and interesting to examine the extent to which the expansion of hiring during political bureaucratic cycles is targeted at mobilizing core supporters or at activating those who would not otherwise support the government, an issue that has received both theoretical and empirical attention in the clientelism literature (Nichter, 2008; Stokes et al., 2013). Connecting the literatures on PBCs and clientelism can also lead to a new interpretation of "partisan budget cycles" (Hibbs, 1977; Alesina, 1987). We can examine not only if parties of a certain ideology are more likely to engage in political bureaucratic cycles, but also whether PBCs are more common under programmatic versus clientelistic parties (Cruz and Keefer, 2015). One may expect that parties of the left, because of their weaker commitment to fiscal discipline, are more likely to engage in PBCs (*hypothesis 4a*), and that clientelistic parties, because of their dependence on patronage will lead to strong PBCs (*hypothesis 4b*). Finally, if political bureaucratic cycles are indeed a clientelistic strategy, we can also examine whether their use decreases with drops in poverty combined with political competition, as proposed by Weitz-Shapiro (2012) (*hypothesis 4c*).[19] In examining these hypotheses, PBCs can

[18] To my knowledge the only studies of PBCs that establish a direct dialogue with the clientelism literature are Hanusch and Keefer (2014) and Pierskalla and Sacks (2017).

[19] These are three large areas of macro-level work on the equilibria that may sustain political bureaucratic cycles,

be leveraged to contribute to the literature on the sources of stability in clientelistic equilibria and on the reasons for their decay.

A long literature has suggested that municipal employment in Brazil is targeted at core supporters (or even party members) and thus can be categorized as patronage (Leal, 1948; Colonnelli et al., 2018). I argue, however, that we need more refined theories to understand the political rationale in the distribution of public jobs in Brazilian municipalities. A good starting point is the conceptualization of distributive politics put forward by Stokes et al. (2013, 7). The authors differentiate non-programmatic politics targeted at individuals by whether benefits are contingent on individuals' political support (if not, they call it nonconditional benefits) and, if they are, by whether benefits are targeted at party members (which they call patronage) or at voters at large (which they call vote buying or turnout buying).

I argue that jobs are however a unique type of benefit. If on one hand jobs constitute a very targetable and selective benefit, which therefore serves well the needs of a clientelistic exchange, on the other hand they have (at least potentially) a public dimension to it depending on how the government employs its labor force. That is to say, a public job can serve a clientelistic goal depending on how it is distributed, but it can also (and even concurrently) serve a programmatic goal if the job is used for producing public goods and services for the whole population.

Put another way, if a politician decides to expand the bureaucracy ahead of elections, the extent to which we call that phenomenon clientelistic politics should depend, I would argue, on who gets the job (or, more precisely, what criteria are used for distributing the job) as well as on what that new employee is asked to do. These two factors would define a continuum of political strategies, but can also help us think of ideal types. On one extreme, politicians would hire their core supporters (or swing voters) merely as a way of transferring income from the public budget to their supporters' pockets and thus ensuring their vote, asking of those new employees nothing but that they work for their re-election. This would be a purely clientelistic (or particularistic) strategy. On the other extreme, politicians simply hire the best available workers in order to boost the production of public goods ahead of the election. Here there may be some rent seeking in that politicians would be un-smoothing public consumption and bureaucratic activity in order to reap an electoral benefit, but the cycle itself would be programmatic (or universalistic) both in terms of who gets the job and in terms of the purpose it serves. Normatively, we want politicians to be responsive to voters, and in a world of voter myopia the un-smoothing of public spending and

but here I only present some preliminary results about partisanship.

bureaucratic activity ahead of elections may be a fair price to pay for democratic accountability. In reality of course politicians' strategies are likely to combine a mix of clientelistic and programmatic elements, but theoretically separating these two dimensions of job assignment and job activity is useful for organizing hypotheses.

If PBCs are aimed mostly at mobilizing supporters, we should observe that those hired before the elections have lower levels of education (*hypothesis 5a*), have less formal labor market experience (*hypothesis 5b*), and are more likely to be members of political parties (*hypothesis 5c*) than the average municipal employee. If, on the other hand, the expansion of the bureaucracy before elections is more programmatic and aims at signaling capacity to the electorate at large, we should observe new hires have "standard" levels of education and experience, and are not more likely to be party members. Moreover, if the inflation of the bureaucracy ahead of elections has a programmatic character in the goal of the jobs, we should observe bureaucratic outputs and outcomes improve in those months (*hypothesis 5d*).[20] If on the other hand bureaucratic outputs and outcomes worsen ahead of the election (*hypothesis 5e*), it is likely to stem from PBCs being clientelistic.

4.3 An expanded theory of the relationship between political and bureaucratic turnover

Akhtari et al. (2018) argue that, when the bureaucracy is not isolated from politics, political turnover disrupts the bureaucracy by inducing bureaucratic turnover, thereby hurting development outcomes. The core of their empirical analyses is a regression discontinuity design of close races in Brazilian municipalities, through which they examine the causal impact of party turnover on bureaucratic turnover and on school quality. Essentially, they argue that incoming administrations reshuffle teachers and appoint lower-quality headmasters, which hurts student learning.

I argue that to better understand the connections between political turnover, bureaucratic turnover, and bureaucratic outcomes, we need to examine not only the period after a new administration takes office, but at the whole period starting on the day after the election. I therefore propose an expanded theory of the link between political and bureaucratic turnover, which complements that of Akhtari et al. (2018). I start by conceptualizing political turnover as a phenomenon

[20] In the future, I will also examine whether PBCs are more common under clientelistic incumbents, which may be defined as a politician who has switched party labels in the past; and whether PBCs are weaker when a municipality has both high electoral competitiveness and low levels of poverty, as hypothesized for clientelism in general by Weitz-Shapiro (2012).

that starts not in January (when the new administration takes office) but in October (when the sitting incumbent loses the election). My interviews with bureaucrats and politicians consistently suggest that hiring decisions are systematically different in the last three months of the election year when the incumbent loses the election: losing incumbents tend to contract the bureaucracy, which would have significant impacts over bureaucratic outputs and outcomes. This is arguably due to the LRF limits and the fear that, once out of office, they will be prosecuted for imperfect compliance with fiscal discipline rules. The logic was illustrated well by a municipal secretary of healthcare in the state of Ceará, when I asked them about whether an electoral defeat of the incumbent impacts government action:

> *A change in government stops everything, because of the transition... The population suffers as a result. For example, we were a reference municipality in the fight against dengue, but because of that transition dengue cases have increased by over 500%. Pregnant women who used to do pre-natal check-ups regularly stopped, which led to fetal deaths, infant deaths, etc. [...] Workers stop working. Those who are in temporary contracts are dismissed, and contracts for example for transportation are canceled. The outgoing mayor does not want to have any more expenses. [...] Tenured professionals stay but with no conditions to do their job, with no materials.*

I therefore hypothesize that dismissals increase after an electoral defeat of the incumbent (*hypothesis 6a*), as a result of their desire to leave municipal accounts within legal limits and of their fear that the opposition will facilitate their prosecution once they come into office in January. This hypothesized contraction of the bureaucracy at the end of a politicians' mandate would thus result from the combination of the election results and the limits imposed by the LRF. If the incumbent wins the election, on the other hand, the need to adjust the accounts is weaker because they will keep control of the municipal government (and the accounts) for four more years.

I hypothesize that the arrival of a new government is accompanied by an increase in hiring (*hypothesis 6b*). This is well shown by Akhtari et al. (2018), but my logic is somewhat different. I argue it is not simply because of a new government's desire to influence the bureaucracy, but also and critically due to their need to fill in the gaps by an outgoing administration that contracted the bureaucracy because of fiscal discipline constraints. Finally, I hypothesize that bureaucratic outputs and outcomes also contract in the last quarter of an electoral year following a defeat of the incumbent, either unconditionally (*hypothesis 6c*) or conditional on a significant fraction of the bureaucracy being non-tenured, and thus sensitive to contraction decisions (*hypothesis 6d*).

5 Research design

To test these hypotheses on political bureaucratic cycles, I employ a multi-methods empirical strategy. First, I am interviewing key agents in the field, in a variety of economic and political contexts, as a strategy for both developing and probing hypotheses about politicians' strategic behaviors and the mechanisms through which they impact bureaucratic inputs and outputs. In particular, I am interviewing municipal secretaries of education, healthcare, and finance; mid-level bureaucrats in healthcare and education; and state prosecutors, in municipalities across four states in Brazil (Ceará, Goiás, Rio de Janeiro and Minas Gerais). Second, I leverage time-series cross-section regressions to examine political bureaucratic cycles, and explore some of their political correlates. Third, I use a quasi-experimental design to test for the effect of an electoral defeat of the incumbent on political bureaucratic cycles.

5.1 Identifying political bureaucratic cycles

As a baseline model, I estimate the following equation with time-series cross-section data at the municipality-month level:

$$Y_{iym} = \sum_{j=2}^{J} \alpha_j \mathbf{I}[j = iy] + \sum_{n=2}^{12} \theta_n \mathbf{I}[n = m] + \sum_{p=1}^{12} \beta_p \mathbf{D}_{iym}^{p} + \gamma Y_{iym-1} + \varepsilon_{iym} \quad (1)$$

Y_{iym} is a given outcome (for example, the number of new contracts or dismissals) for municipality i in year y in month m. α_j is a municipality $\times$ year fixed effect which flexibly controls for yearly-invariant municipality-specific characteristics (such as levels of economic or political development), as well as for municipality-specific yearly shocks that may affect hiring, such as variations in municipal government revenue or local labor markets. θ_n is a matrix of month fixed effects, which control for monthly shocks common to all municipalities and thus account for underlying seasonality in public employment (for example, due to fiscal-year trends).[21] $\mathbf{D}_{iym}$ is a matrix of indicators for whether observation iym belongs to an electoral cycle month, which I define as the 12 months between April of an election year and January of the post-election year, i.e. roughly 6 months before and after the first round municipal elections.[22] Y_{iym-1} is a lag of the dependent

[21] I take January as the baseline category, so the January fixed effect θ_1 drops from the estimating equation.

[22] This is, in effect, an interaction term between the month fixed effects θ_n and a "treatment" dummy for whether

variable.[23] Finally, ε_{iym} is an idiosyncratic error term. I cluster standard errors at the municipality level to allow for arbitrary serial correlation and heteroskedasticity.

β_p is a vector of coefficients corresponding to electoral cycle month effects, with each β_p for each month in the 1-year period around elections. This vector identifies the presence of PBCs as long as assignment into treatment (i.e. elections) is independent of the potential outcomes of observation iym or, more formally, $D_{iym} \perp\!\!\!\perp \{Y_{iym,1}, Y_{iym,0}\}$, where $Y_{iym,1}$ is the potential outcome of observation iym under elections and $Y_{iym,0}$ its potential outcome without elections. The fact that since 1998 elections in Brazil always take place on the first Sunday of October, as per the constitution, makes treatment exogenous in this setting. On the other hand, the treatment is not probabilistic, since elections take place at the same time for all municipalities. This would be particularly worrying for causal inference if we did not have data for multiple electoral cycles, such that we could not separate seasonality and election effects – in that case, for example, we would not be able to tell whether public employment decreases in the fall due to elections or to an idiosyncratic shock that coincided with elections. One might still worry about confounding, but with four municipal election cycles in the period I study (2002-2016), it is hard to think of a confounder that would be unrelated to elections but would systematically alter patterns of bureaucratic inputs and outputs every four years, coinciding with the municipal electoral season.[24]

I estimate Equation 1 with a number of dependent variables. For my baseline models, I use the flow of contracts (new contracts and dismissals) and the stock of contracts, for all contracts, for tenured contracts, and for non-tenured contracts. These are count variables with a very skewed distribution, so I use a natural log transformation, adding 1 in order to keep the observations with zeroes since the log of 0 is undefined.[25] I also examine political bureaucratic cycles in numbers not of contracts but of unique individuals and of Brazilian reais corresponding to contracts (as per their mean salary). Finally, I switch from examining bureaucratic inputs (jobs) to studying bureaucratic outputs and outcomes. I do so by exploiting municipality-month data on healthcare outputs and outcomes available from the Ministry of Healthcare, from 1995 to 2015. Here I focus on pre-natal

they are within a 6-month window of municipal elections.

[23] While the inclusion of a lagged dependent variable and fixed effects may raise concerns about Nickell bias (Nickell, 1981), remember this sort of bias is proportional to 1/T, where T is the number of periods in the panel, and is thus negligible in this setting, with over 179 periods in baseline models. Still, in the future I will examine robustness to using a GMM estimator (Arellano and Bond, 1991).

[24] Nonetheless, I am exploring the possibility of exploiting supplementary elections, which are called at other points in time in a small fraction of municipalities, for example where October elections have over 50% of null votes, or when the winner's candidacy is declared invalid ex post.

[25] I then show robustness to alternative solutions such as not logging the dependent variable, logging it and dropping observations with zeroes, and using a non-linear model.

checkups and on child mortality due to avoidable causes, but in the future I plan to examine other healthcare variables as well.[26]

In a first strategy to uncover heterogeneity in PBCs, I expand the regression in Equation 1 to include covariates that the literature suggests condition PBCs, including the level of electoral competition (measured by the fragmentation of mayoral votes in the previous election, using a Herfindahl index),[27] whether the mayor is in their first term (and thus eligible for re-election), the mayor's partisanship (using the three-categories classification of parties of Power and Zucco Jr (2009)), whether the mayor is a co-partisan of the President, and whether the mayor is re-elected. In the case of electoral competitiveness, which is the only continuous variable, and in order not to impose linearity (Hainmueller et al., 2017), I generate a dummy variable based on whether an observation is at or above the median. To examine heterogeneity in PBCs, I interact contextual variables with both the electoral-cycle month dummies and the month fixed effects, as per the equation below:

$$Y_{iym} = \sum_{j=2}^{J} \alpha_j \mathbf{I}[j = iy] + \sum_{n=2}^{12} \theta_n \mathbf{I}[n = m] + \sum_{p=1}^{12} \beta_p \mathbf{D}_{iym}^{p} + \gamma Y_{iym-1} + \tag{2}$$

$$+ \sum_{k=1}^{K} \left(\sum_{n=2}^{12} \phi_n^k C_{iym}^k \mathbf{I}[n = m] + \sum_{p=1}^{12} \delta_p^k C_{iym}^k \mathbf{D}_{iym}^{p} + \lambda_k C_{iym}^k \right) + \varepsilon_{iym} \tag{3}$$

Where $\mathbf{C}_{iym}^k$ is a matrix of K contextual variables for observation iym. In this setting, the vector of coefficients δ_p^k correspond to the association between the election cycle month dummies and the outcome of interest, conditional on the presence of contextual variable k. Note that the base term of the contextual variables C_{iym}^k drops out of the estimating equation in most cases because it is perfectly collinear with the municipality $\times$ year fixed effects α_j.[28]

[26] I plan to exploit administrative, micro-level data that I have obtained from the Ministry of Healthcare for more refined measures.

[27] The Herfindahl index is a common metric of market fragmentation, defined as $H = \sum_{n=1}^{N} v_n^2$, where N is the number of candidates for mayoral elections and v_n is their vote share.

[28] The exception being alignment with the President, the only contextual variable that has within-year, within-municipality variation.

5.2 Causally identifying heterogeneity in political bureaucratic cycles

Existing research on PBCs has paid relatively limited attention to issues of causal identification. Progress has been done with the timing of elections, even in institutional settings where the schedule is fixed and constitutionally enshrined (Baskaran et al., 2015; Pierskalla and Sacks, 2017). However, little attention has been paid to the issues of causally identifying heterogeneous treatment effects. It is common in the literature to simply split the sample by, or interact the election-period dummies with, a contextual variable of interest. These strategies, however, ignore that changes in that contextual variable of interest are endogenous and hence results could be driven by other confounding variables.

To address this challenge, I leverage a quasi-experimental design to causally identify heterogeneity in political bureaucratic cycles along one variables that is particularly relevant from a political economy point of view: the electoral performance of the incumbent.[29] I use a regression discontinuity design (RDD), which in essence compares municipalities where the incumbent barely loses the election to those where they are barely re-elected, to identify the effect of the mayor's electoral performance on political bureaucratic cycles after the election (i.e. from October onward).

The core of the regression discontinuity design is a forcing variable (in this case, the difference between the vote share of the runner-up or strongest challenger and the vote share of the incumbent) with treatment (in this case, an electoral defeat of the incumbent) determined sharply at a given threshold (in this case, 0). If the forcing is above zero, the incumbent lost the election and a new administration comes in on January 1st. Conversely, if that difference is negative, the incumbent won the election and there is no change of mayor on January 1st. Intuitively, this allows us to interpret a discontinuous jump of the outcome variable at the threshold as the causal effect of an electoral defeat of the mayor. More formally, treatment for observation iy, T_{iy}, is assigned by the forcing variable, which is the difference between the vote share of the runner-up (V^o_{iy}) and the vote

[29] I am currently working on a similar close-races designs to analyze the effect of the incumbent's partisanship, gender, and alignment with the governor. I will also exploit a regression discontinuity design to analyze how PBCs vary by the additional constraints imposed by the LRF on municipalities above certain thresholds of personnel expenses. I plan to use that identification strategy to study the consequences of PBCs on measures of welfare and on the electoral performance of the incumbent. A stream of papers have examined whether voters reward PBCs in the polls but there are, to my knowledge, no studies with a good identification strategy for the effect of cycles on electoral support for the incumbents, and the correlational evidence is mixed (Brender and Drazen, 2008; Klomp and De Haan, 2013; Drazen and Eslava, 2010; Sakurai and Menezes-Filho, 2008; Bojar, 2017).

share of the incumbent (V_{iy}^g) : $D_{iy} = V_{iy}^o - V_{iy}^g$).

$$T_{iy} = \begin{cases} 1 & \text{if } D_{iy} > 0 \quad \text{(incumbent loses election)} \\ 0 & \text{if } D_{iy} < 0 \quad \text{(incumbent wins the election)} \end{cases} \tag{4}$$

My estimand of interest is $\tau = E[Y_{1iy} - Y_{0iy}]$, where Y_{1iy} and Y_{0iy} represent the potential outcome of interest (e.g. number of new contracts in November), under treatment (incumbent loses the October election) and under control (incumbent wins the election). Note that we only observe the outcome of a given observation either under treatment or under control. That is to say, $Y_{1iy}|T_{iy} = 0$ and $Y_{0iy}|T_{iy} = 1$ are unobserved: this is the fundamental problem of causal inference. As long as average potential outcomes are continuous, we can estimate the local average treatment effect (LATE) around the cutoff by taking the difference in the difference in means from above and from below the threshold:

$$\tau = E[Y_{1iy} - Y_{0iy}|D_{iy} = 0] = \lim_{D_{iy} \downarrow 0} E[Y_{1iy}|D_{iy} = 0] - \lim_{D_{iy} \uparrow 0} E[Y_{0iy}|D_{iy} = 0] \tag{5}$$

This is the LATE for municipalities around the threshold, namely where incumbents run and they barely lose or barely win the election. In that sense, estimates of the treatment effect stemming from this design have limited external validity. Nonetheless, since we are interested in the effect of the incumbent losing the election, the LATE for municipalities close to the threshold (those where the incumbent may plausibly switch from losing to winning, or vice versa) is a meaningful quantity of interest.

