

Political bureaucratic cycles: Public employment and service delivery around elections in Brazil*

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Abstract

A vast literature on political cycles has shown that politicians often manipulate policy tools ahead of elections, as a strategy to improve their electoral performance. Much less is known about the effects of policies used to contain these cycles. I argue that legal constraints on politicians' discretion over inputs such as spending or hiring ahead of elections simply displace cycles and can even exacerbate them. I demonstrate these unintended consequences using large, monthly panels of Brazilian municipalities, and measuring cycles in bureaucratic inputs (hires) and outputs (services). Brazilian laws ban hiring and firing bureaucrats in a 6-month period around elections, which take place at the same time throughout the country. Hiring contracts during this freeze period but it expands in the months before the ban, for both temporary and civil service bureaucrats. These patterns are even more pronounced in localities exposed to a randomized anti-corruption audit. Healthcare services also follow cyclical patterns: some services expand ahead of the elections (at the expense of others), and in general services contract immediately before and after the election. Together, these results suggest that constraints on politicians' discretion over inputs around elections displace rather than eliminate electoral cycles, and can have other unintended consequences.

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Introduction

For decades, political scientists and economists have studied how politicians' manipulation of policy around elections leads to cycles in economic outcomes or government policy.¹ The basic insight of this literature is that, in order to increase their chances of re-election, politicians change policy right before the election, in what are often seen as economically suboptimal policy choices that un-smooth government spending and economic activity ([Nordhaus, 1975](#)). This results in what are called political business cycles (related to economic output) or political budget cycles (related to government policy tools like spending).² [Franzese \(2002\)](#) and [Dubois \(2016\)](#) review this extensive literature.³

While this established research agenda has accumulated a significant body of formal models and empirical results showing that politicians often manipulate policy ahead of elections –at least when they have both the incentives and the ability to do so ([Alt and Rose, 2009](#))–, much less is known about the effectiveness of policies designed to contain these cycles. Politicians are often constrained by laws that seek to contain electoral cycles in policy tools like spending, debt, or hiring. For example, Colombia's electoral law forbids most procurement with the private sector in the 4 months leading to the election, and Peru's fiscal prudence law establishes stricter limits on spending and deficit for the first semester of an electoral year. Do these limits successfully constrain electoral cycles in policy tools? What impacts do they have in policy outputs?⁴

I address these questions through a study of *political bureaucratic cycles*, namely cycles in public employment and in the activities government workers perform. Focusing on the bureaucracy has three main advantages. First, public employment is one of the largest spending categories

¹Since 2000, dozens of studies on electoral cycles have been published in just the top journals of these two disciplines: 35 articles in political science (APSR, AJPS, and JOP) and 9 in economics (AER, QJE, JPE, and Econometrica).

²I focus on cycles caused (at least partly) by politicians' actions around elections, but there are also electoral cycles that are not driven by government actions ([Block and Vaaler, 2004](#)), and political cycles that are not driven by elections ([Guo, 2009](#)).

³A related, more recent literature examines increases in government activity ahead of elections, often seen as a sign of government responsiveness ([Lueders, 2021](#); [Dipoppa and Grossman, 2020](#); [Christensen and Ejdemyr, 2020](#)).

⁴Studies of political budget cycles in the American states ([Rose, 2006](#)) and across countries ([Gootjes et al., 2021](#)) have shown that fiscal rules dampen increases in pre-electoral spending. These studies however examine general rules that apply year after year, as opposed to rules specific to the electoral calendar, on which I focus here.

for governments around the world – in the average country, the government payroll makes up a quarter of government spending ([International Monetary Fund, 2016](#)). Second, the political use of public employment is ubiquitous ([Grindle, 2012](#)), and politicians can mobilize hiring towards a variety of political objectives, including mobilizing voters, rewarding supporters, and expanding public services ahead of elections ([Toral, 2020](#)). These two features imply that electoral cycles in public employment are likely to have important effects on both the fiscal and political realms. Constraining political bureaucratic cycles may thus be particularly important for protecting fiscal prudence and fairness in electoral competition between incumbent and opposition parties. Third, focusing on the bureaucracy we can simultaneously examine cycles in inputs (e.g., hiring) and outputs (e.g., public services).

Empirically, I study political bureaucratic cycles in Brazilian municipalities, leveraging detailed administrative data on public employment (between 2000 and 2019, a 20-year period covering 5 elections) and on healthcare services (between 2004 and 2014, a 13-year period covering 3 elections). Brazil, similarly to other middle-income countries, prohibits hiring and firing employees in a 6-month period around elections. This policy seeks to protect fiscal discipline and candidates' equality of opportunity. I exploit the exogenous timing of elections (which take place every 4 years on the first Sunday of October) and the 6-month “freeze period” during which politicians to identify political bureaucratic cycles. By using large, balanced panels of between half to one million municipality-month observations, with month fixed effects (to control for seasonality) and municipality \times year fixed effects (to finely control for local conditions) I am able to measure how public employment and service delivery fluctuate around elections.

The results show that hiring contracts during the freeze period, in agreement with the legal constraints on hiring around elections. In turn, however, hiring expands both before and after the freeze period. This is consistent with politicians responding strategically to both electoral incentives and legal constraints, and simply anticipating election-related hiring rather than refraining from it. Hiring follows similar cyclical patterns for low-skill, professional, and managerial positions. While cyclical patterns are more pronounced for temporary hiring, civil service hiring also follows marked cycles around elections. This calls into question the common assumption of civil service hiring as completely insulated from political influence, and draws attention to the relevance of politicians' discretion over the timing of civil service hires.

Heterogeneity analyses show that cycles in hiring have intensified over time (as rules became stricter and enforcement strengthened), and that municipalities randomly exposed to federal anti-

corruption audits (which increase the salience and improve the enforcement of rules) further boost cycles in employment. Together, these results suggest that, rather than eliminating cycles, legal constraints on policy tools displace and even exacerbate them.

Cycles in hiring also have a correlate in public service delivery. Healthcare services (such as medical consultations or household visits by community health agents) expand before the election, consistent with politicians using service delivery as a signaling device towards voters. Some services, however, contract during that same period, and all of them tend to contract starting one month before the election. These patterns suggest that there are important trade-offs in election-driven government responsiveness.

This paper makes three main contributions to the literature on electoral cycles. First, it advances our understanding of the promises and pitfalls of policies designed to contain them by constraining politicians' discretion around elections – a topic that previous studies have generally overlooked. By showing that these constraints displace cycles rather than eliminating them, this paper draws attention to politicians' strategic responses to anti-cyclical policies and the unintended consequences of constraints. Second, by leveraging month-level panels and uncovering detailed patterns of both expansion and contraction in outcomes during the electoral calendar, this paper draws attention to the month-to-month temporal dynamics of electoral cycles, which year-level analyses tend to obscure.⁵ Third and last, by leveraging randomly assigned anti-corruption audits, this paper identifies the causal effect of context on cycles, thus improving on previous studies of heterogeneity in cycles which tend to rely on splitting the sample or interacting time periods with endogenous covariates ([De Haan and Klomp, 2013](#)).⁶

Theory

I argue that legal constraints limiting politicians' discretion over public employment around elections displace rather than eliminate political bureaucratic cycles. I see these cycles as resulting from the strategic behavior of politicians who are election-motivated and who have their choices constrained

⁵[Labonne \(2016\)](#) uses both yearly and quarterly data to show that the more aggregated data can hide important fluctuations around elections.

⁶In a separate paper, I use a regression discontinuity design to examine another key source of heterogeneity in political bureaucratic cycles: whether the incumbent wins or loses their re-election bid ([Toral, 2022](#)).

(significantly but not completely) by legal and fiscal limits.

Politicians' incentives to expand public employment ahead of elections are likely to be particularly high in low- and middle-income contexts. In less developed contexts, where there are fewer job opportunities in the private labor market, the value of public jobs as a political currency increases. Second, governments in less developed contexts have less state capacity to distribute other benefits (such as human development services or infrastructure works) that may be valued by citizens but require more planning, capacity, and coordination with other actors such as higher levels of government or private firms. In these settings, therefore, the political value of public employment is higher. Government jobs combine two features that make them particularly useful tools ahead of elections. First, they are targetable and (when temporary) reversible ([Robinson and Verdier, 2013](#)), which facilitates solving the double credibility issue of clientelistic exchanges ([Stokes et al., 2013](#)). On the other hand, expanding public employment entails not only distributing income to beneficiaries but also expanding the government's labor force, and thus its ability to distribute public goods and services in a more programmatic manner.

For cycles in public employment to occur, politicians must have some discretion over hiring. Discretion is lower, but often not eliminated, in civil service hiring – while politicians may not be able to choose who gets the job, they often retain discretion over how many civil servants are hired and when. This goes counter the common idea of civil service hiring as completely insulated from political influence.

Given the political value of public employment and politicians' discretion over hiring and firing, some countries have established strict rules limiting their leeway in this area of public policy ([Table 1](#)).⁷ These constraints typically establish a time window around elections during which politicians' are not allowed to hire, dismiss, and/or transfer bureaucrats. The rationale of these laws is often to protect fiscal discipline or to protect the equality of opportunity of candidates running for office. This mirrors other constraints that countries have established over other policy tools such as government spending, public debt, or deficits.

I argue that these temporal limits on politicians' discretion over policy tools around elections displace rather than eliminate the cycles they seek to contain. At least in contexts where electoral incentives are sufficiently strong, politicians will respond to these constraints by anticipating

⁷In other cases, pre-election constraints on hiring do not make it into law. For example, the Electoral Commission of Pakistan placed a temporal ban on hiring ahead of the 2018 elections. Without the protection of a legal instrument, this ban was challenged in court and later lifted.

Table 1: Legal constraints on hiring around elections

Country	Limit on hiring around elections	Period	Legal instrument
Brazil	No hiring, firing, or transfers	Last 6 months of mandate	Law 9504 (1997)
Colombia	No hiring or firing	4 months before the election	Law 996 (2005)
Philippines	No hiring, promotion, or salary increases	45 days before the election	Election Code (1985)
Uruguay	No hiring of civil servants	Last 12 months of mandate	Law 16127 (1990)

the expansion in hiring. Moreover, I argue that by limiting human resources management these constraints can lead to unintended consequences in terms of bureaucratic outputs.

Previous empirical studies of political cycles in public employment have found mixed results. Some studies have found an expansion of public employment before the election in settings as diverse as Indonesian municipalities ([Pierskalla and Sacks, 2019](#)), US states ([Cahan, 2019](#)), Greek municipalities ([Chortareas et al., 2017](#)), Philippine municipalities ([Labonne, 2016](#)) and Finnish and Swedish municipalities ([Dahlberg and Mörk, 2011](#)). On the other hand, [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. This apparent contradiction is arguably driven by the fact that both Brazil and Colombia impose constraints on hiring around elections. In these settings, cycles are likely to be characterized by a sequence of expansion and then contraction of hiring in the pre-election period.

I focus on political bureaucratic cycles in both inputs (hires) as well as outputs (public services). I argue that the constraints on public employment around elections can depress public service delivery, by limiting senior officials' ability to manage human resources at a time when delivering services may be particularly important for incumbents. At the same time, expansion in some services (plausibly those that are more visible to and/or valued by voters) may go hand in hand to a contraction in other services provided by the same bureaucrats.

The idea that policies that impose temporal constraints on politicians' discretion over policy tools can have unintended consequences builds on an insight from the early formal literature on electoral cycles. [Rogoff](#) noted that "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" ([1990, 22](#)).⁸ Rogoff goes on to note (and argue formally) that "in practice, an

⁸A similar idea was put forward by [Tufte \(1978, 149\)](#).

incumbent has a wide array of fiscal actions with which he can 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).

Institutional setting

Brazilian local governments are an ideal setting to examine political bureaucratic cycles. In this setting, elections are held on a fixed schedule, bureaucracies are relatively large and responsible for delivering major public services (including education, healthcare, and social assistance), politicians have some discretion over public employment, and multiple laws constrain their use of such discretion around elections.

Brazilian municipalities hold elections every four years on the first Sunday of October.⁹ State and federal elections are held every four years on a separate calendar, two years before and after municipal elections. Mayors are elected through a majoritarian system,¹⁰ and city councilors are elected through a proportional, open-list system. Mayors can run for reelection only once.¹¹ Local elections are generally competitive – in the 2016 elections, almost half (about 49%) of the incumbents who ran were defeated. There are currently 5,570 municipalities,¹² most of which are small and poor.¹³

Municipal governments have a relatively large workforce, because they are responsible for providing primary services in healthcare, education, and social assistance. On average, municipal governments hired in 2016 4.9% of the local population and 38.2% of those employed in the formal

⁹The rule was established in 1997, and has applied to all elections between 2000 and 2019. 2020 elections were postponed to mid-November because of the COVID-19 pandemic.

¹⁰Municipalities with over 200,000 inhabitants (fewer than 2% in 2016) hold a runoff election on the last Sunday of October if no candidate obtains an absolute majority.

¹¹This rule was also established in 1997.

¹²In the period I study, the number of municipal governments ranges from 5,507 in 2000 to 5,569 in 2019. Brasília, the capital city, is a federal district that holds state rather than municipal elections.

¹³According to the 2010 census, the median municipality had fewer than 12,000 inhabitants and a per capita income of less than 500 Brazilian reais (about USD284 at the exchange rate at the time). According to administrative data described in the next section, the median municipality had 446 employees in 2010.

labor market. Municipal employees enjoy a wage premium relative to the private sector ([Colonnelli et al., 2019](#), 3090), similarly to other developing contexts ([Finan et al., 2017](#)). Mayors and the secretaries they appoint have some discretion over the hiring and firing of bureaucrats in all policy areas. Such discretion differs significantly between the civil service and other hiring modes with fewer employment protections.

Civil servants make up about two thirds of the municipal labor force. The federal constitution requires all permanent staffing needs to be filled with civil service contracts. Candidates with the best performance on competitive examinations are eligible for a position, which has tenure for life after a probationary period.¹⁴ Critically, however, the best performers are not automatically appointed. While politicians have no discretion over the ranking of candidates, they can decide the timing and number of civil service hires.

About a third of municipal employees are hired on temporary contracts,¹⁵ which can legally be used to hire political appointees or to fill short-term or urgent staffing needs. Temporary employees generally have 1-year contracts that can be terminated by the employer much more easily than civil service contracts. In practice, temporary contracts are sometimes used where civil service contracts should prevail. Still, this practice is unconstitutional and politicians may be prosecuted for it.

Of all areas of local policy, healthcare is typically the most salient one for voters at the local level ([Boas et al., 2019](#), 395; [Reis, 2016](#)). Municipalities provide free primary healthcare services to all residents under the umbrella of the federal Unified Health System. To do so, they maintain clinics staffed with doctors, nurses, and other healthcare professionals. Municipalities also hire community health agents (CHAs) to assist with the provision of basic healthcare services, especially preventive care and particularly in rural areas ([Ministério da Saúde, 2012d](#)). More complex services, like specialist consultations and hospitalizations, are generally provided by state governments, especially for residents of small municipalities. Private healthcare provision is common in larger municipalities, but most citizens rely exclusively on the public system ([Castro et al., 2019](#), 5).