The key assumption of this design is that potential outcomes are continuous around the threshold, so that the mean of the outcome of municipalities barely treated is a valid counterfactual for the mean of the outcome of municipalities barely untreated. Formally, I am assuming that $E[Y_{diy}|D_{iy} = d]$ is continuous in d around $D_{iy} = 0$ for both the treatment and the control groups (Imbens and Lemieux, 2008). While this assumption is empirically untestable, we can examine some of its observable implications. A key implication of the continuous potential outcomes assumption is that municipalities do not sort around the threshold. Obviously incumbents will try and win the election, but so will the runner-ups, so if the design is valid we should not see discontinuous jumps around the threshold. Figure 19 in the Appendix shows the histogram and density of the forcing

variable, which has roughly a normal distribution and no signs of sorting or discontinuity around the threshold, as confirmed by the formal test proposed by McCrary (2008). The key is that while incumbents have influence over the difference between the vote share and that of their strongest challenger, they cannot manipulate it *precisely*, which guarantees that, for municipalities around the threshold, treatment assignment is as-if-random (Lee and Lemieux, 2010).

RDD's require specifying the functional form of the regression on both sides of the cutoff, and choosing a bandwidth, i.e. the range of the forcing variable beyond which observations are excluded from the analysis. I follow the common practice of using local linear regression with a triangular kernel smoother, and apply it to the following estimating equation:

$$Y_{iy} = \alpha + \beta_1 T_{iy} + \beta_2 D_{iy} + \beta_3 T_{iy} D_{iy} + \gamma_y + \varepsilon_{iy} \tag{6}$$

Y_{iy} is the outcome of interest (e.g. new contracts in October) for municipality i in the electoral cycle y. T_{iy} is a treatment indicator: $\mathbf{1}$(vote share of strongest challenger $\geq$ vote share of the incumbent). D_{iy} is the distance to the threshold in the forcing variable for observation iy. γ_y is an election fixed effect and ε_{iy} is an error term. If the RDD assumptions hold, β_1 in Equation 6 identifies the LATE in Equation 5: $\beta_1 = \hat{\tau}$. To estimate standard errors, I use the HC1 heteroskedasticity consistent estimator. To choose the bandwidth, I use the algorithm proposed by Imbens and Kalyanaraman (2012), which determines an optimal bandwidth by minimizing the mean squared error. I then show the sensitivity of the results to many alternative bandwidths around the optimal one.

For each month between October and March after an election, I run a separate model following Equation 6, which allows me to causally identify the effect of an electoral defeat for the incumbent on outcomes from 0 to 5 months after the elections. For bureaucratic inputs (jobs) data, I use data for the election cycles of 2004, 2008, 2012 and 2016. Because the healthcare data is available until 2015 only, RDDs examining healthcare outputs and outcomes use data for the election cycles of 2004, 2008 and 2012 only.

6 Data

I exploit three sources of administrative data. First, micro-level data of the universe of municipal jobs, obtained from the Ministry of Labor. Second, administrative data on elections and candidates' characteristics, from the Supreme Electoral Court (TSE). Third, data on healthcare outputs and outcomes at the municipality-month level, from the Ministry of Healthcare.

The main data source is the Ministry of Labor's RAIS dataset (*Relação Anual de Informações Sociais*), which includes the universe of formal labor market contracts, from 1985 to 2016, with employer and individual identifiers. For each contract, the data contain among other variables the employer and the employee's unique identifiers; the date of hire and fire;[30] the contract's type, the job's professional category, hours, and salary; and the employee's age and level of education.[31] I use municipal government employer ID's[32] to identify municipal employees. As shown in Table 1, for each year RAIS contains data on millions of contracts in municipal employment, roughly two thirds of which correspond to tenured civil servants. The table also illustrates two important limitations of the dataset. First, between 1 and 5 percent of the municipalities do not show up as effective employers in RAIS. This may be due to administrative data entry errors or to low capacity among some municipalities. Second, unique identifiers for employees are very low quality in 2002, and unavailable before that date. I thus focus baseline analyses in the period 2002-2016. In order to keep panels balanced, I exclude from the analyses observations corresponding to the 661 municipalities that, in at least one of the years between 2002 and 2016, showed up as having 0 employees in the month of January, including municipalities that did not exist in the beginning of the period. This leaves me with a balanced panel of 4,909 municipalities, 73,647 fixed effects, and 878,711 observations for baseline models.[33]

For bureaucratic outputs I use data from the Ministry of Healthcare at the municipality-month level. First, as a measure of bureaucratic outputs I look at the number of pre-natal check ups done by doctors or nurses, at clinics or at home.[34] As a measure of bureaucratic outcomes I look at

[30] For some years the data reports the day, month and year, while for others only the month and year is reported. This determines the need to analyze data at the monthly level.

[31] For more information about RAIS, see http://www.rais.gov.br/.

[32] Municipal employer ID's (CNPJ, *Cadastro Nacional da Pessoa Jurídica*) were obtained from the Ministry of Finance's National Secretariat of the Treasury (STN, *Secretaria do Tesouro Nacional*).

[33] Other fact visible in Table 1 that is worth noting is that both the number of municipal employees and the share tenured have decreased in recent years, possibly because of the impact of the economic crises on government finances.

[34] The data are collected by municipal secretariats of healthcare, consolidated by state governments, and cleaned

Table 1: Micro-data of municipal employees, 2002-2016 – Descriptive statistics

Year	# municipal employers	# contracts (millions)	Non-tenure contracts (%)	# individuals hired (millions)	Invalid employee IDs (%)
2016	5449	5.98	32.20	5.44	0.00
2015	5496	6.04	33.32	5.46	0.55
2014	5507	6.08	34.03	5.52	0.54
2013	5486	6.10	35.21	5.50	0.54
2012	5483	5.86	34.41	5.35	0.53
2011	5480	5.70	35.25	5.21	0.55
2010	5496	5.53	35.44	5.06	0.53
2009	5469	5.36	35.26	4.93	0.52
2008	5472	5.16	34.05	4.73	0.55
2007	5475	4.81	33.58	4.46	0.57
2006	5481	4.57	33.36	4.25	0.62
2005	5431	4.24	32.76	3.97	0.77
2004	5366	3.90	29.83	3.66	0.87
2003	5350	3.76	30.29	3.53	0.98
2002	5309	3.61	32.35	0.21	94.06

the number of deaths for children aged 1-4 due to avoidable causes, namely those attributable to weaknesses in the healthcare system.[35]

7 Results

I present three sets of results. First, I present results of unconditional political bureaucratic cycles, stemming from estimating Equation 1 with different outcomes (stock and flows of total contracts, tenured contracts, and temporary contracts, as well as pre-natal checkups and child mortality). Then, as a first approximation to looking into how the political context impacts PBCs, I present results about conditional bureaucratic cycles stemming from estimating Equation 2 on the logged number of contracts, by contract type. Finally, I present results from estimating the regression discontinuity model of Equation 6 to examine how an electoral defeat for the incumbent in October

by the federal government's Basic Healthcare Information System (SIAB, *Sistema de Informação da Atenção Básica*). More information is available at http://tabnet.datasus.gov.br/cgi/siab/At_bas_prod_marca_desde_1998.pdf.

[35]The data are collected by municipal secretariats of healthcare using official death records, and reported to the federal government. The data are then aggregated in the Mortality Information System (SIM, *Sistema de Informações sobre Mortalidade*). More information can be found at http://tabnet.datasus.gov.br/cgi/sim/Obitos_Evitaveis_0_a_4_anos.pdf.

impacts PBCs in contracts and healthcare outputs for the months from October to March after the election.

7.1 Unconditional political bureaucratic cycles

My baseline models examine the existence of political bureaucratic cycles in the flow (hires and dismissals) and the stock of the logged absolute number of contracts, by contract type (all, tenured, and non-tenured), following Equation 1 in page 16.[36] Figure 2 plots the coefficients of interest, namely the monthly election-cycle period fixed effects, together with its 99% confidence interval.[37] Each line corresponds to a separate regression. Table 2 in page 43 in the Appendix presents the results of these nine regressions.[38] The results show that hires and dismissals have cyclical patterns around elections, consistent with hypotheses 1a-1d. Note however that, contrary to hypothesis 1b, during the third quarter of electoral years hiring not only stabilizes but contracts. This may be due to politicians' need to compensate for the hires of the second quarter, and to the higher levels of attention paid by horizontal accountability actors (such as state prosecutors or state courts) during the spending freeze. As expected, these patterns are significantly more pronounced for temporary contracts (for which politicians have higher degrees of discretion) than for tenured contracts (although these show cyclical patterns as well), which is consistent with hypothesis 2a.

Looking at the results for hires first (Figure 2a), we see that municipal governments expand hiring in the months leading up to the personnel spending freeze imposed by the LRF, particularly in June and July.[39] Hires are then lower than in off-election years for the months under the spending freeze, especially before the election but even after the election in October. Finally, there is a large peak of hires in the first few months of the post-election year, particularly in January. These effects are statistically significant, in most cases with p-values below 2×10^{-16}.

The magnitude of these monthly election-cycle effects is large. Since the dependent variables are in the logged scale, coefficients are to be interpreted as follows: other things being equal, a municipality sees its hires (/ fires / total stock of employees) changed by $100 \times e^{\hat{\beta}} - 100$ % in a

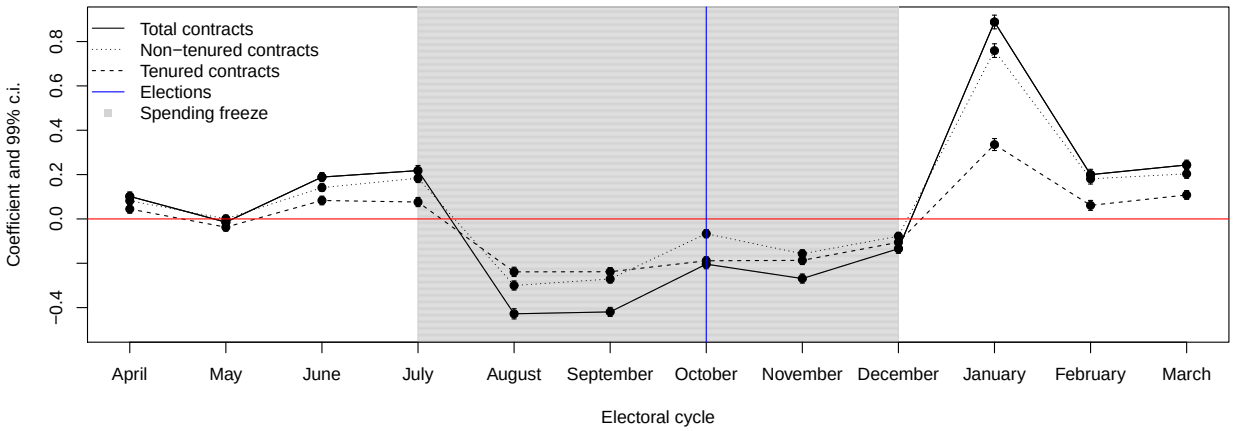
[36] As explained in Section 5, I add 1 to the dependent variable in order not to drop observations with zero contracts when taking the log.

[37] Some confidence intervals are so small that they are not readily visible in the graphs.

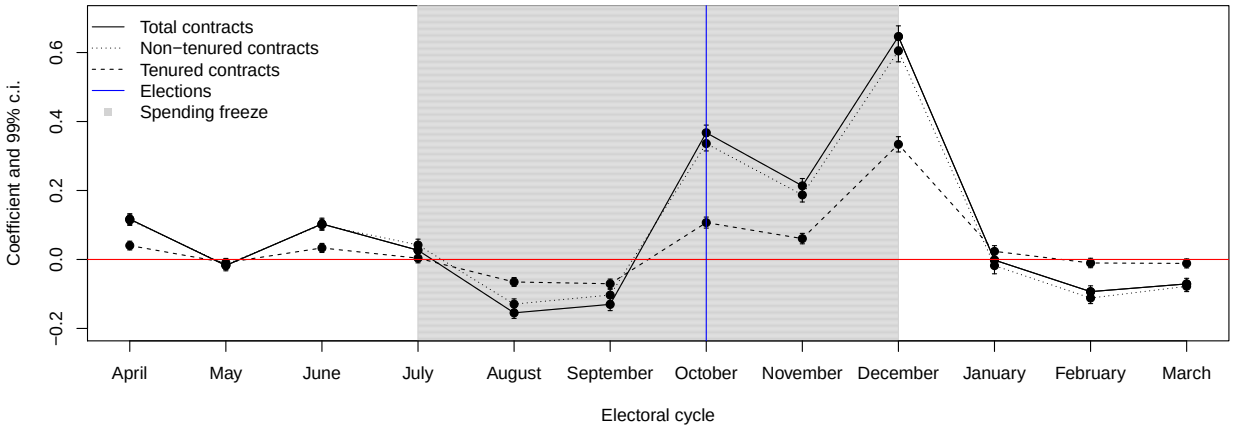
[38] For the sake of brevity, for all other models I only show results in figures, but regression tables are available from the author.

[39] The spending freeze starts 180 days before the end of the mayor's mandate, i.e. on July 4th. Hires on the first of the month (which is the date when contracts tend to start) do not fall under the 180-day period.

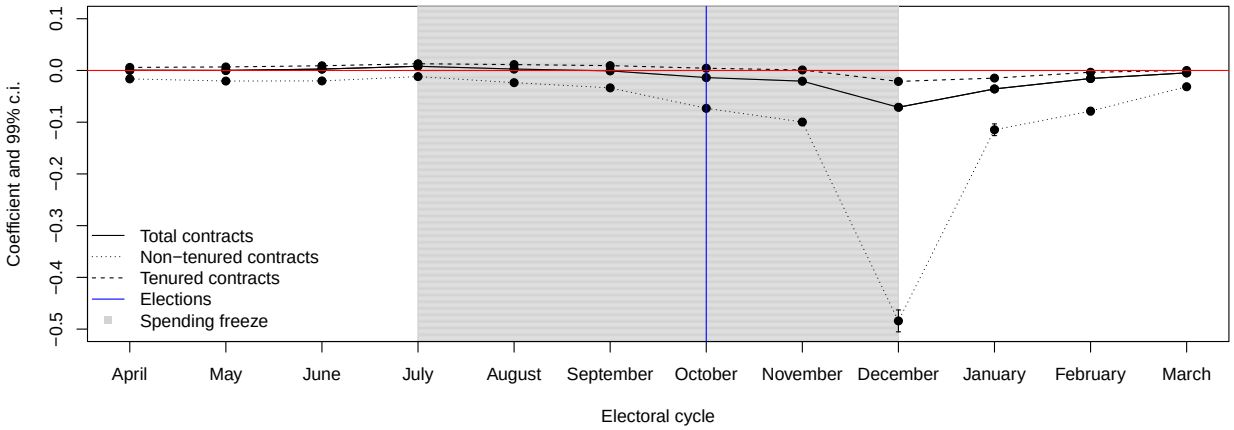
Figure 2: Monthly election cycle effects on logged absolute number of contracts



(a) Flow of new contracts



(b) Flow of dismissals



(c) Stock of contracts

given month of an electoral cycle. Remember that these effects are netted out of seasonality in municipal contracts (controlled for by month fixed effects) and of any yearly-invariant, municipality-specific effects such as levels of economic or political development (controlled for by municipality $\times$ year fixed effects). For small coefficients (say between -0.2 and 0.2), it is a fair approximation to interpret the coefficient, when multiplied by 100, as the percentage change in the dependent variable corresponding to that month in the election cycle.

To be specific, the results show that the number of new municipal contracts in June of an electoral year is, other things being equal, about 20.8% higher than in June of a non-electoral year ($p < 2 \times 10^{-16}$). This effect is stronger when we look only at non-tenured contracts (15.1%) than when we look at tenured contracts (8.6%). In contrast, hires in August are 34.8% lower in an electoral year than in a non-electoral year, other things being equal ($p < 2 \times 10^{-16}$). In December, hires are lower than in non-electoral years by about 12.6% ($p < 2 \times 10^{-16}$). In January of an election cycle (i.e. the first month of the post-election year, when the hiring freeze is no longer in place), hiring is, other things being equal, 143% higher than in a January that does not follow elections ($p < 2 \times 10^{-16}$). This major increase makes sense since it follows a 6-month period of personnel spending freeze and intensified vigilance over municipal finances. Nonetheless, it is important to keep in mind that temporary contracts in the education sector (the most important policy area in Brazilian municipal governments, both in terms of budget and of personnel) are often made later on, since the school year goes from March to December.[40] That is probably one of the reasons why we see significant increases in hiring in February and March of post-electoral years, compared to February and March of other years (of 22.1 and 27.6%, respectively, with $p < 2 \times 10^{-16}$).

Dismissals of municipal employees also show cyclical patterns, as can be seen in Figure 2b. Dismissals decrease in the months between the beginning of the personnel spending freeze and the elections, and increase by a lot in the months between the election and the end of the year, which is consistent with hypothesis 5a. To be specific, results show that in August of an electoral year there are 14.3% less dismissals than in August of a non-electoral year, other things being equal ($p < 2 \times 10^{-16}$). On the other hand, dismissals increase by 44.4 and 90.9 % in October and December respectively ($p < 2 \times 10^{-16}$), which is consistent with the hypothesis that municipal governments use the post-electoral months to adjust their finances for the fiscal year, after the pre-electoral expansions. Regressions also show however that firings increase in the months before the spending freeze comes into effect (except in May). This could respond to politicians' desire to

[40] Month fixed effects for all employees, healthcare employees and education employees are shown in Figures 15, 16 and 17 respectively, in the Appendix.

"open space" in the bureaucracy to have some hiring maneuver during the last semester of their mandate.

Finally, Figure 2c shows the results when we take the stock of municipal employees as the dependent variable. Given that the stock is more path dependent than hires and dismissals, in these regressions we find much smaller coefficients and significantly higher values for the R^2.[41] In general, the total stock of employees inflates between June and August (by between 0.27 and 0.81%, with p-values below 1.8×10^{-6}), and decreases between October and March (by between 0.5 and 7%, with $p < 2 \times 10^{-16}$), when compared to the same months off the election cycle. Results also show an important divergence by contract type: the stock of temporary employees decreases in electoral cycles (especially between November and January), while the stock of tenured employees increases slightly between April and October, and decreases between December and February). In general, the stock results suggest that the firing of employees during electoral years dominates the effect of the new hires when it comes to temporary employees, possibly due to the restrictions that the LRF imposes on municipal finances in electoral years.

In general, it may seem surprising that Figure 2 uncovers cycles not only in temporary but also in tenured contracts. Oftentimes the literature, with a Weberian bias, assumes that tenured bureaucrats are isolated from political influence.[42] My quantitative results and my interviews in the field suggest that while tenured bureaucrats are more protected from political influence than non-tenured ones, political dynamics matter in that field as well. How do politicians manage to influence the hiring and firing of tenured bureaucrats? First, calls for hiring tenured bureaucrats are usually made by politicians, and thus the timing of hiring responds partly to political dynamics, also in high-income countries. Second, while hiring of tenured bureaucrats is usually more rule-bound than the hiring of temporary ones, the openings can still be targeted, for instance through general education or experience requirements. Third, exams for tenured bureaucrats are not immune to fraud and manipulation – for example, some cases have been detected and prosecuted in Brazil. Fourth, and this is perhaps peculiar about the Brazilian context, the government is not obliged to hire those who pass a competitive examination for a tenured position. The government is legally obliged to hire in the order of performance in the exam, and to hire those approved for tenure before hiring temporary workers for the same job, but the timing of when those approved are made effective bureaucrats is often politically influenced (and may even depend on the relative placement of candidates in the exam). In fact, in an online survey of school directors of the state of Mato

[41] See columns 7-9 in Table 2 in page 43 in the Appendix.

[42] In fact, some studies examine PBCs in tenured bureaucrats as a placebo test, e.g. Pierskalla and Sacks (2017).

Grosso that I did in April of 2018, over 50% of them said that politics influences quite a lot or a lot the hiring of tenured teachers, less than 26% of them said that politics has no influence at all.[43] As for dismissals, the constitution allows for the dismissal of tenured bureaucrats under certain conditions (e.g. judicial sentence, bad performance), although in practice it is very unlikely for a tenured bureaucrat who has overcome the initial probationary period to be dismissed.

To make sure that the cyclical patterns uncovered in Figure 2 are not driven by the particular way in which I operationalize the dependent variables, I replicated the analyses with different operationalizations. Results included in the Appendix show similar patterns when we look at the logged number of hires and dismissals as a share of the stock in that given month (Figure 8), the logged number of unique employees (Figure 9), the logged number of Brazilian reais corresponding to contracts' mean salaries (Figure 10), the un-logged absolute number of contracts (Figure 11), or the logged absolute number of contracts excluding observations without adding 1 and thus omitting observations with a zero (Figure 12).

To sum up, results show that hires increase in spending years before the spending freeze, decrease during its duration, and increase again after the end of the freeze, while dismissals increase heavily in the months between the election and the end of the year. This is quite informative of politicians' responsiveness to the combination of incentives and constraints stemming from elections and the LRF, but not much about who they target with their employment decisions. Is the pre-election expansion of the bureaucracy more clientelistic, or more programmatic?

As a first approximation to this question, I examine PBCs in the share of municipal employees (hires, dismissals, and stock) with an education level above the median, i.e. more than a high school diploma. Results are shown in Figure 3.[44] Figure 3b shows that hires in June and July tend to have lower education levels on election years than off-cycle years, higher education levels between August and November, and again lower in January and February. This suggests that the pre-electoral inflation of the bureaucracy is targeted at individuals with low-education levels. This is consistent with hypothesis 5a and with these efforts being clientelistic, but could respond as well to politicians' desire to boost low-skill bureaucratic activities ahead of the election.[45] On the other hand, considering that hires contract during the second semester of an electoral year, it is perhaps

[43] Survey responses correspond to 80 self-selected municipal school directors from Mato Grosso, with observable characteristics very similar to the universe of municipal directors in the state. The invitation to participate in the survey was sent to over 90% of the state's municipal schools.

[44] Note than in these regressions observations with 0 contracts drop from the estimating equation since division by 0 is undefined.

[45] Testing that hypothesis would require analyzing occupation-adjusted schooling of the contracts.

not surprising that those who get hired tend to have higher educational backgrounds (for example, it may be easier to justify the need to hire a teacher than a cleaner during the duration of the LRF personnel spending freeze). Figure 3b suggests that the contraction of the bureaucracy between the election and the end of the year is at the expense of less educated workers, who may be the more dispensable ones and the ones who are more easily fired. The peaks in the education level of temporary workers shown in Figure 3c are best seen as the result of the cumulative effect of these hiring and firing decisions.

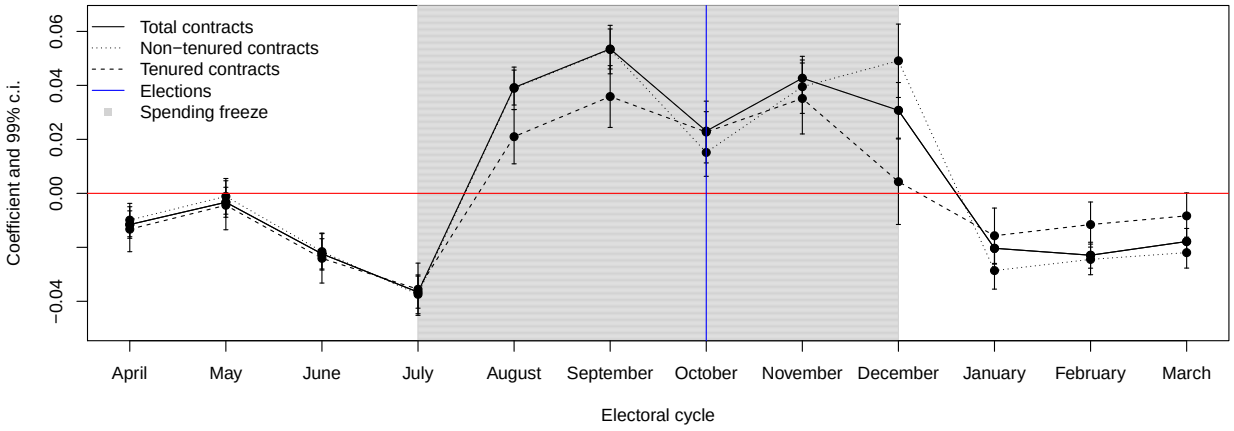
Another way to examine whether PBCs respond to a more clientelistic or more programmatic rationale is to examine bureaucratic outputs and outcomes. I do so by looking at two particularly relevant measures of bureaucratic results in the context of Brazilian municipalities: prenatal checkups and child deaths. Healthcare is the second most important policy area for municipal governments after education and, unlike education, it offers us administrative monthly data on outputs and outcomes. While in the future I plan to examine other existing measures of healthcare outputs and outcomes, pre-natal, infant and child health are meaningful variables to start with since they are critical for human development and have significant variation in Brazil.[46] Examining healthcare results allows me to address whether the combined effects of the elections and the LRF limits on bureaucratic inputs have a correlate on bureaucratic outputs.

I exploit administrative data on the number of pre-natal checkups and child deaths by municipality and month from 2002 to 2015, and run the same estimating equation 1. Results, shown in Figure 4 suggest cycles in bureaucratic inputs shown in Figure 2 are indeed associated with cycles in the results of the healthcare bureaucracy. First, Figure 4a shows that the number of pre-natal checkups decreases in the months around the election, from -2.6% in September to -10% in December, with $p < 9.5 \times 10^{-7}$). Decreases are less pronounced but still significant for the first few months of the post-electoral year. While these results are not necessarily driven (exclusively) by the drop in hires and the increase in fires in the last few months of the year, it is reasonable to expect that they are at least partly related to them, and interviews with bureaucrats indeed suggest so. Moreover, I find very similar patterns of PBCs in jobs to those shown in Figure 2 when subsetting to healthcare professionals only, as shown in Figure 13 in the Appendix.[47]

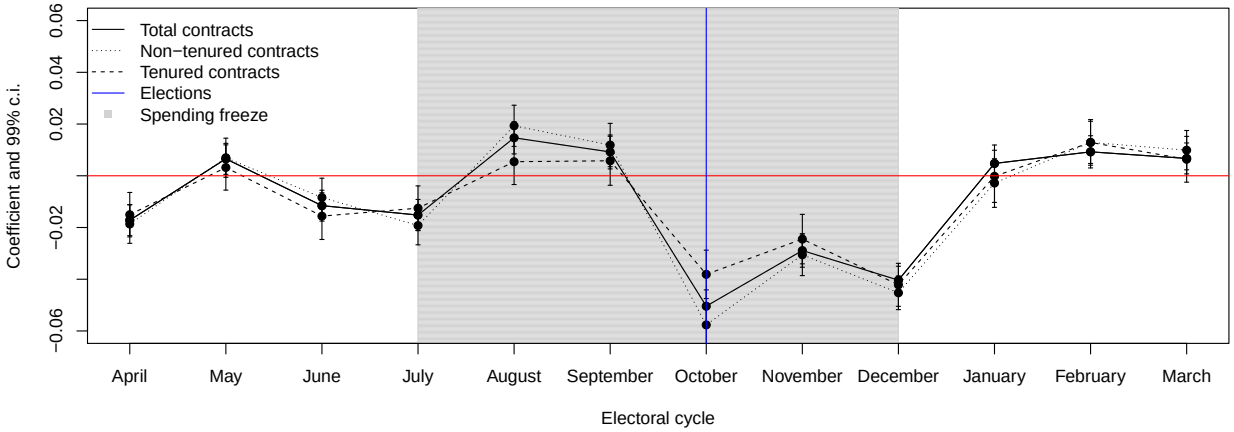
[46] Despite significant improvements over the past few decades, child health figures continue to be alarming in some areas of Brazil. By 2010, child mortality rates was still 1.72% for the whole of Brazil (compared to say 0.5% in high-income countries), with some municipalities having rates around 5%. Data for Brazil come from IBGE, and for high-income countries from the World Bank.

[47] Figure 13shows the results of replicating the analyses in Figure 2 when examining contracts of healthcare professionals only. RAIS includes occupation classifications using the federal government's Brazilian classification of occupations, detailed at http://www.mtecbo.gov.br/cbosite/. Using this classification, I select those municipal

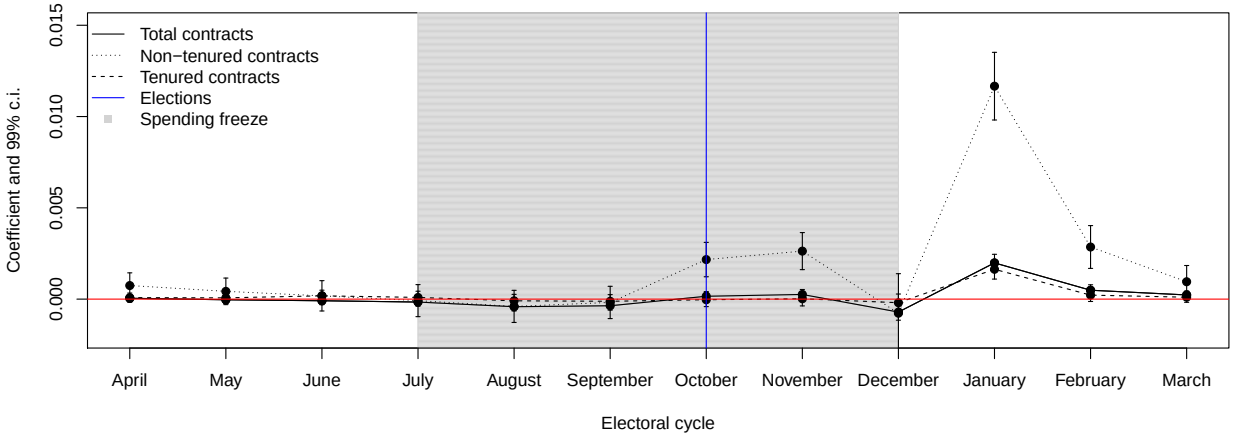
Figure 3: Monthly election cycle effects on the share of contracts corresponding to workers with more than a high school diploma



(a) Flow of new contracts

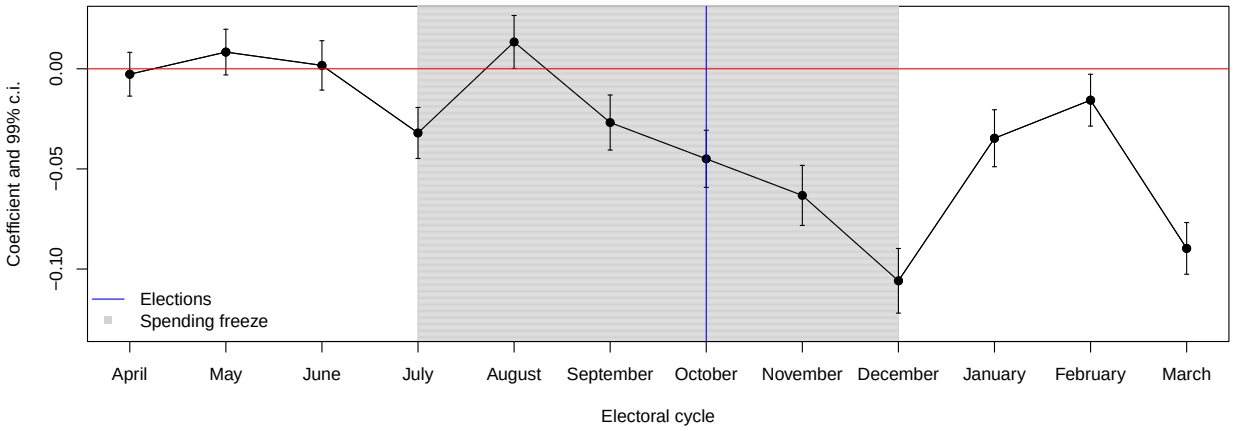


(b) Flow of dismissals

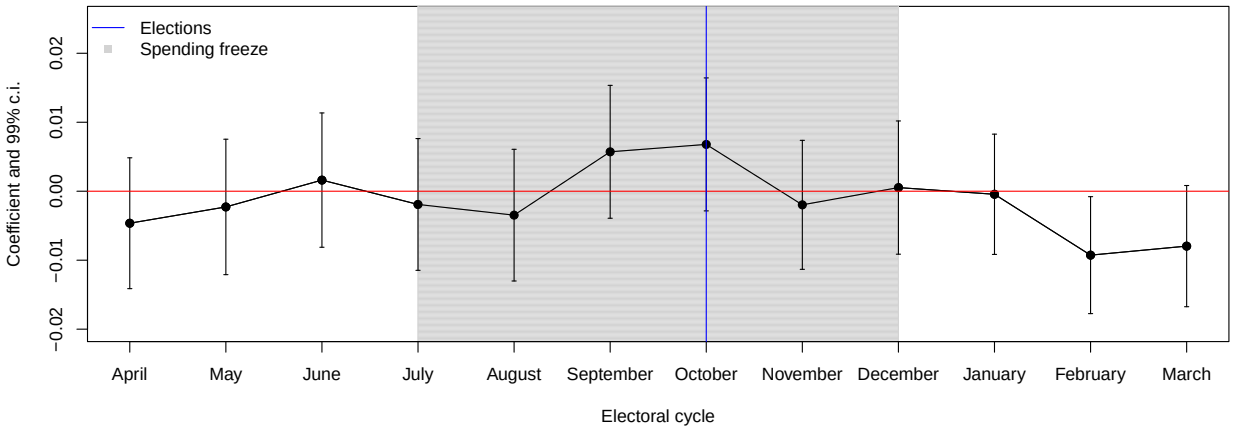


(c) Stock of contracts

Figure 4: Monthly election cycle effects on healthcare outputs and outcomes



(a) Number of pre-natal check-ups conducted (logged)



(b) Deaths of children aged 0-4 (logged)

Figure 4b examines political bureaucratic cycles in one of the most important outcomes of a healthcare system, child deaths due to avoidable causes. This is an outcome that one may expect is less elastic to the effects of hires and fires, and that – to the extent it responds to cycles in bureaucratic inputs – does so with a lag. That is possibly the reason why we do not observe the same cyclical behavior in this outcome variable as we do in bureaucratic inputs and outputs. In fact, most of the electoral-cycle month coefficients are small and statistically indistinguishable from zero, except for the coefficients for February (at the 99% level), March (at the 95% level) and October (at the 90% level). Interestingly however, these results suggest that the outcomes of the healthcare system may worsen on the month of the election, and improve shortly after the beginning of the post-electoral year.

7.2 Conditional political bureaucratic cycles

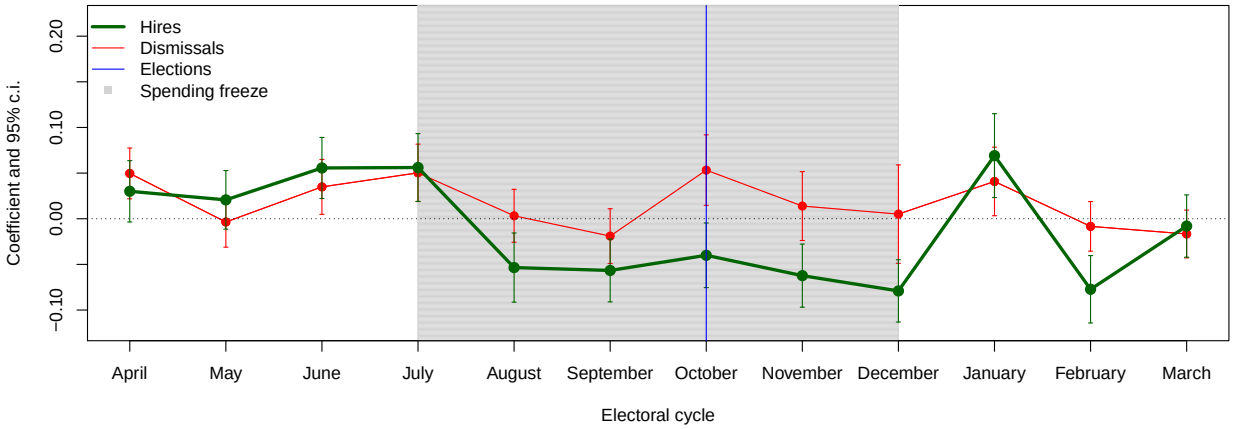
How do political bureaucratic cycles vary with the political context? A long literature on conditional PBCs has examined how factors such as political competition, partisanship, political alignment, or term limits affect the existence and magnitude of cycles. Despite the endogeneity problems that these kind of analyses have (noted in Section 5), here I present correlational evidence stemming from estimating Equation 2 in page 18 with the following contextual variables interacted with the month fixed effects and the electoral-cycle month dummies: electoral competition (as measured by whether a municipality had a Herfindahl index below the median in the previous election), whether the mayor is in their first term and thus eligible for re-election, whether the mayor belongs to a left-wing, center, or right-wing party, whether the mayor belongs to the party of the President, and whether the mayor is re-elected in October.[48] I run separate regressions for hires and dismissals, and for tenured and non-tenured contracts.

Of these seven variables, only three show somewhat consistent results: electoral competition,

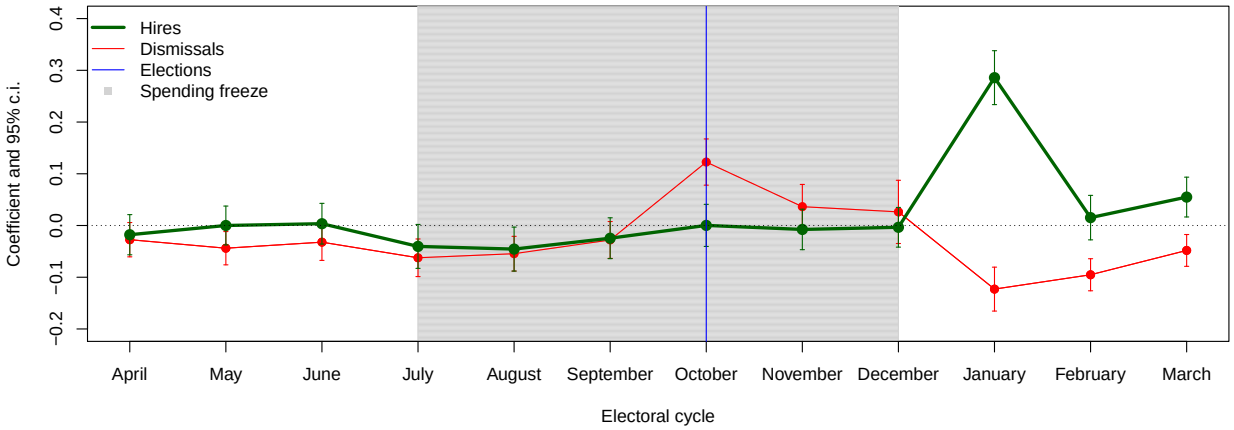
employees with occupational categories that clearly correspond to healthcare workers, such as doctors, nurses and nursing technicians of different specialities, healthcare agents, pharmacists, etc. This classification however does not allow me to select more general workers (such as cleaners or general administrative staff) who also work in the healthcare sector. Results show very similar patterns, although the size of the coefficients is smaller, possibly due to the reduced elasticity of this mid-to high-skill workers. Figure 14 in the Appendix shows similar results when subsetting to education professionals only such as teachers of different specialties, school directors, and pedagogy coordinators.