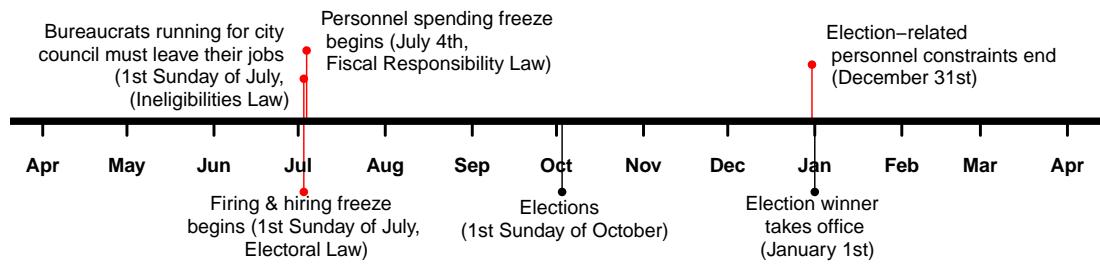
Brazilian laws limit the hiring and firing of bureaucrats around elections, creating a “freeze period” starting 3 months before the election and lasting until the end of the mayor’s mandate

¹⁴Once tenured, civil servants can be dismissed only in extraordinary circumstances such as being convicted of corruption.

¹⁵I use the term temporary contracts to refer to all non-civil-service contracts. These contracts use a variety of labor regimes, all without tenure.

(Figure 1).¹⁶ The 1997 Electoral Law forbids hiring, firing, or transferring bureaucrats 3 months before and after the election.¹⁷ These limits aim at protecting the equality of opportunity of candidates running for office. Additionally, the 2000 Fiscal Responsibility Law mandates that personnel expenses do not increase during the 180 days before the end of a government's mandate, i.e., roughly three months before and after the election. This provision has the goal of limiting the fiscal impact of governments' electioneering through public employment. Finally, the 1990 Ineligibilities Law mandates that no public employee can run for office, requiring candidates to take paid leave (if they are tenured) or abandon their job (if non-tenured) 3 months before the election.¹⁸

Figure 1: Timeline of election cycles in Brazil



These laws establish tough penalties for violation of the rules constraining incumbents around elections, sometimes referring to the penalties in other pieces of legislation.¹⁹ For example, mayors who do not comply with the constraints on hiring around elections are subject to penalties of 1-4 years of prison, losing their post, and being disqualified for election for 5 years. Mayors and other local officials are held accountable by multiple instances, including regular courts, the state audit court, and the state prosecutor's office. Ample anecdotal evidence suggests that these laws are enforced.²⁰

¹⁶See Appendix A for additional details on legal constraints.

¹⁷The law allows for hiring, firing or transferring of positions of trust, and the hiring of civil servants who had previously passed competitive exams. The Electoral Law also forbids salary adjustments exceeding inflation during the whole year of the elections.

¹⁸For some positions, they are required to exit 6 months before the election.

¹⁹Including a decree from 1967, the penal code, the 1992 administrative dishonesty law, and the 2002 fiscal crimes law.

²⁰For example, the former mayor of Chupinguaia in the northern state of Rondônia was condemned in July of 2017 to 18 months of prison for increasing personnel expenses during the last 180 days of his mandate. The former mayor of Marília in the southeastern state of São Paulo, was recently condemned also for increasing personnel expenses towards the end of his mandate. As a result, his political rights were suspended for 8 years, he was stripped of any public jobs he may have held, he was forbidden from contracting with public governments for 5 years, and he was imposed a fine equivalent to twice the financial loss to

One of the institutions overseeing municipal governments is Brazil's federal comptroller's office (CGU, *Controladoria-Geral da União*). CGU has long targeted its audits through randomized lotteries. Randomly selected municipalities are visited by a team of CGU auditors, who review how the local government has spent federal transfers. After the visit is concluded, CGU releases the results of the audits to the media and to other accountability actors like the federal prosecutor's office, the state audit court, the federal police, and the municipal legislative chamber. These randomized audits have been found to decrease corruption and increase the chances that mayors will be prosecuted for corruption charges ([Avis et al., 2018](#)).

Research design

To identify political bureaucratic cycles in Brazilian municipalities, I exploit the exogenous timing of local elections, which are held every four years on the first Sunday of October. By examining long panels of municipality-month data, covering multiple election cycles, and using thousands of fixed effects to finely control for municipality-specific seasonality, I estimate how bureaucratic inputs and outputs vary in the months surrounding elections, compared to those same months in non-election years.

In baseline specifications I use linear regression on a balanced panel of municipality-month observations, as per the following estimating equation.

$$Y_{iym} = \alpha_{iy} + \theta_m + \sum_{p=-6}^5 \beta^p D_{iym}^p + \gamma Y_{iym-1} + \varepsilon_{iym} \quad (1)$$

Y_{iym} is a given outcome (for example, the number of hires or the number of medical consultations) corresponding to municipality i in year y in month m . Since outcomes are right-skewed count variables,²¹ I use the log on both the dependent variable and its lag, after adding 1 to keep observations with zeroes. α_{iy} are municipality \times year fixed effects, which flexibly control for municipality-and-year-specific characteristics (e.g., municipal income, social development, or political party in office). θ_m is a set of month fixed effects, which control for monthly shocks the municipality resulting from the increases in personnel. The mayor of Maragogipe in the northeastern state of Bahia was condemned in October 2017 to pay a fine of over USD16,000 (or about BRL53,000 in October 2017) for dismissing 104 temporary workers shortly after the election.

²¹Appendix D reports descriptive statistics of the outcomes by month.

common to all municipalities and thus account for underlying seasonality in public employment (for example, due to fiscal-year trends). D_{iym}^p is an indicator for whether observation iym is p months away from a municipal election, where p goes from -6 (corresponding to April of an election year) to 5 (March of a post-election year). β^p are the coefficients corresponding to those 12 electoral cycle periods. Y_{iym-1} is a lag of the dependent variable. Finally, ε_{iym} is an idiosyncratic error term. I cluster standard errors at the municipality level to allow for arbitrary serial correlation and heteroskedasticity.

In Appendix J, I show results are robust to alternative specifications, including using different transformations of the dependent variable (dropping observations where the unlogged outcome equals zero, taking the inverse hyperbolic sine transformation, or transforming employment outcomes into a binary measure of whether the count is larger than zero), omitting the lagged dependent variable, using two-way fixed effects instead of interactive fixed effects, using unbalanced panels, and omitting years with federal and state elections.

To identify heterogeneity in political bureaucratic cycles, I expand Equation 1 by adding a binary covariate and interacting it with both the month fixed effects and the election cycle period indicators, as per the following equation.

$$Y_{iym} = \alpha_i + \lambda_y + \theta_m + \sum_{p=-6}^5 \beta^p D_{iym}^p + \left(\zeta + \phi_m + \sum_{p=-6}^5 \delta^p D_{iym}^p \right) K_{iy} + \gamma Y_{iym-1} + \varepsilon_{iym} \quad (2)$$

K_{iy} is an indicator for whether municipality i in year y belongs to a subgroup of interest (e.g., municipalities exposed to a federal anti-corruption audit). Where K_{iy} is exogenously assigned (as in the case of the audits), the δ^p coefficients identify the heterogeneity in the cycles caused by that covariate. Where K_{iy} is not exogenous (e.g., decade), the δ^p coefficients simply describe how the cycles differ, descriptively, among the two groups. This specification uses two-way rather than interactive fixed effects because K_{iy} only varies at the municipality-year level.