[48]The choice of contextual variables included here responds to the literature and to data availability. In the future I will include other variables such as alignment with state government, poverty, and relative size of the municipal labor market, as a share of the overall formal labor market.

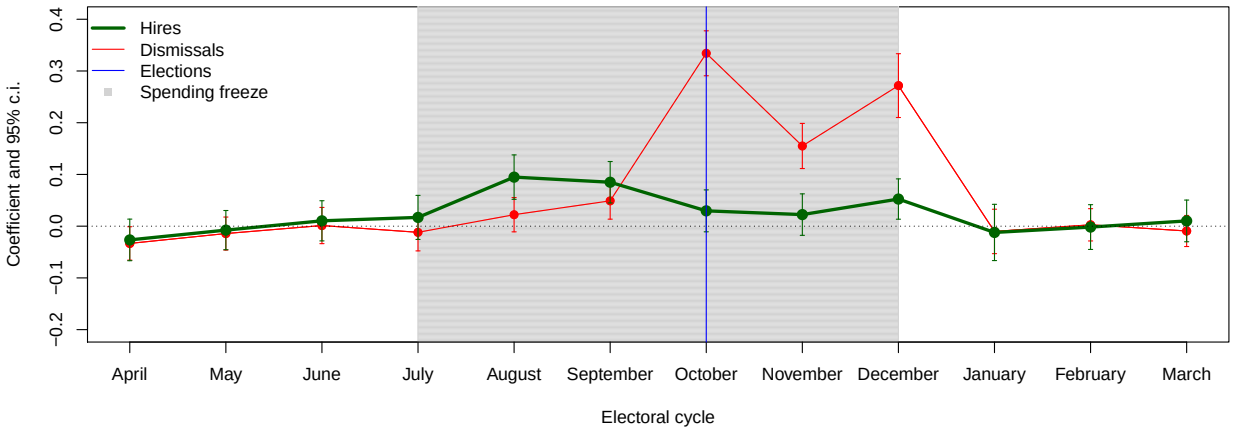
Figure 5: Conditional monthly election cycle effects on logged absolute number of non-tenured, municipal employees



(a) Monthly election-cycle effects conditional on high electoral competition



(b) Monthly election-cycle effects conditional on the incumbent mayor being on their first term



(c) Monthly election-cycle effects conditional on the incumbent losing the October election

re-eligibility, and an electoral defeat of the mayor. Results are, as expected, much more pronounced for temporary contracts (shown in Figure 5 below) than for tenured contracts (shown in Figure 18 in the Appendix), where red and green lines stem from different regressions. Results shown in Figure 5a results suggest that incumbents who were elected under higher degrees of competition take – other things being equal – more pro-cyclical employment decisions, expanding both hiring and firing in June and July, and contracting hiring between August and December, more than in those same months off the electoral cycle when compared to incumbents elected under low levels of competition. This suggests that electoral incentives are associated to political bureaucratic cycles. Results in Figure 5b however show that incumbents who are in their first term, and are thus eligible for re-election, tend to fire less temporary employees in the months leading up to the election, when compared to incumbents in their second term, and more on the month of the election. Finally, results in Figure 5c suggest that incumbents who suffer an electoral defeat in October increase dismissals in the last quarter of the year (before they leave office) than incumbents who do not lose the election.

While these results are interesting, it is plausible that they respond to unobserved confounders, despite the inclusion of the above-mentioned 7 contextual variables together with month and municipality $\times$ year fixed effects. For example, municipalities with higher levels of electoral competition may also have a more active citizenship, more media fragmentation, or other contextual features that might explain the results. To address these concerns, in the following section I show results of a quasi-experimental design examining in particular the effect of an electoral defeat of the incumbent on political bureaucratic cycles.

7.3 Causal effect of an electoral defeat for the incumbent on PBCs

In order to examine how an electoral defeat of the incumbent impacts political bureaucratic cycles, I leverage a close-races regression discontinuity design which essentially compares places where the incumbent barely loses to those where they barely win the municipal election. This design allows us to identify the causal effect of the electoral defeat on hires and dismissals in the months following the election with a high degree of internal validity. On the other hand, these analyses lose some external validity, since now we are only examining places where the incumbent actually runs and where their electoral performance is close to that of the runner-up. We must also switch from a regression with separate election-cycle effects for each month to a framework where we run a separate regression (following Equation 6) for each monthly measure, from October to March, by

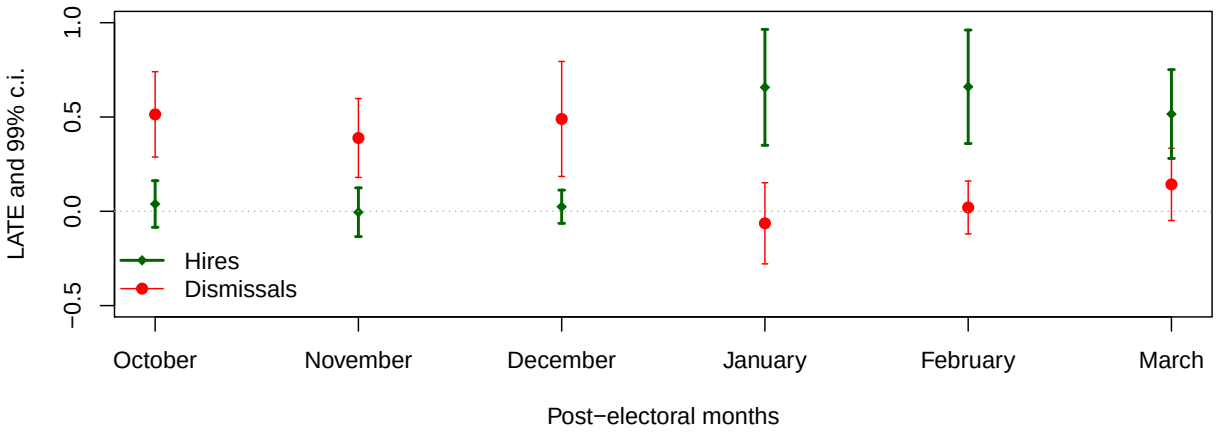
flow type (hires and dismissals) and contract type (temporary and tenured contracts).

The results of these 24 regressions are shown in in Figure 6. Figure 6a shows that, consistent with hypothesis 5a, incumbents who lose the election fire more temporary workers before leaving office (in October, November and December) than incumbents who are re-elected. Moreover, in municipalities where the incumbent loses and thus a new administration takes office on January 1st, the government hires more temporary workers than in municipalities where the incumbent is re-elected. These effects are large and statistically significant. Again, the dependent variable is in the logarithmic scale, so coefficients are to be interpreted accordingly. An electoral defeat of the incumbent causes firings of temporary contracts to increase by 67.2% in October, 47.5% in November, and 63.2% in December, when compared to cases where the incumbent is re-elected ($p < 3.5 \times 10^{-5}$). On the other hand, the electoral defeat of the incumbent causes hires to increase by 92.9, 93.5 and 67.5% in January, February and March after the election, respectively ($p < 3.6 \times 10^{-8}$).

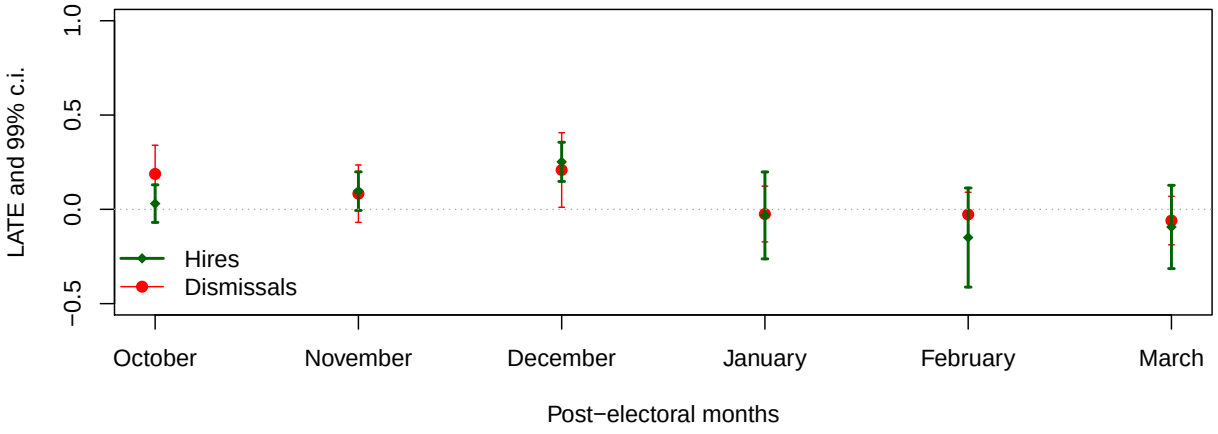
Results shown in Figure 6b show that tenured contracts are not immune to the effects of a defeat of the incumbent. In particular, an electoral defeat of the incumbent causes a significant increase of 20.6% in dismissals of tenured employees in October ($p < 0.01$), and an increase of 28.6% in hires in December ($p < 4.1 \times 10^{-10}$). This is consistent with an effort at shaping the bureaucracy (and tying the opponent's hands) before leaving office, and draws attention to the smaller but still existent discretion politicians have over tenured employees, at least in Brazilian municipalities.

Taken together, these results support my expanded theory of the link between political and bureaucratic turnover. If Akhtari et al. (2018) show that hires increase after political turnover takes place in January, my results show that this is at least partly due to the need to compensate for the dismissals implemented by the outgoing administration. My interviews suggest that these firing decisions are due to outgoing politicians' desire to adjust the municipality's fiscal situation before leaving office, out of fear that they may be prosecuted, while minimizing the opponent's room for maneuver. The regression discontinuity design, the robustness of these results to many alternative bandwidths (as shown in Figure 20 in the Appendix), and the failure of the immense majority of placebo tests run by moving the RD threshold to values of the forcing variable other than zero (as shown in Figure 21 in the Appendix) all lend support to the interpretation of these findings as causal effects. In future versions of the paper I will explore whether an electoral defeat of the incumbent also hurts bureaucratic outputs and outcomes.

Figure 6: Regression discontinuity estimates of the causal effect of an electoral defeat of the incumbent on hires and dismissals after the election, by contract type and month



(a) Effect of an electoral defeat of the incumbent on PBCs for non-tenured bureaucrats



(b) Effect of an electoral defeat of the incumbent on PBCs for tenured bureaucrats

The next question is whether an electoral defeat of the incumbent has an effect on bureaucratic outputs. It is reasonable to expect that with an increase in dismissals, and thus less human resources but potentially also less motivation and accountability, bureaucratic outputs will worsen in the last few months of the year. Moreover, since it may take a while for new teams to be assembled after the new administration takes office, bureaucratic outputs may continue to be worse in the beginning of the year after an electoral defeat of the incumbent, compared to situations where the incumbent is re-elected. To test this, I use Equation 6 with the number of pre-natal checkups as the dependent variable.[49] Figure 7a shows the results. In general, the incumbent losing the election has no effect on the number of pre-natal ckecups after the election.

To explore to what extent this null effect is driven by the fact that in many municipalities most healthcare professionals are tenured, I run the same models interacting treatment (i.e. an electoral defeat of the incumbent) with an indicator for whether the share of healthcare professionals who were not-tenured was – before the election, in September – above the median for all municipalities.[50] Results, shown in Figure 7b show that, in municipalities with a relatively low share of its healthcare professionals under tenured contracts (and thus where the outgoing administration's bureaucratic contraction efforts are less constrained),[51] the electoral defeat of the incumbent leads to a significant decrease in the number of pre-natal checkups in December and January (by about 12.3% and -9.7%, respectively, with p-values < 0.0003).[52]

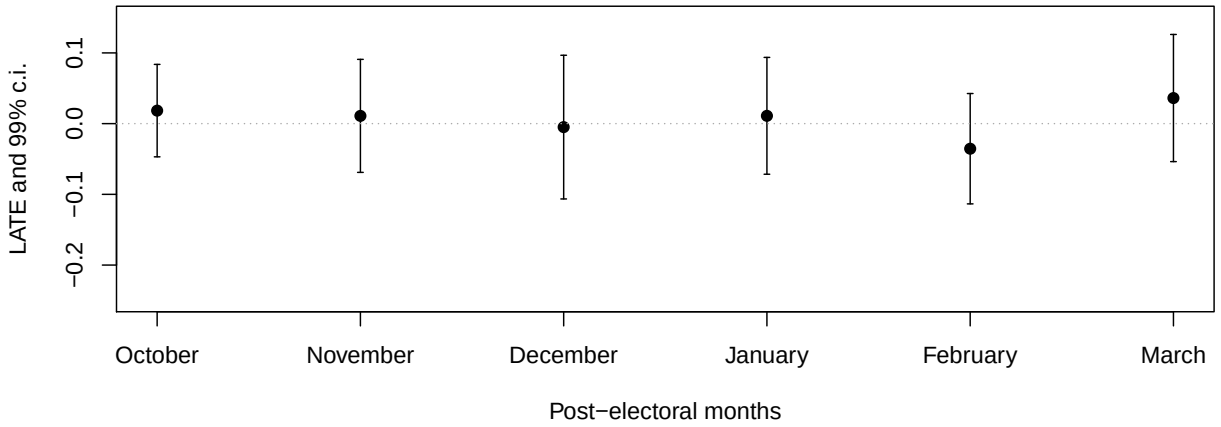
[49] Once again, because this variable is skewed I take the natural log and add 1 to retain observations with no prenatal checkups reported.

[50] In effect, I expand Equation 6 with an interaction term: $Y_{iy} = \alpha + \beta_1 T_{iy} + \beta_2 D_{iy} + \beta_3 T_{iy} D_{iy} + \beta_4 T_{iy} H_{iy} + \beta_5 H_{iy} + \gamma_y + \varepsilon_{iy}$, where $H_{iy} = 1$ if the share of healthcare professionals who are tenured in that municipality and election cycle is below the median of all municipalities in September of that election cycle. The plots show the values of β_4.

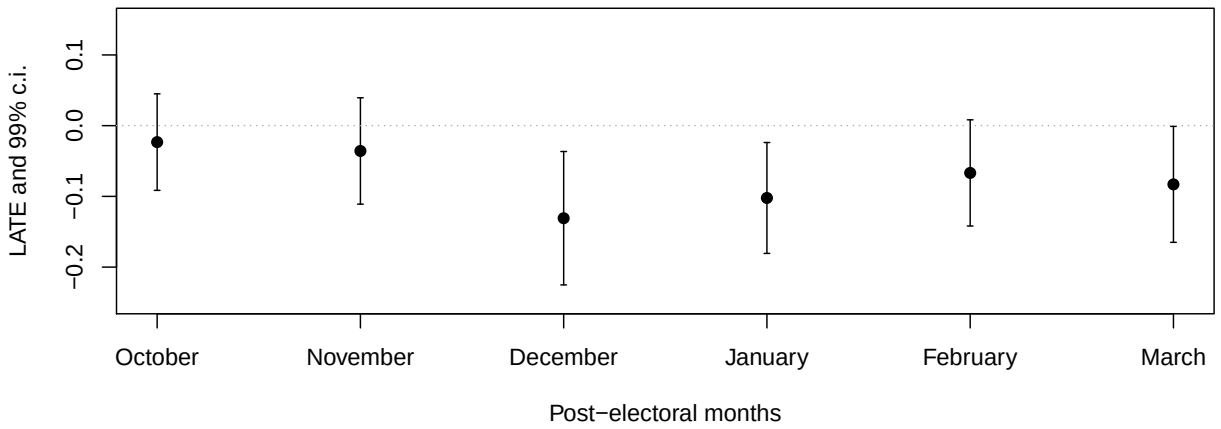
[51] Figure 22 in the Appendix shows the distribution of the share of healthcare professionals who are not tenured across municipalities and election years.

[52] The decreases of -6.5% and -8% corresponding to February and March, respectively, are significant at the 95% level.

Figure 7: Regression discontinuity estimates of the causal effect of an electoral defeat of the incumbent on pre-natal checkups after the election



(a) Unconditional effect of an electoral defeat of the incumbent on prenatal checkups



(b) Effect of political turnover on pre-natal checkups, conditional on the municipality having a low share of healthcare bureaucrats on tenured contracts before the election

8 Conclusion

In this paper I have proposed a theory of political bureaucratic cycles (namely, political cycles in the hiring and firing of bureaucrats, in the activities bureaucrats do, and in the results they achieve), and presented a number of empirical tests using data from Brazilian municipalities. I argue that in low- and middle-income environments, with limited private sector employment opportunities and limited government capacity for implementation, the distribution of public jobs is a critical political resource, particularly around elections. I leverage concepts from decades of research on political business and budget cycles, and on clientelism and patronage, to argue that legal limits on the hiring of bureaucrats around election time (which exist in Brasil and other countries) displace cycles rather than eliminate them, and may even exacerbate them. These cycles, I argue, are best understood through the conceptual apparatus of the clientelism literature, while recognizing that public jobs may play at the same time a clientelistic and a programmatic role, depending on how the job is assigned and how the labor force is used. Finally, I argue that to understand the links between political and bureaucratic turnover we need to examine dynamics from the day of the election, and not only from the day the new administration takes office. Building on previous research, on the Brazilian institutional environment, and on in-depth interviews with municipal politicians and bureaucrats, I deduce a number of hypotheses, which I test using micro-level, administrative data on the universe of municipal employees in Brazil between 2002 and 2016.

The initial results presented here are generally consistent with my hypotheses. During the pre-electoral months ahead of the personnel spending freeze, governments hire more than they normally do in a non-electoral year, as they do once the freeze is removed at the beginning of the following year. These results suggest that the fiscal constraints faced by politicians around election time produce a wider cycle, rather than producing more stable hiring patterns. Dismissals on the other hand increase heavily between election time and the end of the year, a pattern that is arguably due to outgoing administrations' desire to maximize compliance with fiscal rules before leave office. I then show that these hiring and firing cycles are not homogeneous in the educational qualifications of workers. Those hired ahead of the spending freeze and those fired after the election tend to be less educated than those hired and fired in the same months of a non-electoral year. This suggests that political bureaucratic cycles may be targeted to poor voters. Although more analyses are needed on how hires and dismissals are targeted, an examination of bureaucratic outputs reinforces the idea that the patterns uncovered respond to clientelistic strategies. During the months around elections, pre-natal checkups are significantly worse (and increasingly so as months pass by) than in

the same months in non-electoral years. I find no robust evidence of cycles in a healthcare outcome (child deaths), which may be due to outcomes being inelastic to variations in bureaucratic inputs, and/or to effects happening with a longer lag. Finally, using a close-races regression discontinuity design I show that incumbents who lose the election increase both the dismissal of temporary workers and the hiring of tenured ones. This is consistent with outgoing administrations trying to comply with fiscal limits before leaving office (and thus before losing control of the accounts), while trying to reduce the opponent's discretionality in the bureaucracy. These results also confirm that incoming administrations increase hiring, a finding that is not original but takes here another significance, once put in the framework of a bureaucratic contraction right before political turnover is made effective. Relatedly, I show that an electoral defeat of the incumbent causes (arguably as a result of the dismissals under the outgoing administrations) a significant decrease in prenatal checkups in the months immediately before and after a new mayor taking office. In general, while I have not proved a causal connection between bureaucratic inputs (dismissals) and outputs (less prenatal checkups), the evidence suggests that political bureaucratic cycles worsen bureaucratic performance and can hurt development outcomes.

The paper makes a number of contributions, both theoretical and empirical. First, the paper advances our understanding of the temporal dynamics of PBCs, partly thanks to the use of administrative, micro-level data. Second, the paper questions the common assumption that legal and fiscal limits aimed at constraining PBCs are effective and welfare-enhancing, by showing that in fact, under the strict limits of Brazilian legislation, cycles persist and may simply be wider in time than they would be without those limits. Third, by linking data on bureaucratic inputs (jobs) to bureaucratic outputs (results of how that labor force is employed), the paper advances our understanding of how politicians' resource allocations and policy outcomes are linked. While other papers have examined cycles in policy outputs (e.g. Baskaran et al. (2015)), this is to my knowledge the first paper examining both bureaucratic inputs and outputs in the same empirical setting. Fourth, by using identified, contract-level data instead of the macro-level yearly or quarterly data commonly found in the literature, the paper opens a whole set of theoretical and empirical questions related to characterizing the individuals who are hired and dismissed. This characterization, which I have only started here by examining the education level of those who are hired and fired around elections, allows us to better understand the strategies used by politicians, and in particular how they relate to clientelistic and programmatic rationales. By doing so, I hope to strengthen the dialogue between the literatures on PBCs and clientelism, two fields that I believe have a lot to gain from cross-fertilization. Finally, by exploiting a close-races quasi-experimental design, the paper goes

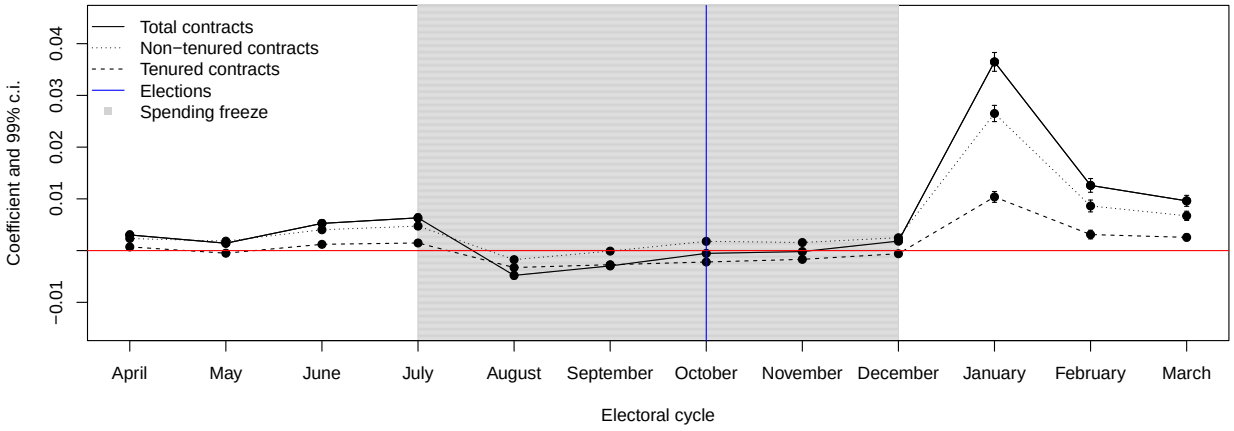
beyond the common strategy of exploring heterogeneity in PBCs without identifying causality, and expands our understanding of how political turnover affects political bureaucratic cycles, especially in the last few months of the outgoing administration.

This is the first version of an ongoing project, and as such has a number of important limitations. Some of the issues that are central to my theory (such as the targeting of jobs) have been explored only superficially and will have to be addressed more systematically in the future. Examples include assessing how political bureaucratic cycles relate to workers' partisanship; exploring a wider set of healthcare outputs and outcomes (and doing so by leveraging the more refined micro-level data); causally identifying the effect of incumbent partisanship, gender, and alignment with state governments on political bureaucratic cycles; and causally identifying the effect of special fiscal limits on PBCs, and potentially exploiting that identification to study the effects of PBCs on electoral outcomes and bureaucratic outputs. A lot is still to be understood about the implementation of the LRF limits, and the roles that the federal Ministry of Finance, state prosecutors, state audit courts, and the local political opposition play in that process. Ongoing in-depth interviews with actors in the field, which were key for developing the hypotheses tested here, are likely to provide invaluable insight into issues of enforcement as well as on the mechanisms behind the political bureaucratic cycles uncovered here.

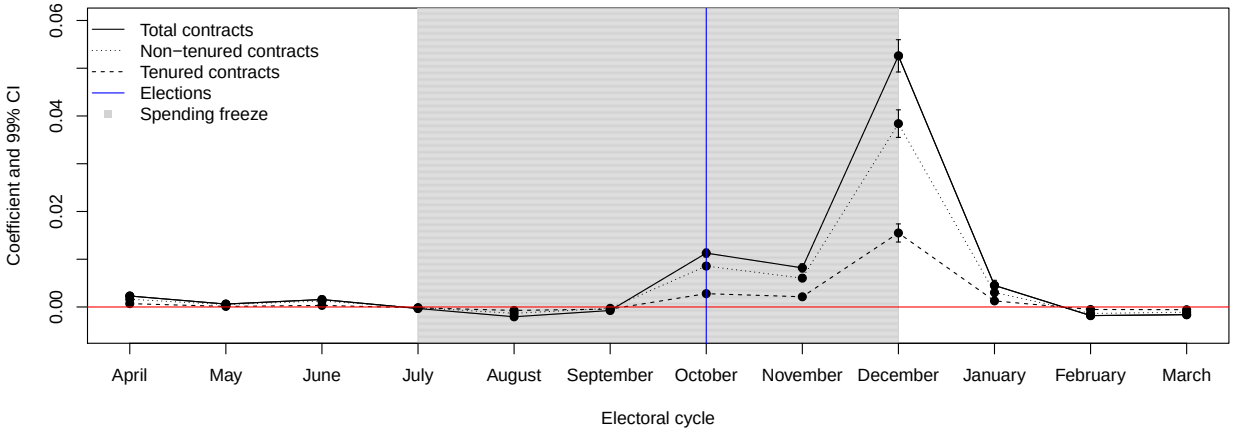
Appendices

Additional results identifying political bureaucratic cycles

Figure 8: Monthly election cycle effects on logged relative number of municipal contracts (as a share of stock in that same month)



(a) Flow of new contracts (relative to stock)



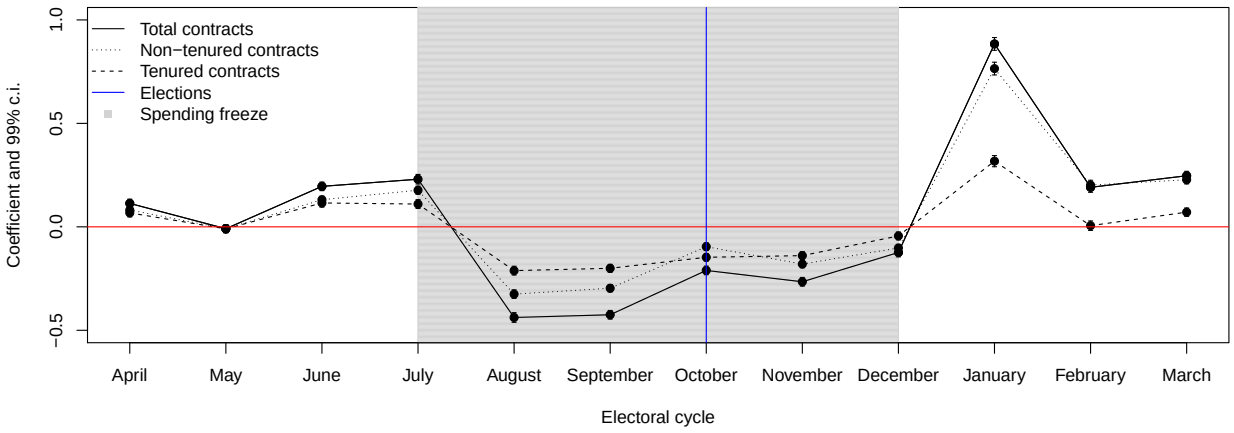
(b) Flow of dismissals (relative to stock)

Table 2: Political bureaucratic cycles in absolute number of contracts, with monthly effects

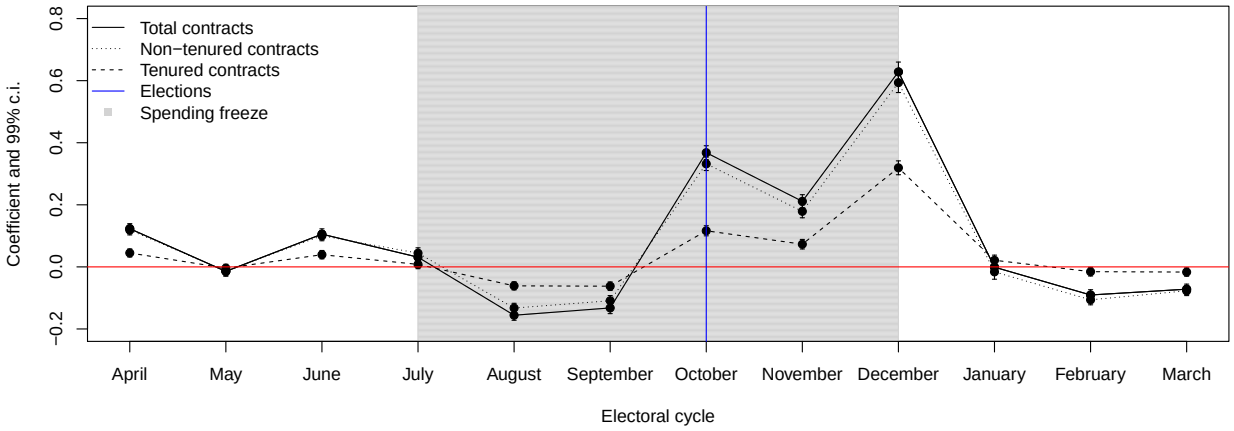
	Dependent variable: logged number of municipal employees, by contract type								
	Hires			Fires			Stock		
	Total	Temp.	Tenured	Total	Temp.	Tenured	Total	Temp.	Tenured
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
April EY	0.102***	0.082***	0.045***	0.117***	0.114***	0.040***	0.0004	−0.016***	0.006***
	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.001)	(0.001)	(0.001)
May EY	−0.015	0.001	−0.037***	−0.017*	−0.015*	−0.009	0.0001	−0.020***	0.007***
	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.001)	(0.002)	(0.001)
June EY	0.189***	0.141***	0.083***	0.103***	0.101***	0.033***	0.003***	−0.020***	0.009***
	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)	(0.005)	(0.001)	(0.002)	(0.001)
July EY	0.218***	0.184***	0.076***	0.027***	0.042***	0.004	0.008***	−0.012***	0.013***
	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)	(0.005)	(0.001)	(0.002)	(0.001)
Aug. EY	−0.428***	−0.300***	−0.239***	−0.155***	−0.129***	−0.065***	0.003***	−0.024***	0.011***
	(0.009)	(0.008)	(0.008)	(0.006)	(0.006)	(0.005)	(0.001)	(0.002)	(0.001)
Sep. EY	−0.419***	−0.271***	−0.238***	−0.130***	−0.103***	−0.070***	−0.001	−0.034***	0.009***
	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)	(0.001)	(0.002)	(0.001)
Oct. EY	−0.204***	−0.067***	−0.189***	0.367***	0.336***	0.107***	−0.014***	−0.073***	0.004***
	(0.008)	(0.007)	(0.007)	(0.009)	(0.008)	(0.006)	(0.001)	(0.002)	(0.001)
Nov. EY	−0.269***	−0.157***	−0.187***	0.214***	0.187***	0.060***	−0.021***	−0.100***	0.001
	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)	(0.006)	(0.001)	(0.003)	(0.001)
Dec. EY	−0.134***	−0.078***	−0.105***	0.646***	0.605***	0.334***	−0.071***	−0.484***	−0.021***
	(0.008)	(0.007)	(0.007)	(0.012)	(0.012)	(0.009)	(0.002)	(0.008)	(0.002)
Jan. post-EY	0.888***	0.759***	0.335***	−0.002	−0.018	0.024**	−0.036***	−0.115***	−0.015***
	(0.012)	(0.012)	(0.010)	(0.010)	(0.009)	(0.006)	(0.001)	(0.004)	(0.002)
Feb. post-EY	0.200***	0.181***	0.061***	−0.093***	−0.112***	−0.010	−0.016***	−0.079***	−0.003*
	(0.009)	(0.009)	(0.009)	(0.007)	(0.006)	(0.005)	(0.001)	(0.003)	(0.001)
Mar. post-EY	0.243***	0.203***	0.108***	−0.071***	−0.078***	−0.011	−0.005***	−0.031***	0.00000
	(0.008)	(0.008)	(0.008)	(0.006)	(0.006)	(0.005)	(0.001)	(0.002)	(0.001)
Observations	878,711	878,711	878,711	878,711	878,711	878,711	878,711	878,711	878,711
R^2	0.677	0.703	0.618	0.704	0.702	0.685	0.996	0.987	0.999

Standard errors clustered at the municipality level; *p<0.01; **p<0.001; ***p<0.0001
All models include a lagged dependent variable, month fixed effects, and municipality × year fixed effects

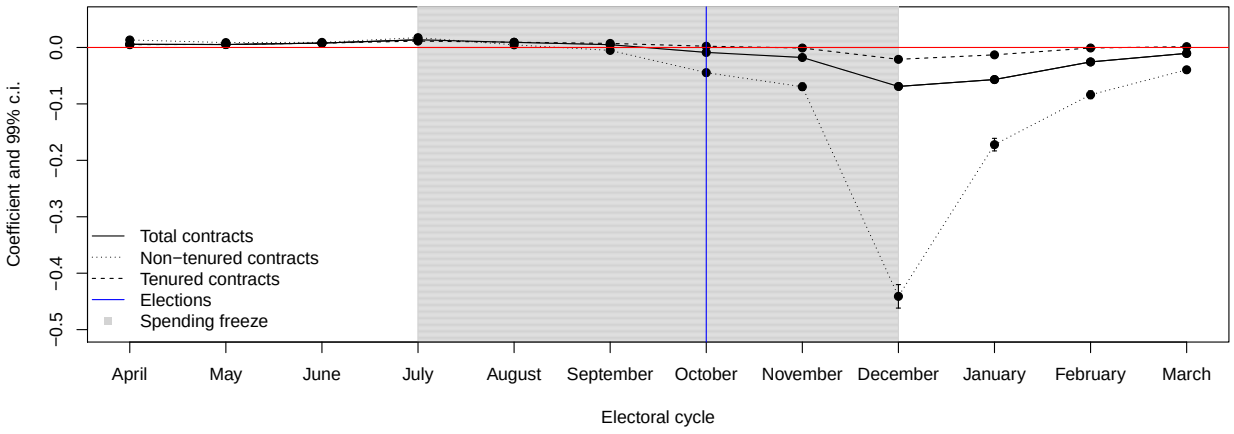
Figure 9: Monthly election cycle effects on logged absolute number of municipal workers (using unique personal identifiers)



(a) Flow of new people hired

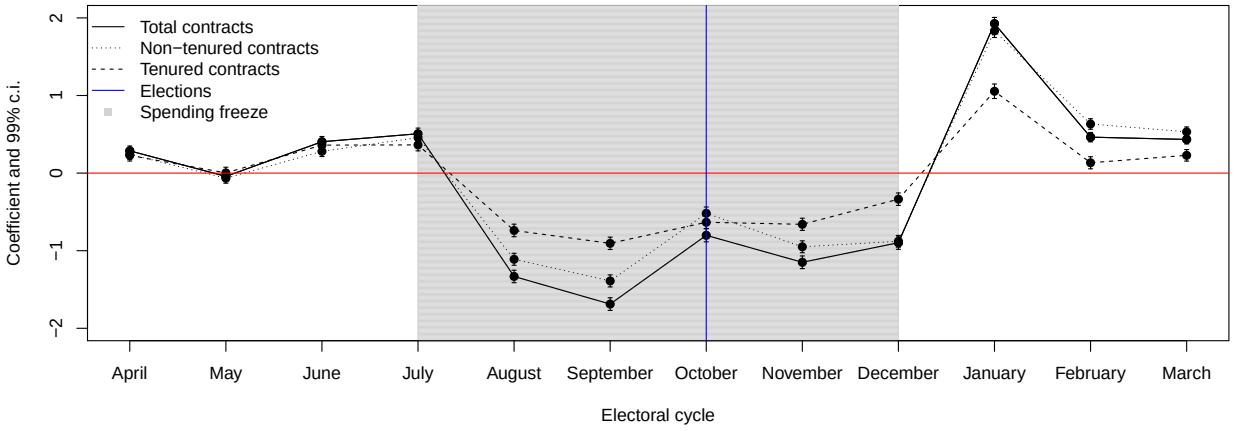


(b) Flow of people dismissed

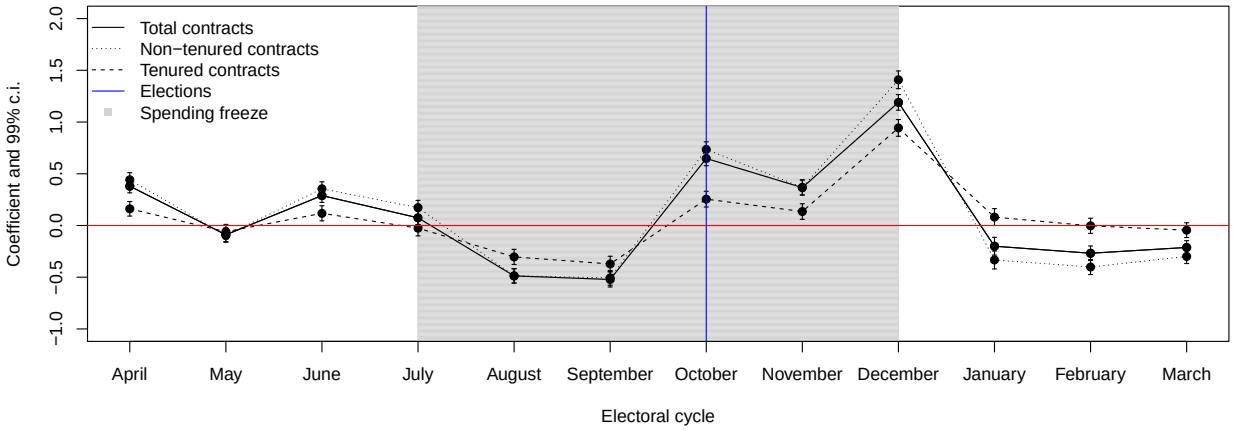


(c) Stock of people hired

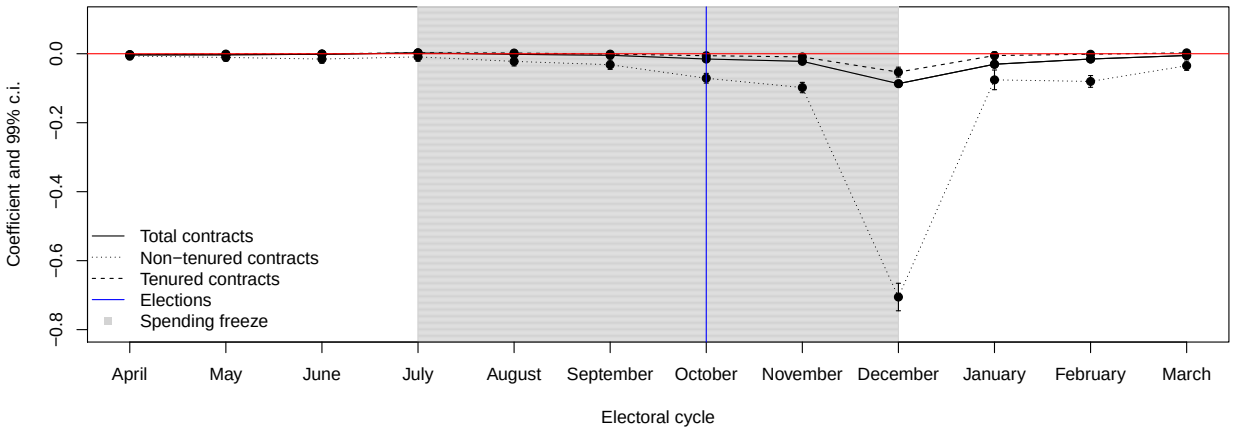
Figure 10: Monthly election cycle effects on logged absolute number of Brazilian reais corresponding to the sum of municipal workers' average salary