Data

I leverage administrative data on public employment, healthcare service delivery, and anti-corruption audits in Brazilian municipalities. I focus on monthly variation to identify political bureaucratic cycles with a high level of granularity.

To measure how the electoral calendar shapes public employment I use the federal government's Annual Social Information Report (RAIS, *Relação Anual de Informações Sociais*) from 2000 to 2019 (a 20-year period covering 5 elections). Municipal governments –like all employers in the formal sector– are legally obliged to report all their contracts to the Ministry of the Economy every year.²² RAIS therefore contains data on the universe of municipal employees, including contract type, start and end dates, salary, reason for termination, and professional category, among other variables.²³ Using RAIS, I generate counts of hires, dismissals,²⁴ and other employment outcomes, by type of contract, for each municipality and each month.

To measure effects on public service delivery, I use data from the Ministry of Health's Basic Healthcare Information System (SIAB, *Sistema de Informação da Atenção Básica*).²⁵ The data are collected by municipal secretariats of healthcare, consolidated by state governments, and published by the federal government at the municipality-month level from 2004 to 2014 (a 11-year period covering 3 elections).²⁶ I use SIAB to generate counts of a number of healthcare services for each municipality in each quarter around elections. First, I use data on the number of home visits done by community health agents, nurses, and doctors. Second, I use data on the number of medical consultations with patients within different age brackets: 0-1, 1-4, 5-9, 10-14, 15-19, 20-39, 40-49, 50-59, and 60+.

I focus on these dimensions of healthcare service delivery for three main reasons. First, these activities are at the core of Brazil's municipal healthcare system. Indeed, studies seeking to assess the effectiveness of the system often include these variables as outcomes (Bhalotra et al., 2020; Castro et al., 2019; Aquino et al., 2009). Second, these activities are of substantive importance, since they help keep the local population alive and healthy. Medical consultations are critical for monitoring the health of infants and children, and for diagnosing and treating disease across all age

²²Additional details of the labor dataset are reported in Appendix B.

²³As shown in Appendix B, duringt he period 2000-2019 a small fraction between 1 and 10 percent of the municipalities do not show up as having any employee in any given year. Municipalities that fail to report employment data to the Ministry of Labor are generally smaller, poorer, and less developed. Analyses presented in this paper are therefore not representative of all of Brazil but of municipalities that report data to RAIS every year in the 2000-2019 period. This selection plausibly biases results towards zero, since poorer and less developed municipalities –where the clientelistic uses of public employment are more common, and bureaucracies are smaller and less professionalized– are likely to experience more pronounced cycles.

²⁴I consider dismissals to be contract terminations initiated by the employer (*exonerações a iniciativa do empregador*), and resignations terminations initiated by the employee (*exonerações a pedido*).

²⁵Additional details of the healthcare services dataset are reported in Appendix C.

²⁶From 2015 onwards the Ministry uses a different data system producing data that are not comparable.

groups. Prenatal and child healthcare are critical for lifelong health ([Forrest and Riley, 2004](#)) and frequently used as proxies for the quality of healthcare systems.²⁷ Home visits help provide care to people with reduced mobility (including people in rural areas) and complement services provided in healthcare facilities ([Ministério da Saúde, 2012b](#)).²⁸ Last, these healthcare services (except medical consultations with patients older than 5) are mandated rather than elective, so they are less subject to variation in citizen demand and sociodemographics than other healthcare services. Brazil's Ministry of Health recommends at least 1 monthly visit to every registered household ([Ministério da Saúde, 2012b](#)), 6 prenatal check-ups during pregnancies ([Ministério da Saúde, 2012a](#)), 7 medical consultations for children in their first year of life and at least 1 medical consultation per year for children older than 1 ([Ministério da Saúde, 2012c](#)).

To examine how whether the increased salience and enforcement of rules shapes cycles, I leverage data from the CGU on the randomly-assigned audits. These audits were done between 2006 and 2015.²⁹ I take municipalities to be exposed to an audit from the year when they are selected until 3 years later, i.e. for a four-year period which is the length of a mayoral mandate.³⁰ Given the salience and stakes of federal audits ([Avis et al., 2018](#)), it is reasonable that not only incumbent politicians but also bureaucrats and opposition leaders place higher weights on compliance with the law after an audit.

Results

Regression results demonstrate that, in Brazilian municipalities, bureaucratic inputs (hires) and outputs (healthcare services) fluctuate around elections, in a manner consistent with cycles being caused by both electoral incentives and the laws constraining political discretion during the 6-month window around elections.

²⁷For example, reducing child mortality and improving maternal health are two of the eight main United Nations Millennium Development Goals.

²⁸For example, home visits allow healthcare providers to change citizens' practices in order to prevent diseases and improve health outcomes. These at-home interventions are particularly important in rural and less developed areas.

²⁹Starting in 2016, CGU started targeting some audits by criteria other than audit, and has the policy not to report which municipalities were selected via lottery and which ones were not.

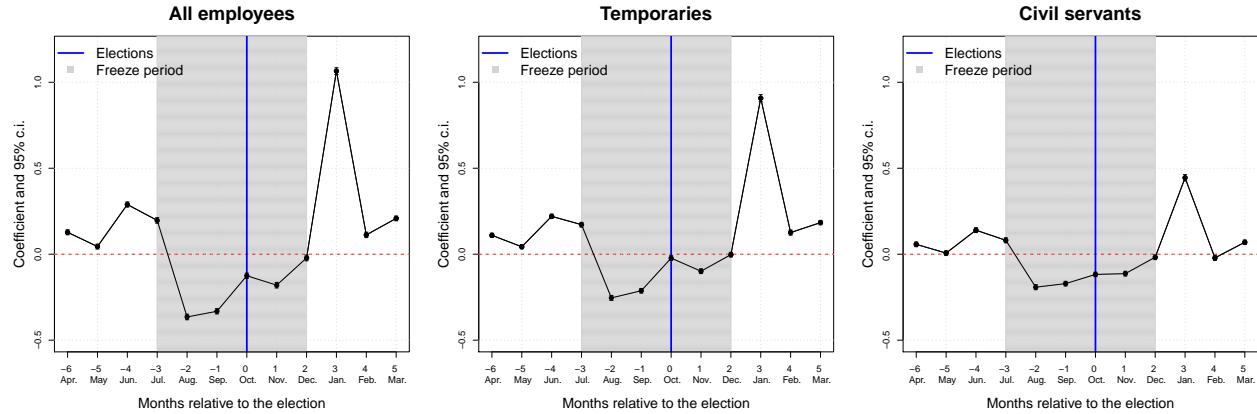
³⁰Results are similar when considering them exposed to an audit for 8 years, or for the whole period since selected via lottery.

Public employment

Figure 2 displays how the hiring of municipal employees fluctuates in the months around elections, compared to those same months in non-electoral years. These results, detailed in Table 2, are consistent with cycles in the hiring of employees being shaped by both electoral incentives and the legal rules designed to limit the use of public employment around elections. These cycles are not only driven by the hiring of bureaucrats who work hand in hand with politicians, such as managers, advisors, or assistants. Similar patterns are observed among low-skill employees and among professionals (Appendix E).