(a) Expenditures on flow of new contracts

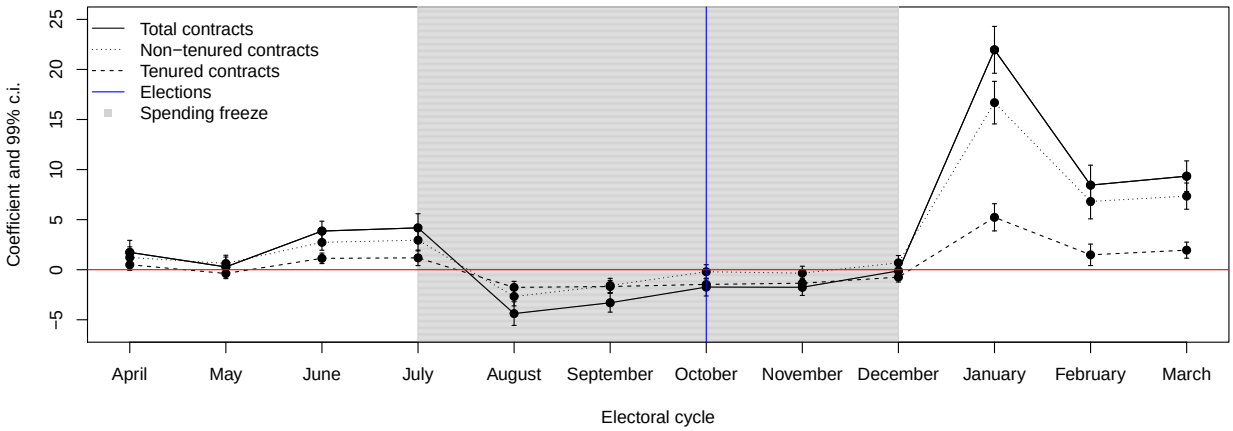


(b) Savings on flow of dismissals

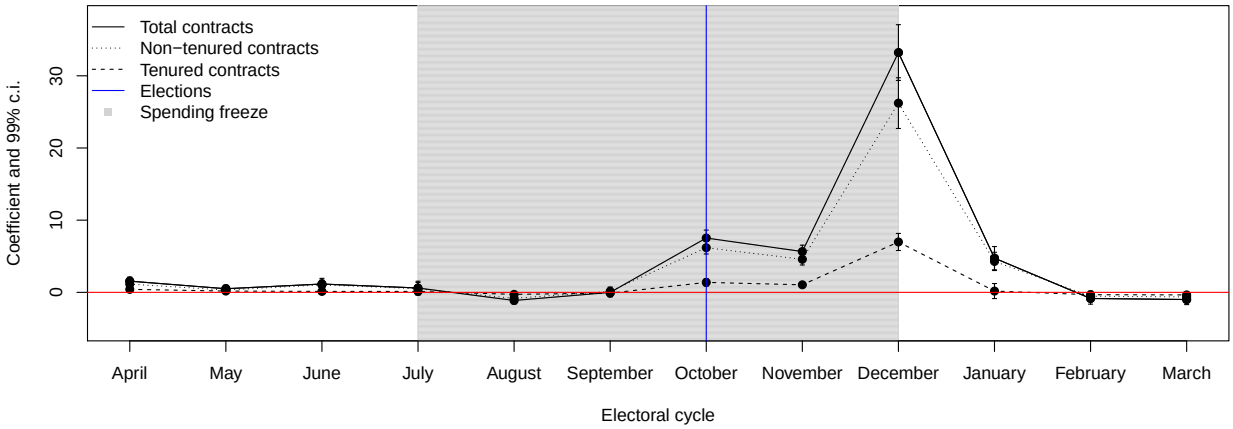


(c) Expenditures on stock of contracts

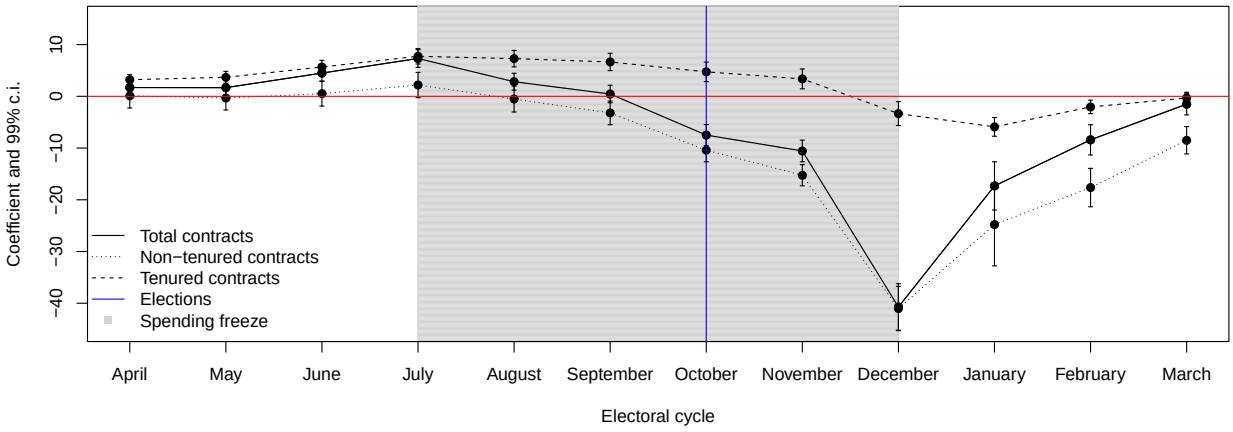
Figure 11: Monthly election cycle effects on absolute number of contracts (no logging)



(a) Flow of new contracts

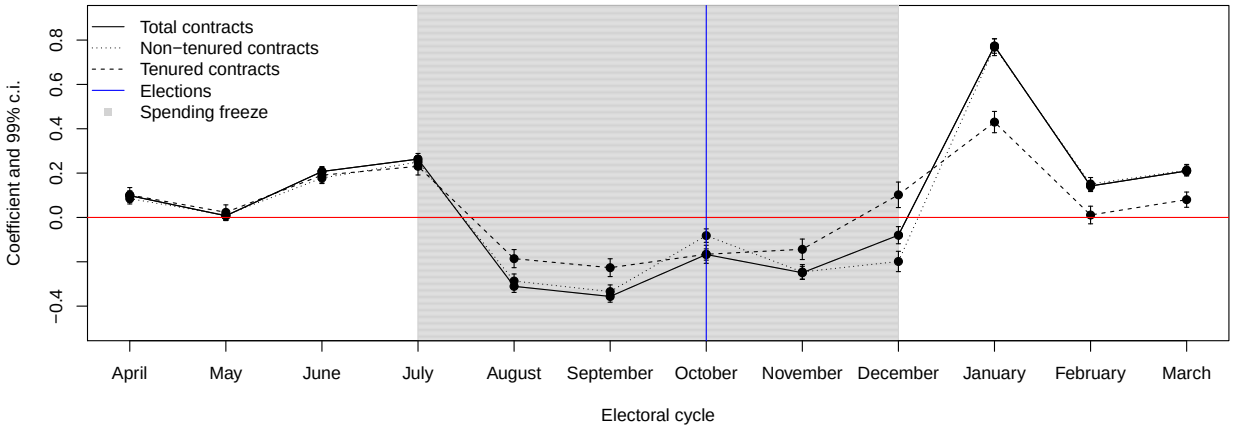


(b) Flow of dismissals

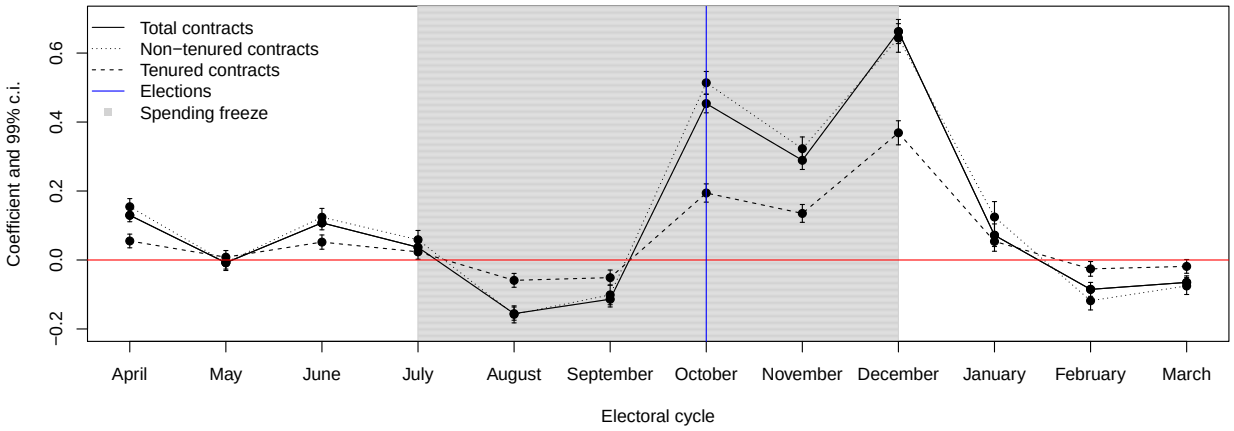


(c) Stock of contracts

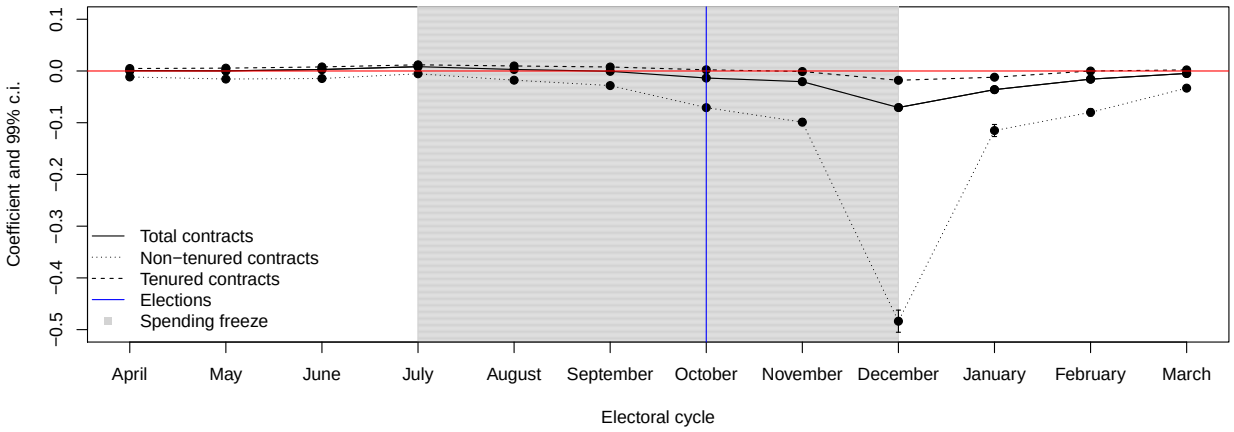
Figure 12: Monthly election cycle effects on logged absolute number of contracts (without adding 1, omitting observations with 0 in the dependent variable or its lag)



(a) Flow of new contracts

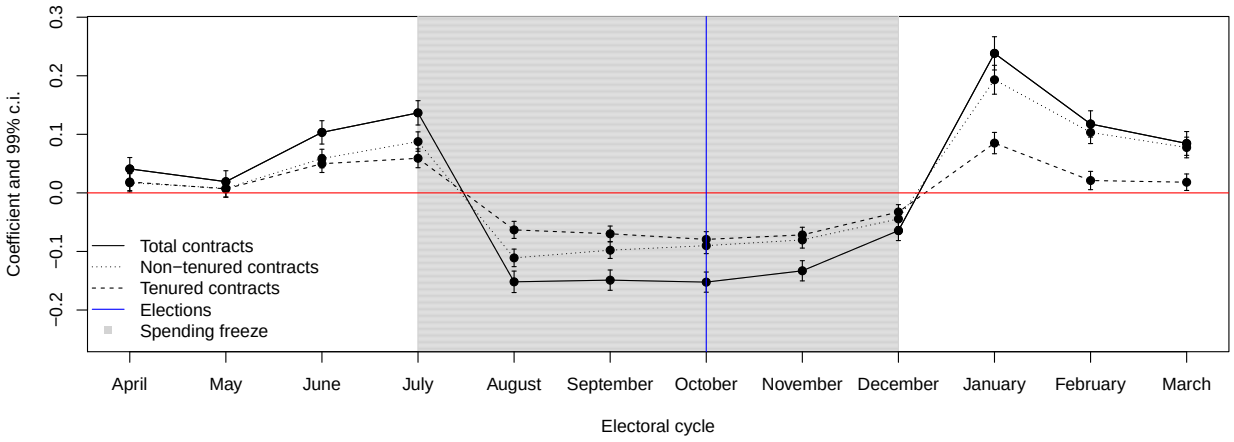


(b) Flow of dismissals

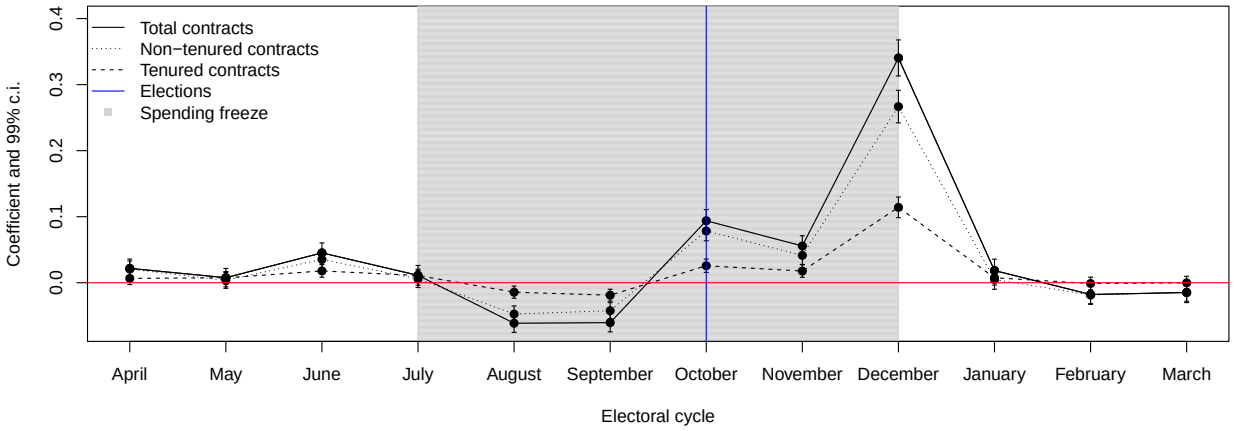


(c) Stock of contracts

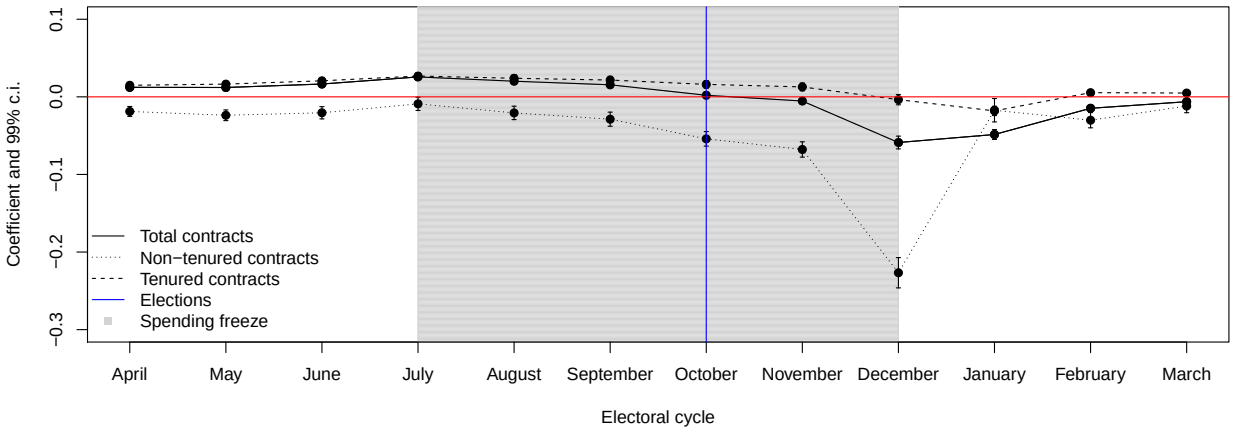
Figure 13: Monthly election cycle effects on logged absolute number of contracts, healthcare professionals



(a) Flow of new contracts

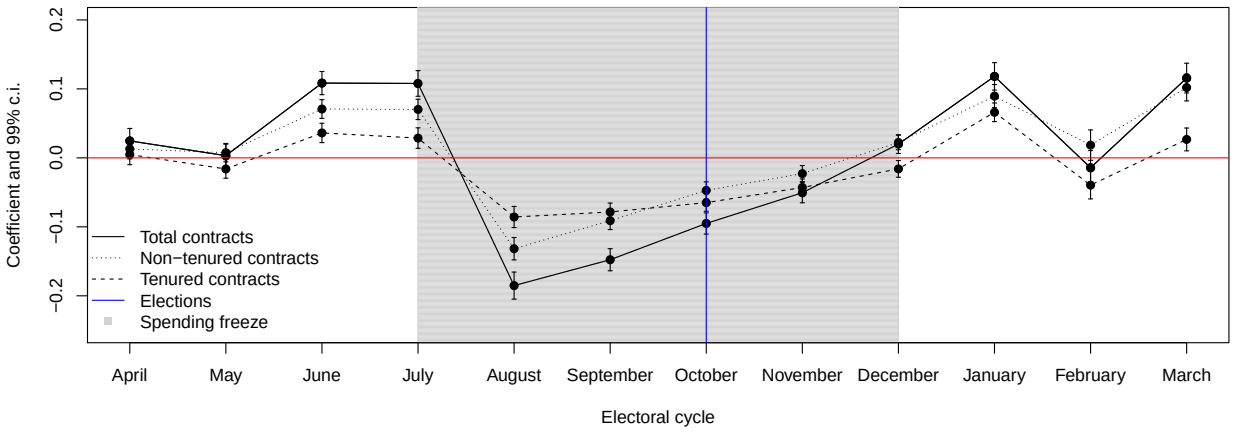


(b) Flow of dismissals

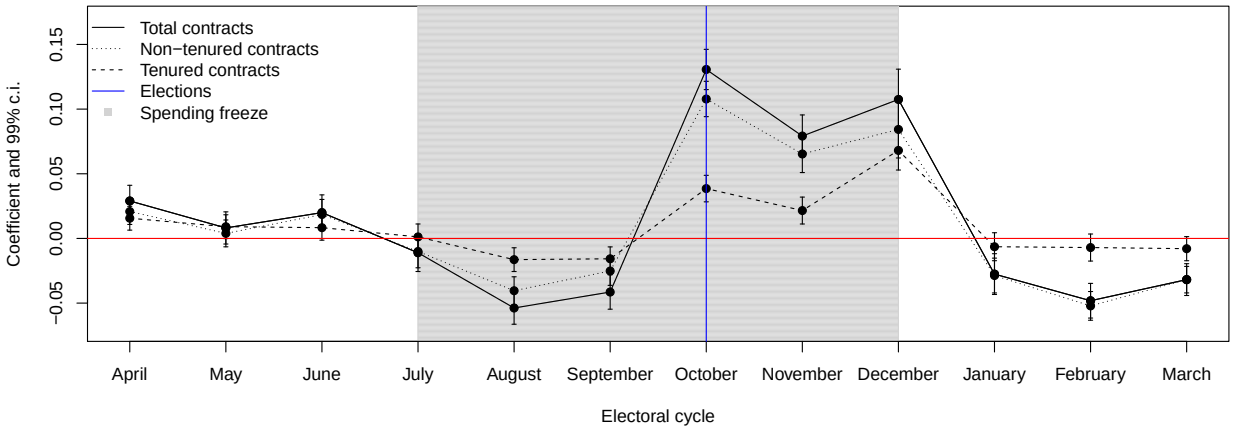


(c) Stock of contracts

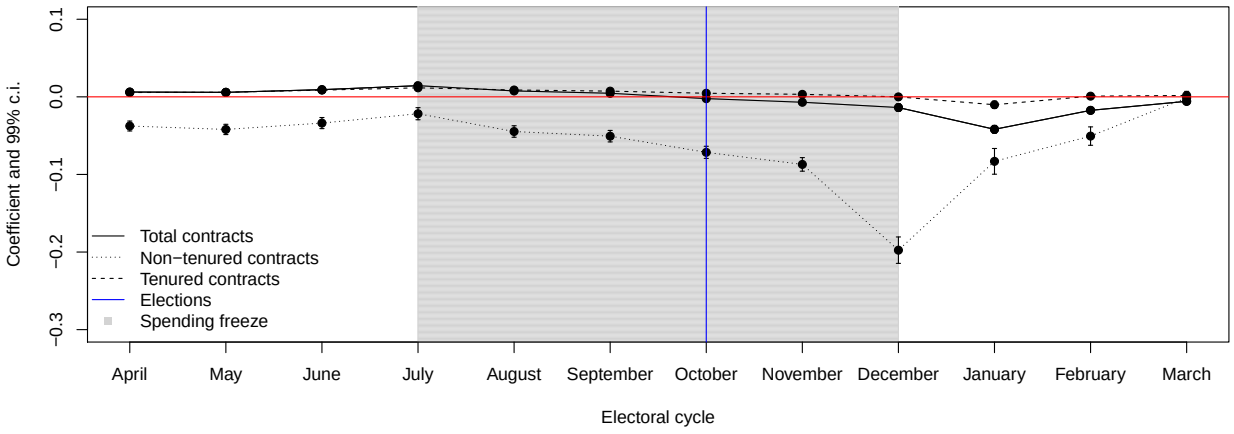
Figure 14: Monthly election cycle effects on logged absolute number of contracts, education professionals



(a) Flow of new contracts



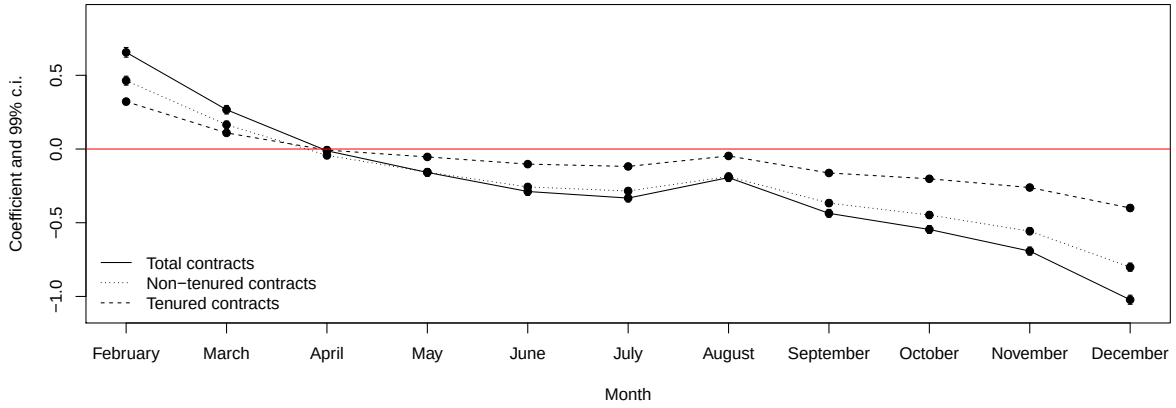
(b) Flow of dismissals



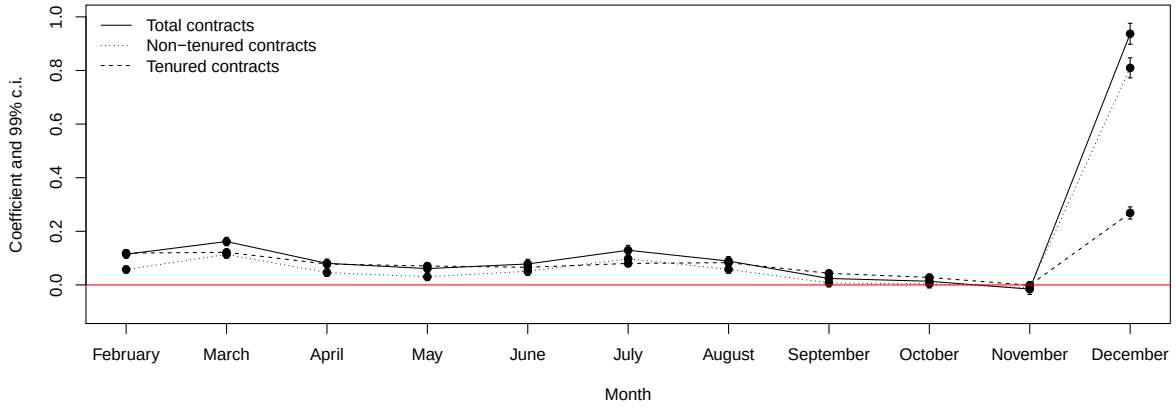
(c) Stock of contracts

Monthly seasonality in municipal employment (month fixed effects)

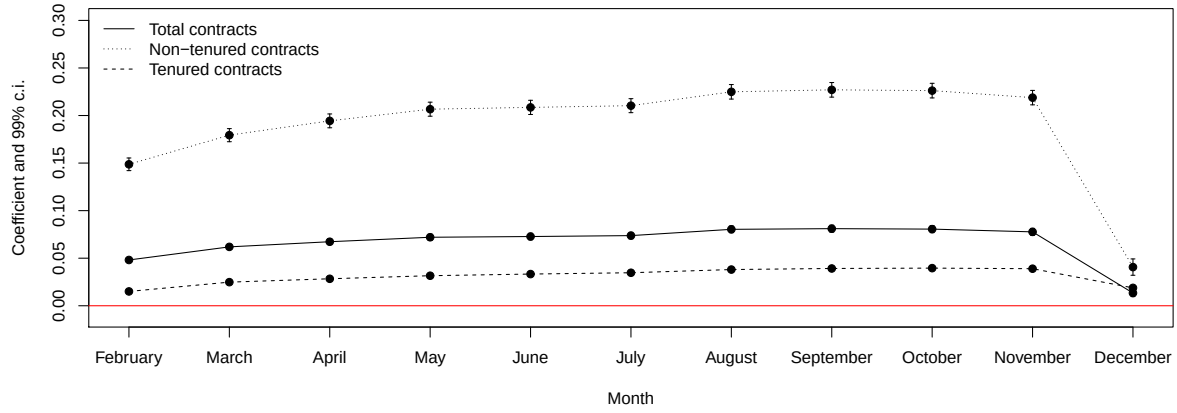
Figure 15: Month fixed effects on logged absolute number of municipal contracts



(a) Flow of new contracts

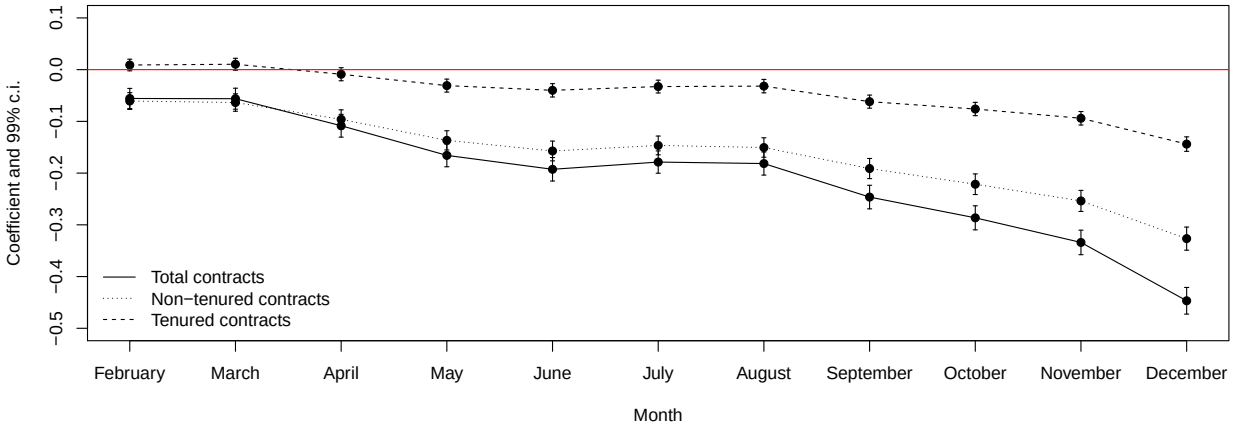


(b) Flow of dismissals

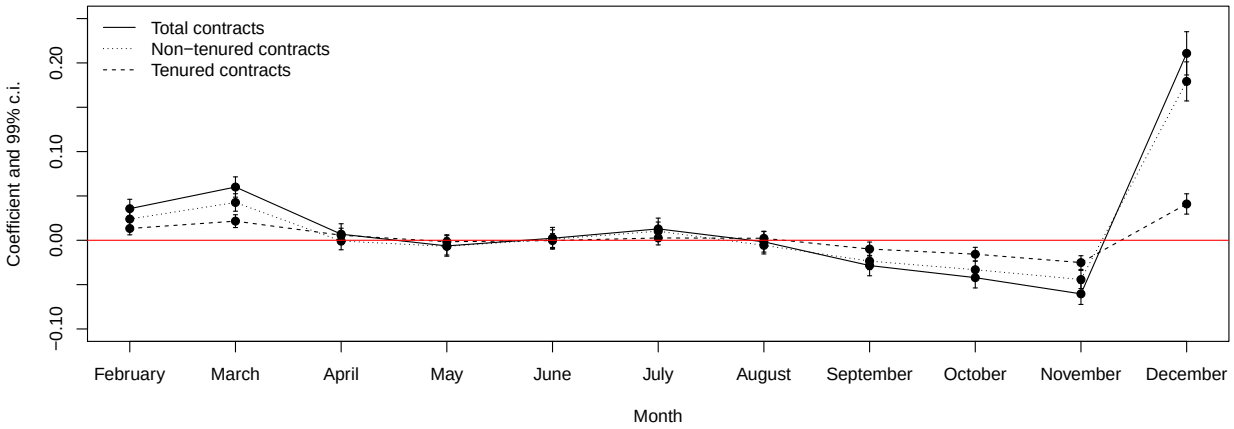


(c) Stock of contracts

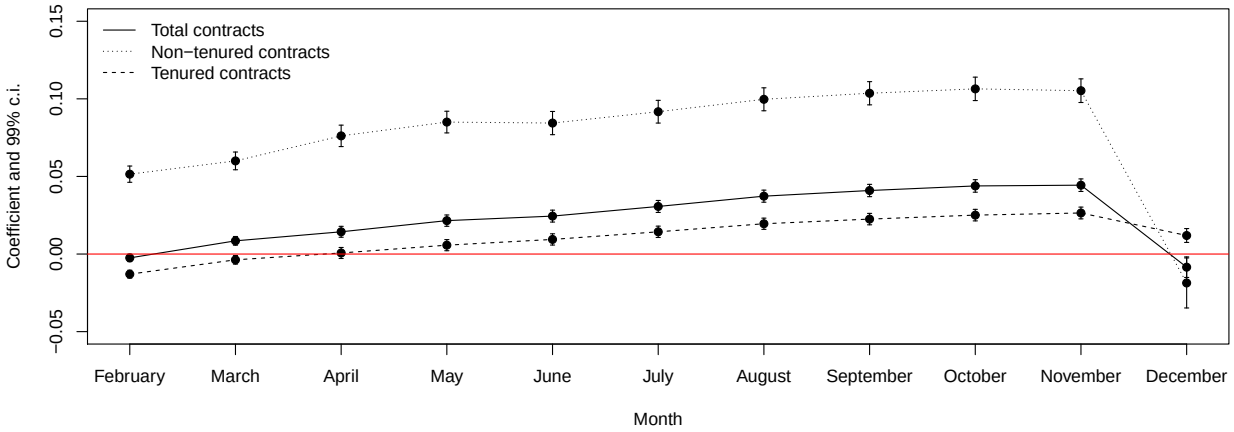
Figure 16: Month fixed effects on logged absolute number of municipal contracts, healthcare professionals



(a) Flow of new contracts

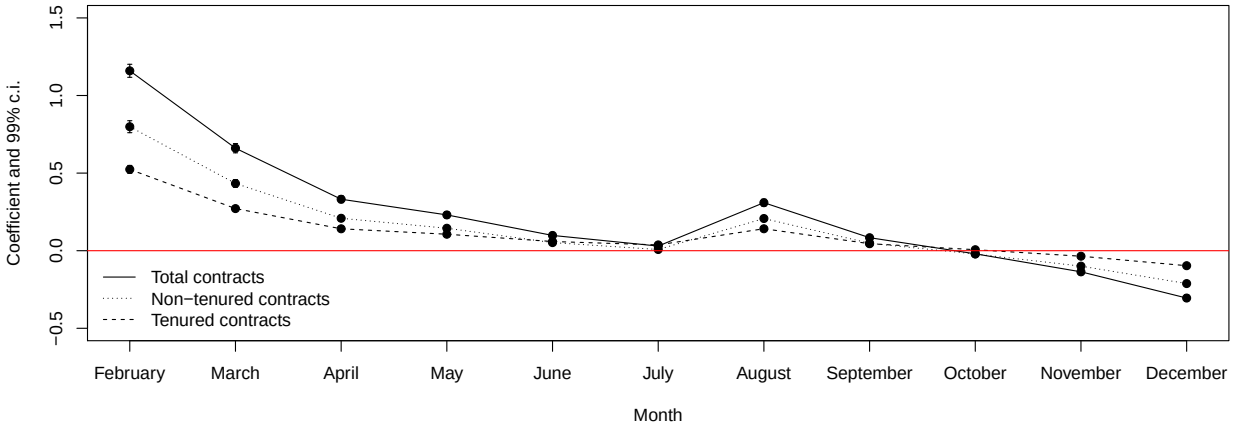


(b) Flow of dismissals

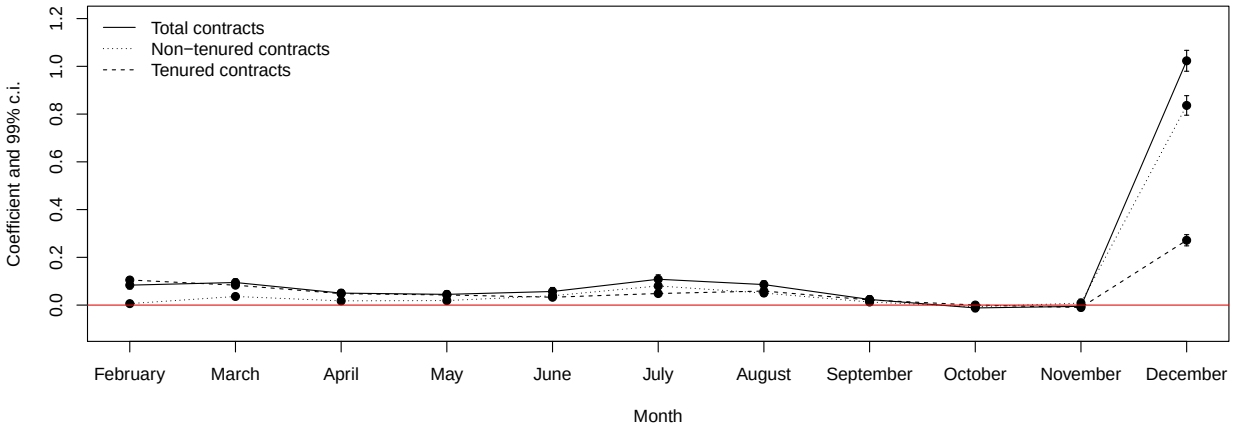


(c) Stock of contracts

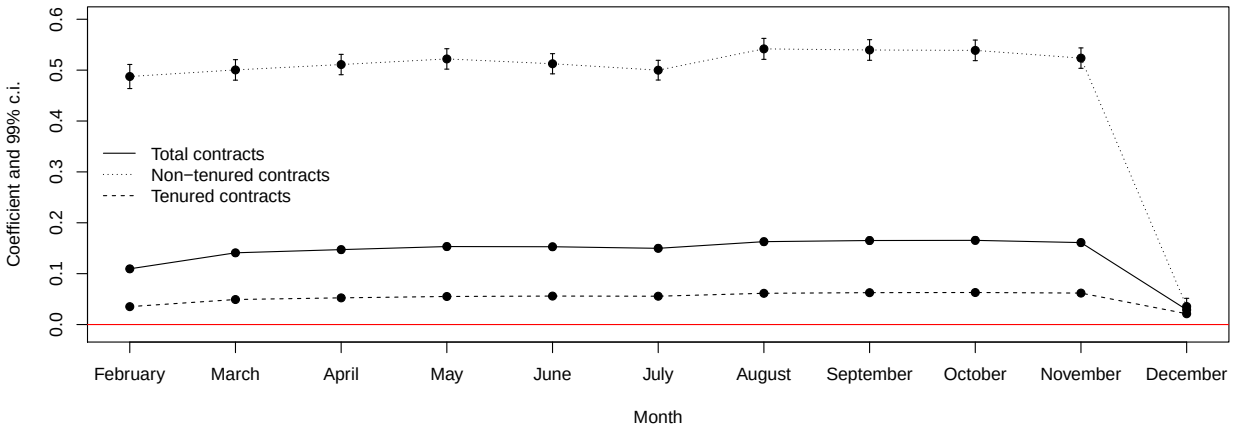
Figure 17: Month fixed effects on logged absolute number of municipal contracts, education professionals