Hiring expands in the pre-freeze period, compared to the same period in non-election years. For example, in June of an electoral year, the hiring of employees is 33.5% higher than in June of a non-electoral year ($p < 0.001$).³¹ Hires on the first day of July are not covered by the freeze period, unless that day happens to fall on a Sunday. That is arguably why we observe the expansion of hiring persist in July, with 21.71% more hires than in July in a non-electoral year ($p < 0.001$).³² The expansion of hiring before the freeze period is consistent with politicians anticipating the legal constraints limiting their hiring and firing discretion, and which start in early July.

Figure 2: Political bureaucratic cycles in hires, by contract type



Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1.

Hiring contracts during the freeze period, consistent with politicians responding to the legal constraints on hiring around elections. For example, hires are 30.5% less common in August of

³¹In average, there are 12.09 hires in the month of June in a non-electoral year.

³²In average, there are 14.03 hires in the month of July in a non-electoral year.

an electoral year, compared to a non-electoral year ($p < 0.001$).³³ This contraction in hiring is more pronounced before the election, and less pronounced in the last quarter of the year, when the incidence of hiring is generally lower.³⁴

Hiring expands significantly after the election-related constraints on hiring are lifted. The effect is most pronounced in January, immediately after the end of the legal limits on hiring: we observe 190% more hires compared to a month of January that does not follow local elections ($p < 0.001$). This effect is particularly pronounced when considering that in January of a non-electoral year hires are quite common (41.59 in the average municipality), since many temporary contracts start on that month. The post-election expansion in the bureaucracy continues after January. For example, hires are 23.12% higher in March after an election than in that same month in a non-electoral year.³⁵ This expansion after the freeze period is likely driven by two mechanisms that coincide in time: the end of the freeze period and the beginning of a new political mandate.

The panels on the center and on the right of Figure 2 (and the corresponding columns in Table 2) show that there is significant heterogeneity in the intensity of the cycles by contract type. Unsurprisingly, the cycles are more pronounced for temporary contracts, where politicians have more discretion. On the other hand, there are also cycles in the hiring of civil servants. For example, civil service hiring is 15.01% higher in June, 17.45% lower in August, and 55.98% higher in January around an election, when compared to the same months in a regular year ($p < 0.001$).³⁶

The existence of political cycles in the hiring of civil servants has important implications for both research and policy. In cases like Brazil, the civil service limits (or eliminates) politicians' discretion over the targeting of jobs, but it does not eliminate their power over the quantity and the timing of hires. As the results in Figure 2 demonstrate, that discretion can be mobilized strategically ahead of elections. While some studies of cycles treat civil service hiring as a placebo outcome (Pierskalla and Sacks, 2020), in many contexts it may be important to examine empirically whether civil service hiring is as insulated from political influence as is typically assumed.

The pre-freeze period is also characterized by an uptick in resignations, as shown in Figure 3 and Table 3. For example, resignations are 9.65% higher in June of an electoral year, compared to a

³³In average, there are 16.63 hires in August in a non-electoral year.

³⁴For instance, hiring in November after an election declines by 16.48% ($p < 0.001$). In non-election years we observe in average 6.97 hires on that month.

³⁵In non-election years, there are in average 31.33 hires in March.

³⁶Civil service hiring is much less common. In average, there are 4.09, 5.57, and 10.9 civil service hires in June, August, and January in non-election years.

Table 2: Political bureaucratic cycles in hires, by contract type

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.127*** (0.007)	0.110*** (0.006)	0.057*** (0.007)
May	0.044*** (0.007)	0.043*** (0.006)	0.007 (0.007)
June	0.289*** (0.008)	0.220*** (0.007)	0.140*** (0.007)
July	0.196*** (0.008)	0.172*** (0.007)	0.081*** (0.007)
August	-0.365*** (0.008)	-0.254*** (0.007)	-0.192*** (0.007)
September	-0.332*** (0.008)	-0.213*** (0.007)	-0.171*** (0.007)
October	-0.125*** (0.008)	-0.023*** (0.007)	-0.118*** (0.007)
November	-0.180*** (0.008)	-0.099*** (0.007)	-0.113*** (0.007)
December	-0.022** (0.008)	-0.002 (0.007)	-0.018** (0.007)
January	1.07*** (0.010)	0.907*** (0.011)	0.445*** (0.009)
February	0.112*** (0.008)	0.126*** (0.008)	-0.021*** (0.007)
March	0.208*** (0.007)	0.184*** (0.007)	0.070*** (0.006)
Observations	998,640	998,640	998,640
Municipalities	4,161	4,161	4,161
R ²	0.702	0.727	0.628

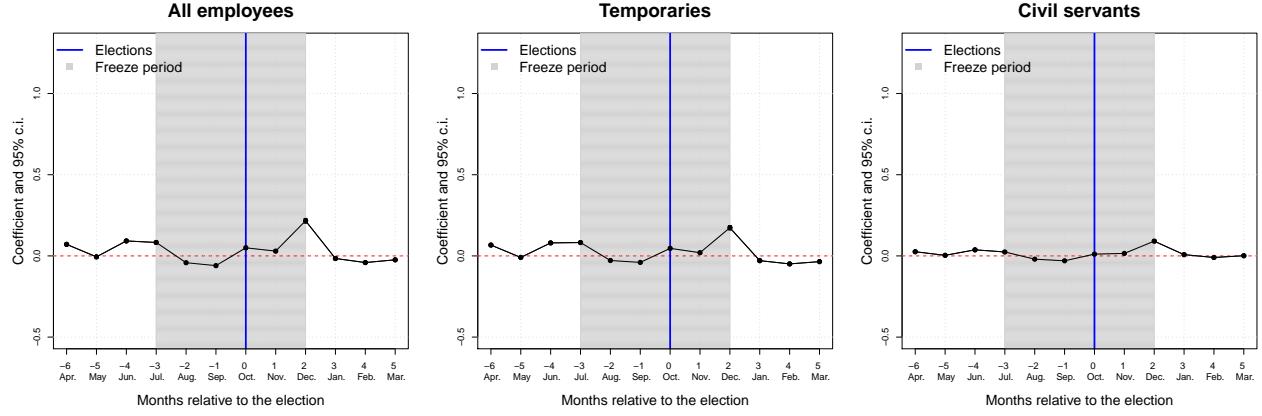
All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. * $p<0.05$; ** $p<0.01$; *** $p<0.001$.

non-electoral year ($p < 0.001$).³⁷ This increase in resignations is likely due to the legal requirement that bureaucrats who are running resign 3 or 6 months before the October election, depending on their post. These outflows of experienced bureaucrats, while small, may drive part of the increase in hires documented in Figure 2.

As a placebo test, I examine whether there are cycles in two outcomes that are reported in the same labor dataset (RAIS), namely deaths of employees and retirements. As shown in Figure 4, these two variables barely oscillate around elections, when compared to the same months in non-

³⁷Resignations are rare. In June of a non-electoral year, there are in average 0.57 resignations.