(a) Flow of new contracts

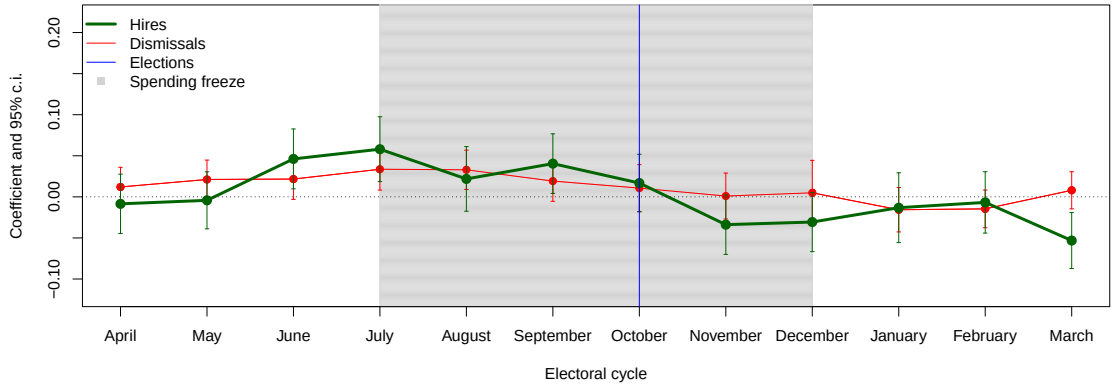


(b) Flow of dismissals

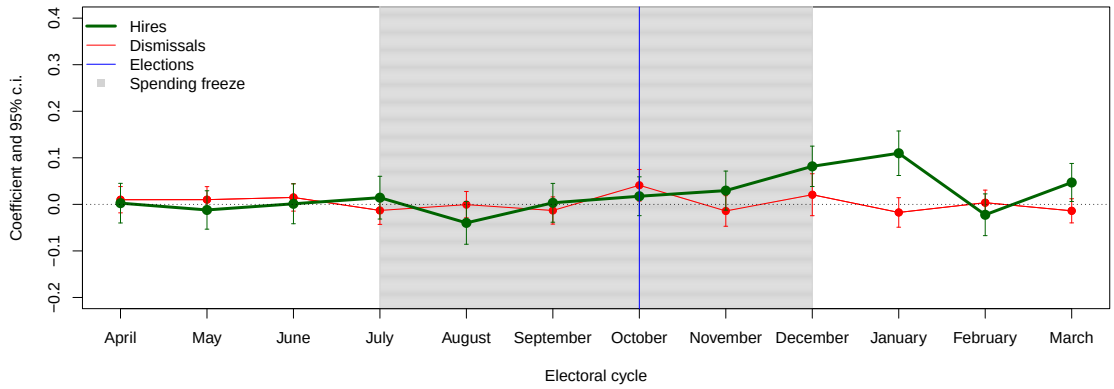


(c) Stock of contracts

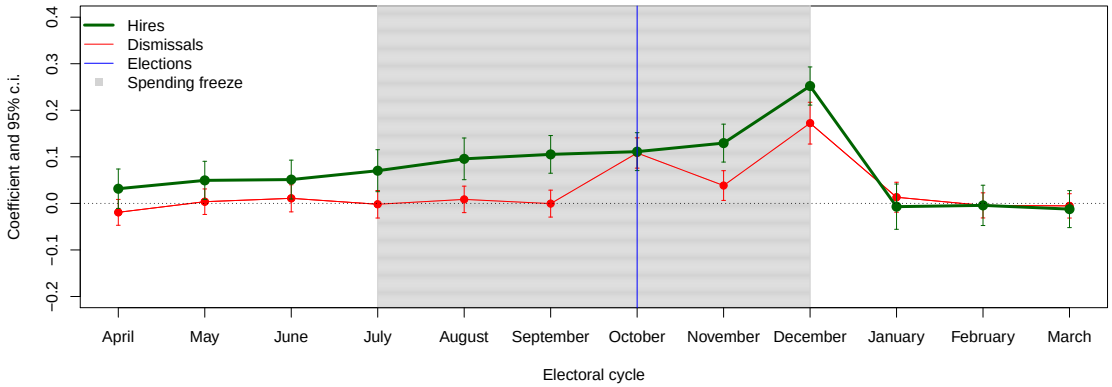
Figure 18: Conditional monthly election cycle effects on logged absolute number of tenured, municipal employees



(a) Monthly election-cycle effects conditional on high electoral competition



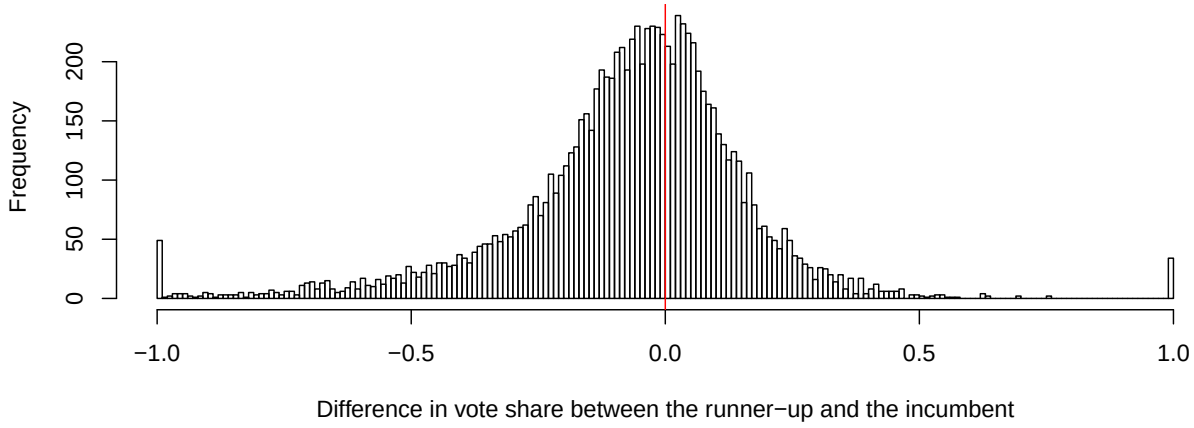
(b) Monthly election-cycle effects conditional on the incumbent mayor being on their first term



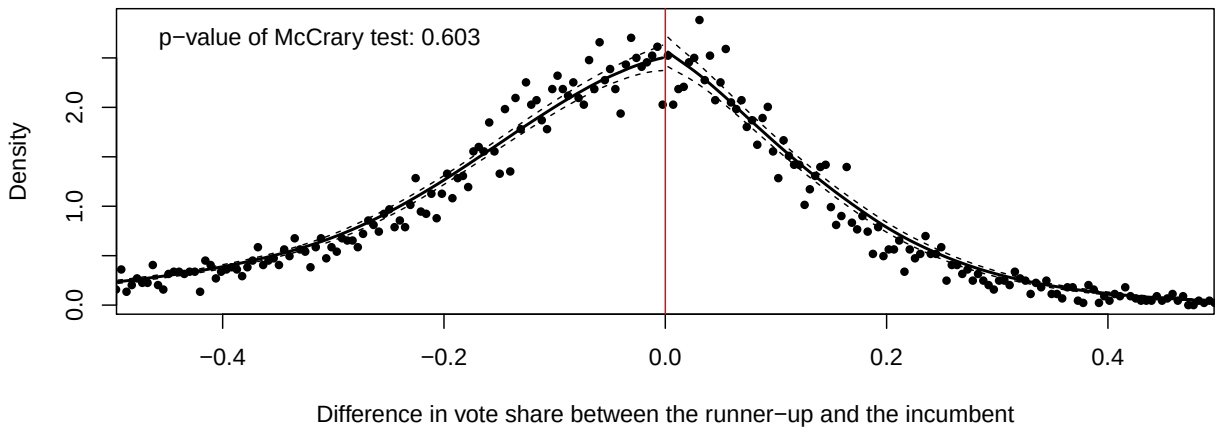
(c) Monthly election-cycle effects conditional on the mayor losing the election in October

Robustness checks for the election results regression discontinuity

Figure 19: Histogram and density of the forcing variable, with results of the McCrary density test (municipal elections of 2004, 2008, 2012 and 2016)

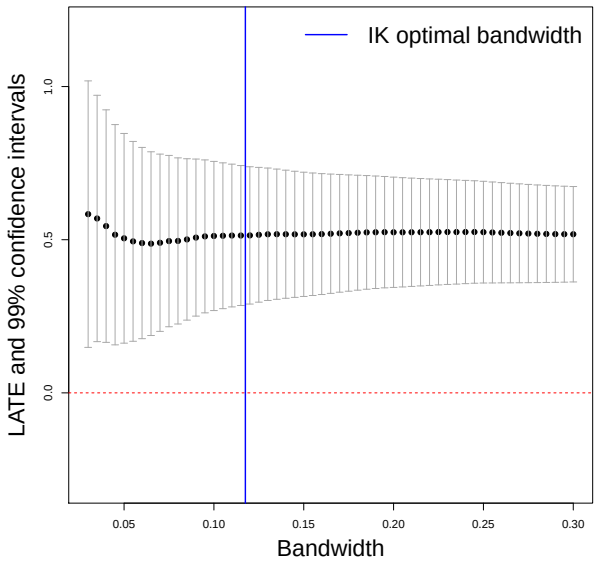


(a) Histogram of the forcing variable

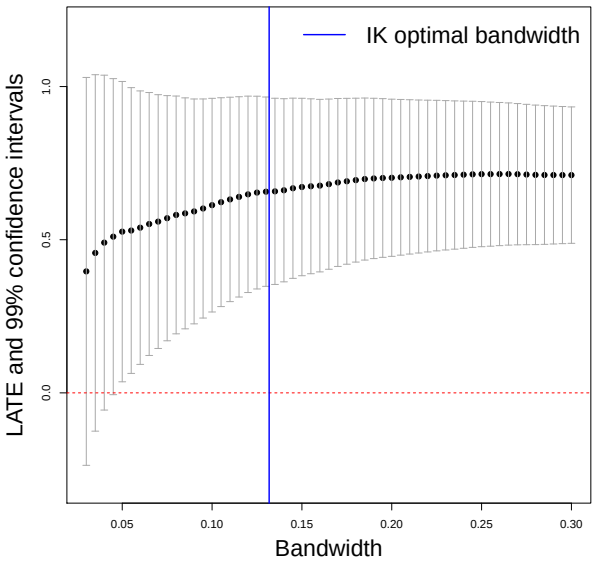


(b) Density of the forcing variable with formal test of discontinuity around the threshold

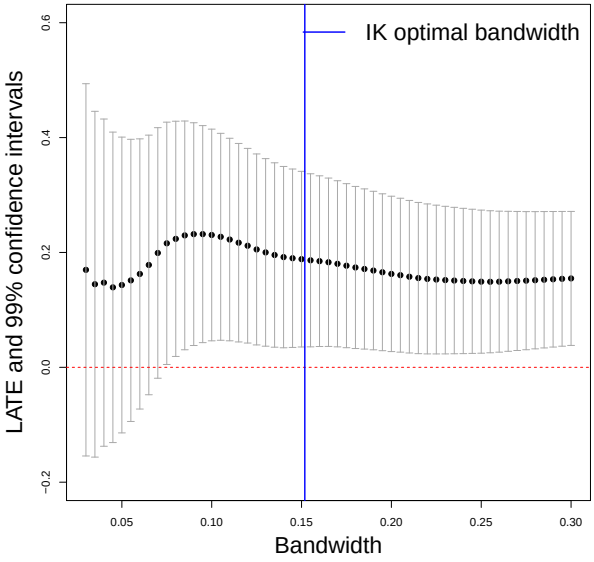
Figure 20: Robustness of the main regression discontinuity models to alternative bandwidths: effect of an incumbent electoral defeat on logged number of contracts



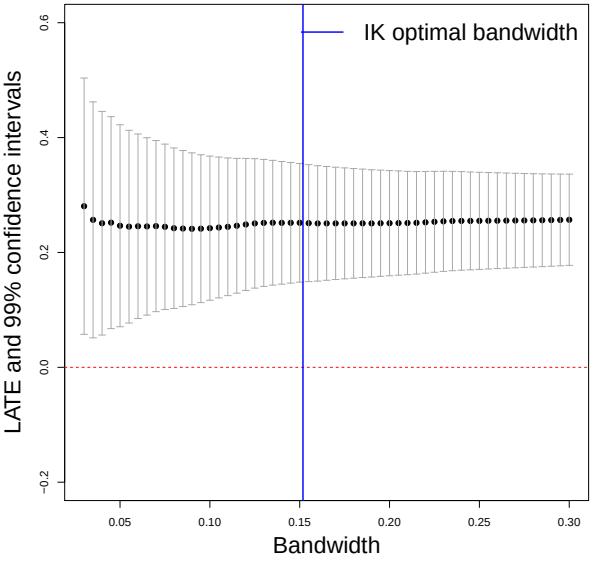
(a) Dismissals of temporary contracts in October



(b) New temporary hires in January

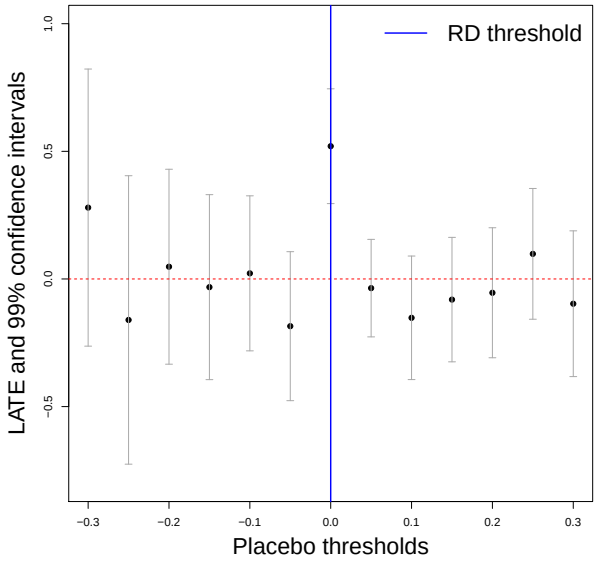


(c) Dismissals of tenured contracts in October

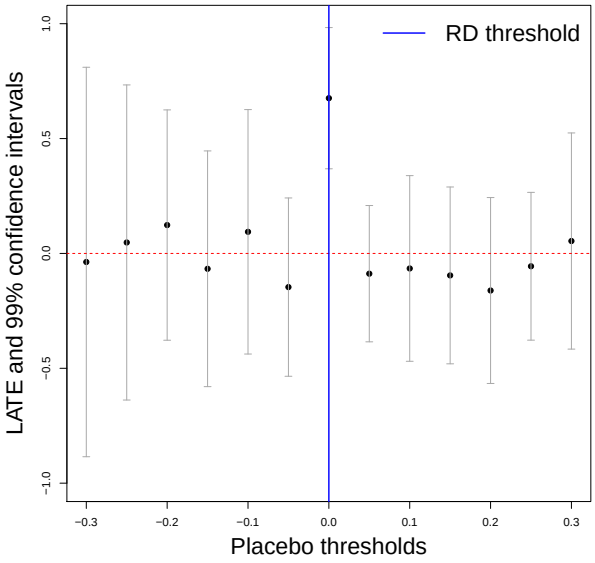


(d) New tenured hires in December

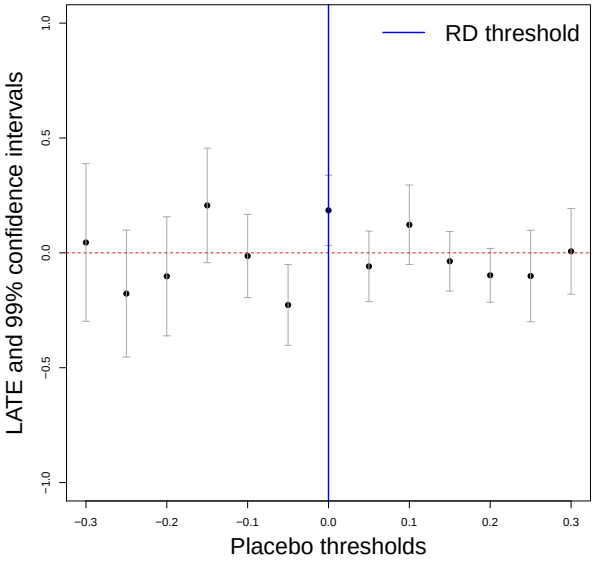
Figure 21: Placebo tests of the main regression discontinuity results, for the effect of an incumbent electoral defeat on logged number of contracts, moving the RD threshold away from 0



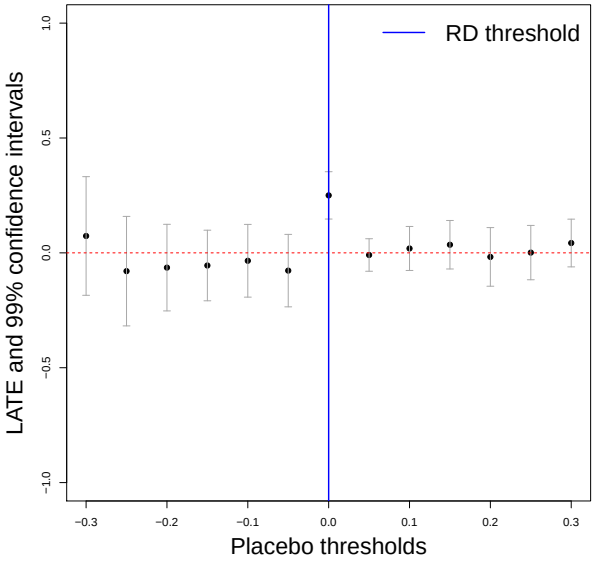
(a) Dismissals of temporary contracts in October



(b) New temporary hires in January

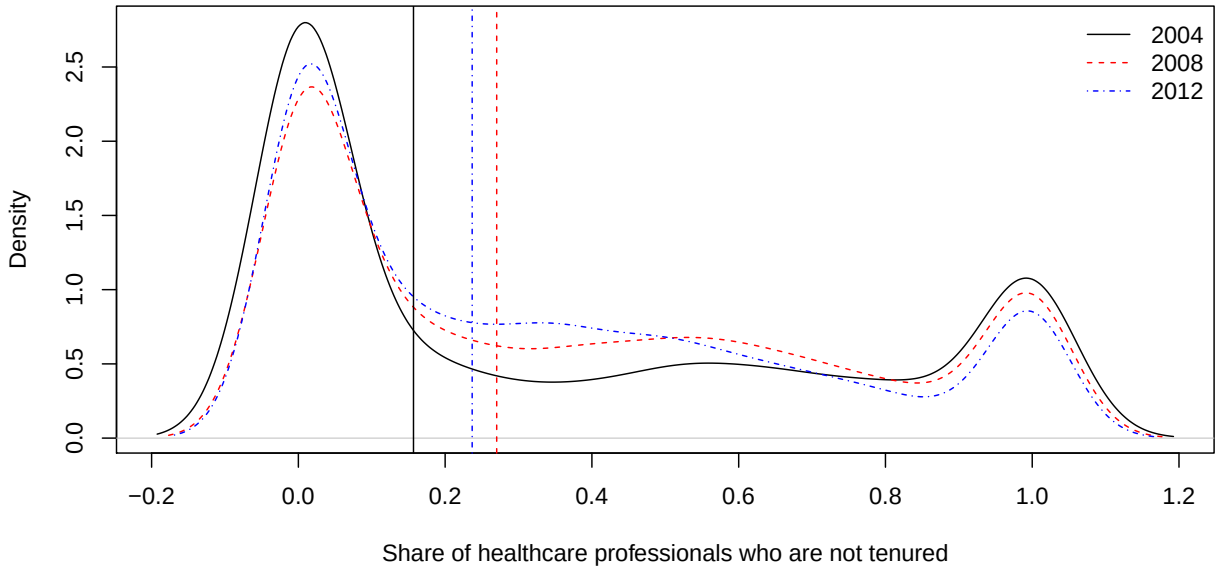


(c) Dismissals of tenured contracts in October



(d) New tenured hires in December

Figure 22: Density of the distribution of the share of healthcare professionals who are not tenured, by election cycle (vertical lines indicate the median of each distribution)



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