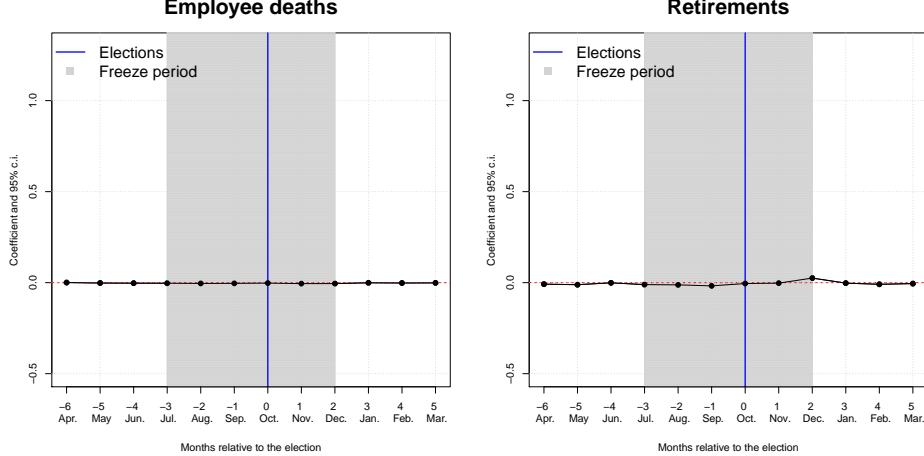
Figure 3: Political bureaucratic cycles in resignations, by contract type



Points and their confidence intervals (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1.

electoral years. While some coefficients are statistically significant, especially for retirements which may be strategically delayed in some cases until after the election, the size of these associations is very small compared to those in Figures 2 and 3.

Figure 4: Political bureaucratic cycles in placebo outcomes: Employee deaths and retirements



All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. * $p<0.05$; ** $p<0.01$; *** $p<0.001$. Regression details are in Appendix F

Two additional pieces of evidence lend support to the hypothesis that the shape of the cycles (with expansion of hiring before the freeze period, a contraction during the freeze, and an expansion again after the freeze) is driven by the combination of electoral incentives and the legal constraints

Table 3: Political bureaucratic cycles in resignations, by contract type

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.071*** (0.004)	0.067*** (0.004)	0.026*** (0.003)
May	-0.006 (0.004)	-0.009*** (0.004)	0.003 (0.003)
June	0.092*** (0.004)	0.080*** (0.004)	0.037*** (0.003)
July	0.083*** (0.005)	0.082*** (0.004)	0.024*** (0.004)
August	-0.042*** (0.004)	-0.028*** (0.004)	-0.020*** (0.003)
September	-0.060*** (0.004)	-0.040*** (0.004)	-0.030*** (0.003)
October	0.050*** (0.005)	0.046*** (0.004)	0.011*** (0.004)
November	0.030*** (0.005)	0.020*** (0.004)	0.015*** (0.003)
December	0.216*** (0.008)	0.173*** (0.007)	0.091*** (0.005)
January	-0.016*** (0.005)	-0.029*** (0.004)	0.008** (0.003)
February	-0.041*** (0.004)	-0.049*** (0.004)	-0.010*** (0.003)
March	-0.024*** (0.004)	-0.035*** (0.003)	0.001 (0.003)
Observations	998,640	998,640	998,640
Municipalities	4,161	4,161	4,161
R ²	0.735	0.705	0.700

All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. * $p<0.05$; ** $p<0.01$; *** $p<0.001$.

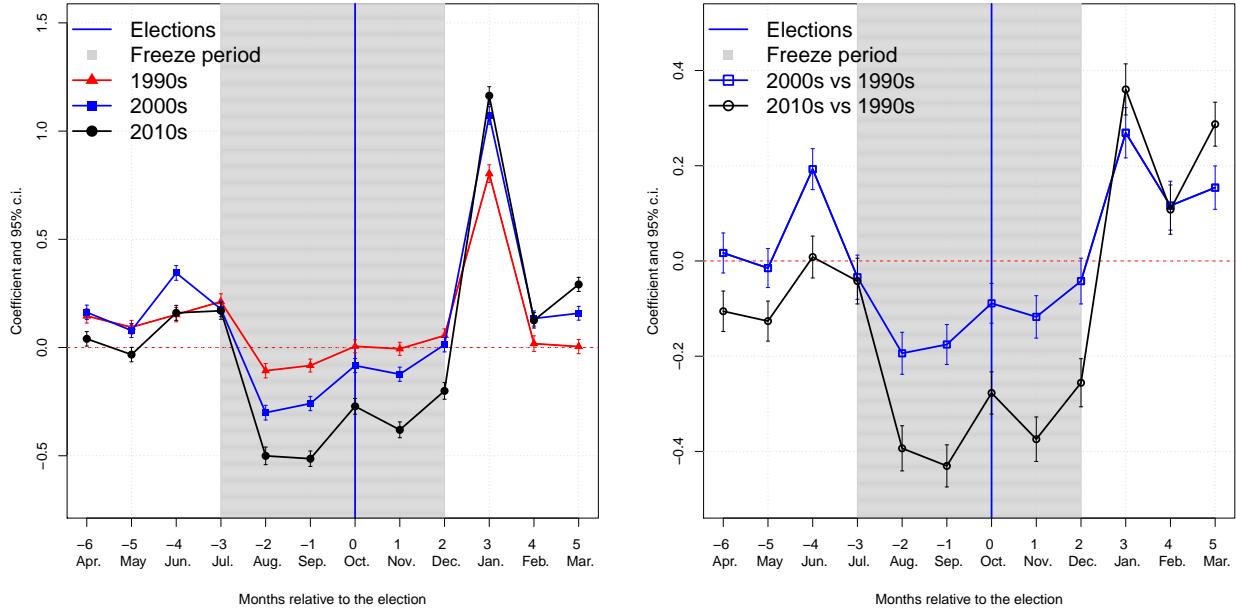
on hiring around elections. First, political bureaucratic cycles in hiring have intensified over time. Figure 5 describes how cycles vary by decade. For this analysis I include observations since 1995, at the expense of having a smaller balanced panel since compliance with RAIS was lower in the 1990s. The 1996 elections³⁸ were held before the 2000 Fiscal Responsibility Law and the 1997 Electoral Law were passed, and at a time when both monitoring and enforcement of rules was weaker.³⁹

³⁸The 1996 municipal elections, which were held before the constitutional amendment that mandated elections to be held on the first Sunday of October, were held on Thursday October 3.

³⁹Although a 1974 law, approved during the military dictatorship, made it illegal to hire or dismiss employees in the 6-month window around elections, the law that the democratic Congress passed in 1995 to regulate the 1996 elections did not mention any limits on hiring. In any case, monitoring and

Consistent with this, the depression of hiring during the freeze period is significantly weaker when considering only the 1995–1999 period, as are the expansions of hiring before and after the freeze period. In the 2000s and the 2010s, when the salience and enforcement of these rules increased, we observe more intense cycles in hiring.

Figure 5: Political bureaucratic cycles in total hires, by decade (1995–2019)

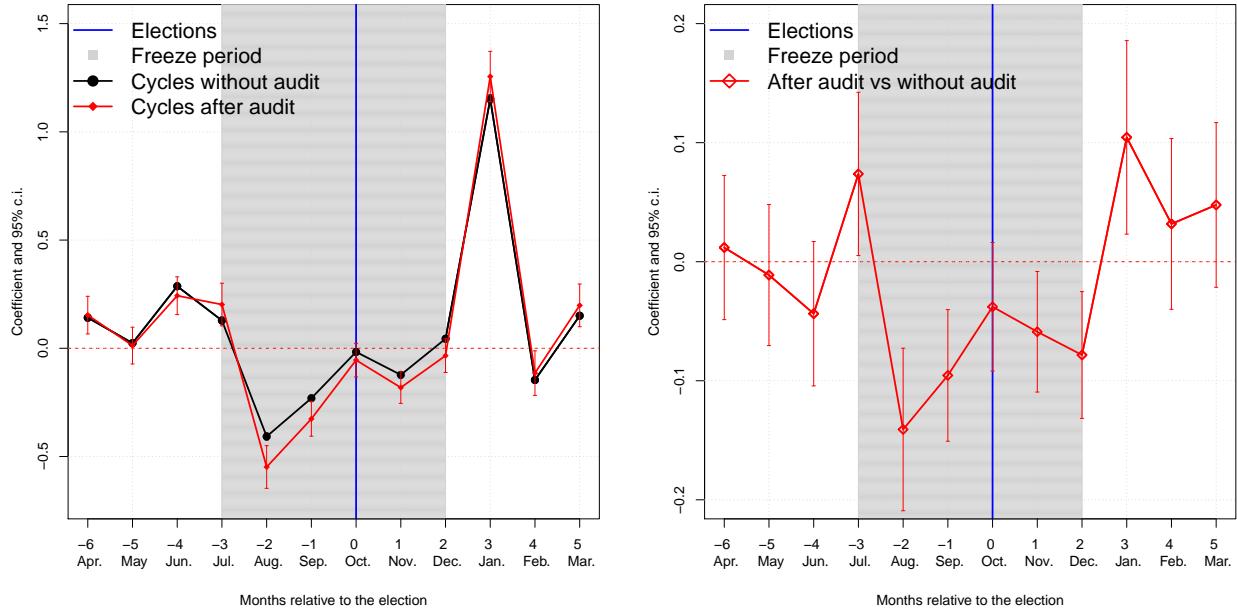


In the plot on the left, points and their confidence interval (c.i.) correspond to $\hat{\beta}$ coefficients in Equation 2. In the plot on the right, points and their ci correspond to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients in Equation 2. In the plot on the right, points and their confidence interval correspond to $\hat{\delta}$ coefficients in Equation 2. Regression details are in Appendix H.

Additional evidence comes from examining how political bureaucratic cycles in hiring vary in municipalities exposed to a CGU anti-corruption audit. While rules apply homogeneously to all municipalities *de jure*, their enforcement likely varies with oversight. The randomized federal government audits can be seen as an enforcement shock that not only uncover potential irregularities in the use of public funds, but also provide information and incentives that strengthen the work of other accountability actors (including the local opposition, the media, the prosecutors' office, etc.). Figure 6 shows that randomized anti-corruption audits intensify political bureaucratic cycles in hiring – with more pronounced expansions in hiring before and after the freeze period, and more enforcement were arguably much weaker in that election cycle, before the high-profile Electoral Law and Fiscal Responsibility Law, and before other more recent laws increased the penalties.

pronounced contractions during the freeze period. For example, audits cause the expansion of hiring in June of an electoral year to be 7.65% larger than in municipalities not exposed to an audit ($p < 0.05$). On the other hand, audits cause the contraction of hiring during the freeze period to be 13.14% larger ($p < 0.001$). In January, once the freeze is over, hiring expands 11% more in municipalities subjected to an audit than in those not selected under the CGU audit program ($p < 0.05$). These patterns are consistent with the legal constraints on hiring around elections, and their enforcement, displacing and shaping political bureaucratic cycles, rather than eliminating them.

Figure 6: Political bureaucratic cycles in total hires, by exposure to an anti-corruption audit



In the plot on the left, points and their confidence interval (c.i.) correspond to $\hat{\beta}$ coefficients in Equation 2 (dots) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients in Equation 2 (diamonds). In the plot on the right, points and their confidence interval correspond to $\hat{\delta}$ coefficients in Equation 2. Regression details are in Appendix I.

All in all, these results demonstrate that in Brazilian municipalities hiring follows marked cyclical patterns around elections, with hiring contracting in the 6-month window around elections and expanding both before and after such period, when compared to the same months in non-electoral years. These patterns are consistent with politicians anticipating the legal constraints on their discretion. In fact, cycles have only become more pronounced over time, and are more pronounced after a municipality is randomly exposed to an anti-corruption audit. In sum, laws

constraining politicians' use of public employment around elections displace rather than eliminate that practice, thereby shaping and even intensifying political bureaucratic cycles.

Service delivery

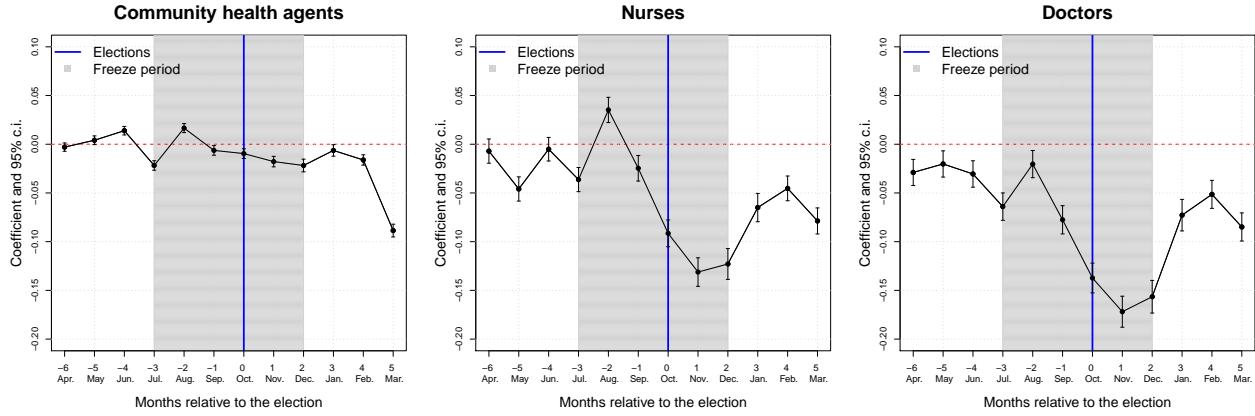
Do political bureaucratic cycles in inputs (public employment) have a correlate in outputs (services)? To address this question I examine political bureaucratic cycles in the delivery of healthcare services. Primary healthcare is one of the main responsibilities of municipal governments and a highly salient issue for voters in local elections ([Boas et al., 2019](#), 395), also shows patterns that are consistent with electoral incentives and legal constraints shaping cycles around elections.

Figure 7 and Table 4 show that there are marked political bureaucratic cycles in home visits by healthcare professionals. Home visits by community health agents, for whom home visits is probably the most important output, expand ahead of the elections. For example, in June of an electoral year there are 1.14% more visits by community health agents than in June of a non-electoral year ($p < 0.001$). In the average municipality, where community health agents perform 5,272 home visits in June of a non-electoral year, this translates into an increase of about 60 visits. The number of visits contracts during the first month of the freeze period (July) and from September onward. These declines are modest, especially when compared to those found for household visits by nurses and by doctors. For instance, in September of an electoral year, household visits by nurses and doctors decline by 3.33% and 8.29%, respectively, when compared to that same month in a non-electoral year ($p < 0.001$).

In-clinic medical consultations fluctuate following a pattern similar to those of household visits by CHAs: an expansion in the months leading up to the election (with a dip in July when the freeze period begins), and a decline starting one month before the election. These results are detailed in Figure 8 and Table 5. Interestingly, the pre-electoral expansion starts earlier and is more pronounced for medical consultations with adult patients than with youngsters.

These findings highlight an under-appreciated but important feature of electoral cycles in policy outputs: to the extent that delivering services is a function of finite resources, a pre-election expansion in some services is likely to lead to contractions in other areas. This is best appreciated by examining what one single type of bureaucrat does. In this case, evidence suggests that doctors perform more medical consultations with adults, to the detriment of medical consultations with

Figure 7: Political bureaucratic cycles in home visits, by healthcare professional category



Each point and its confidence interval (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1.

children and teenagers. On the other hand, the increases in medical consultations come at the expense of household visits. Cyc

A variety of factors likely shapes these political bureaucratic cycles in healthcare services. A first potential driver is the main input for the delivery of services in this context: human resources. As shown in Figure 9, hiring of specialized healthcare bureaucrats (such as community health agents, nurses, and doctors) do follow similar patterns of expansion before, and contraction during, the freeze period. For example, hires of healthcare professionals are 13.90% higher in July of an electoral year than in July of a non-electoral year ($p < 0.001$). In absolute terms, this expansion is small, since the number of new hires in this sector in July on a regular year is only 2.7. Therefore, the pre-election expansion of some healthcare services (e.g., household visits by CHAs, medical consultations with adults) must rely on factors other than an increase in workers. These factors may include an increase in individual bureaucrats' productivity or, as the results above suggest, a redistribution of bureaucratic effort away from certain tasks.

The contraction of healthcare services before and after the election is striking, because unlike hiring and other activities related to campaign (e.g., media appearances, shows, etc.) service delivery is not subject to legal constraints around elections. One possible driver of that contraction of service delivery around elections is the rigidities imposed by the electoral law on human resources management. If workers cannot be hired, dismissed, or transferred, it may be more challenging for clinic managers and senior officials at the secretariat of healthcare to fulfill the local needs of the population. Another plausible driver of these declines is the focus of senior officials and managers

Table 4: Political bureaucratic cycles in home visits, by healthcare professional category

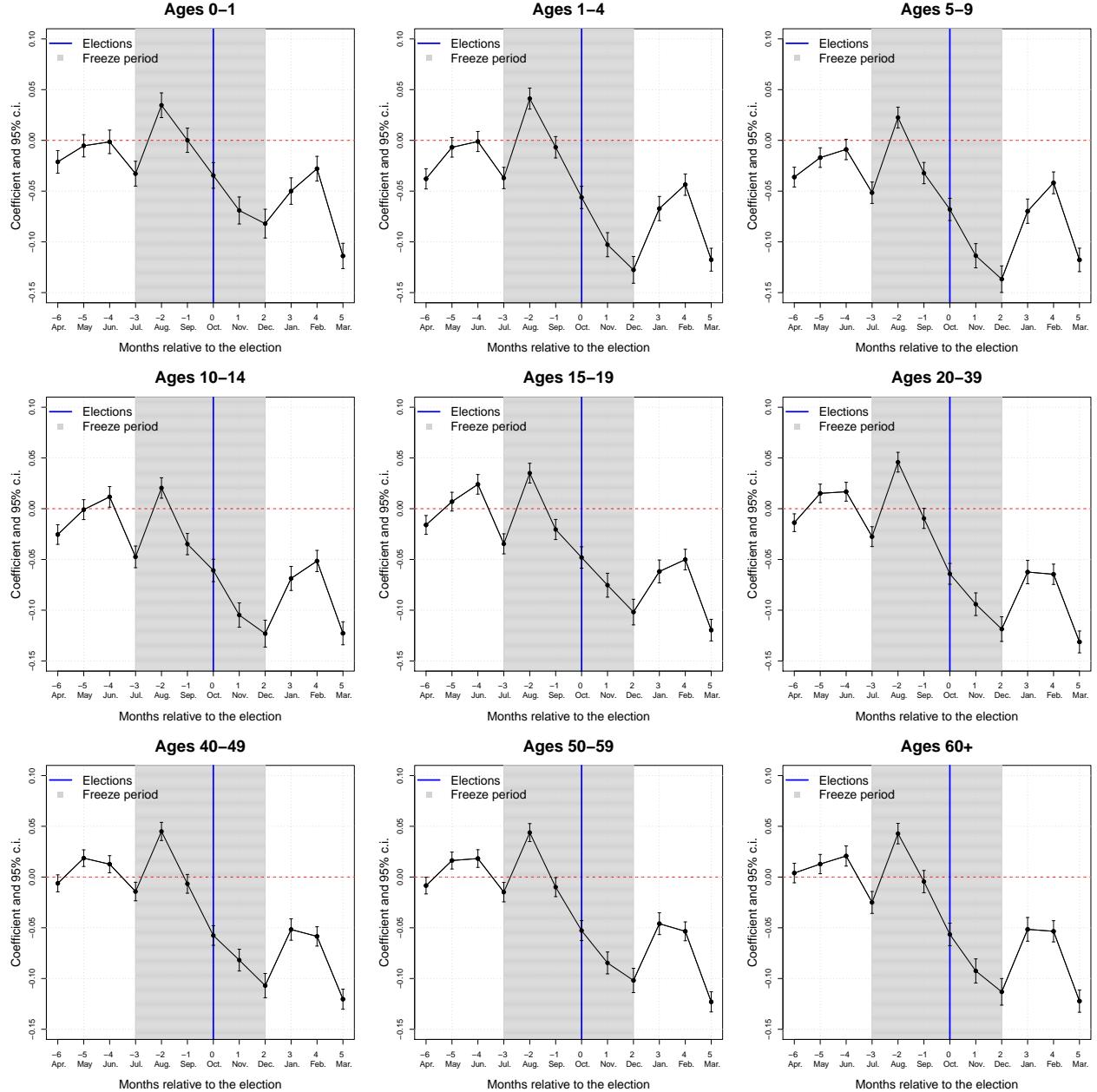
	CHAs (1)	Nurses (2)	Doctors (3)
April	-0.003 (0.002)	-0.007 (0.006)	-0.029*** (0.007)
May	0.004* (0.002)	-0.046*** (0.006)	-0.020*** (0.007)
June	0.014*** (0.002)	-0.005 (0.006)	-0.030*** (0.007)
July	-0.022*** (0.002)	-0.036*** (0.006)	-0.064*** (0.007)
August	0.017*** (0.002)	0.035*** (0.007)	-0.020*** (0.007)
September	-0.006** (0.003)	-0.025*** (0.007)	-0.078*** (0.007)
October	-0.010*** (0.003)	-0.091*** (0.007)	-0.137*** (0.008)
November	-0.018*** (0.003)	-0.131*** (0.007)	-0.172*** (0.008)
December	-0.022*** (0.003)	-0.123*** (0.008)	-0.156*** (0.009)
January	-0.006** (0.003)	-0.065*** (0.007)	-0.073*** (0.008)
February	-0.016*** (0.003)	-0.045*** (0.006)	-0.051*** (0.007)
March	-0.089*** (0.003)	-0.079*** (0.007)	-0.085*** (0.007)
Observations	549,911	473,879	439,031
Municipalities	4,166	3,590	3,326
R ²	0.963	0.852	0.846

All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. * $p<0.05$; ** $p<0.01$; *** $p<0.001$.

on the campaign, and the corresponding disruptions that may introduce in teams. Demand for healthcare services is unlikely to explain the patterns observed in Figures 7 and 8. That is particularly the case for services that are mandated rather than elective, such as medical consultations with infants (below 1 year old) and small children (ages 1-5) for which the Ministry of Health requires at least 7 per year and at least 1 per year, respectively.

These results are suggestive rather than conclusive, but point to the possibility that constraints around elections may have important costs in terms of public service delivery, and to the trade-offs that election-induced government responsiveness induces. These trade-offs appear to operate across activities that bureaucrats perform (with some services expanding to the detriment of others)

Figure 8: Political bureaucratic cycles in medical consultations, by age of patient



Each point and its confidence interval (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1.

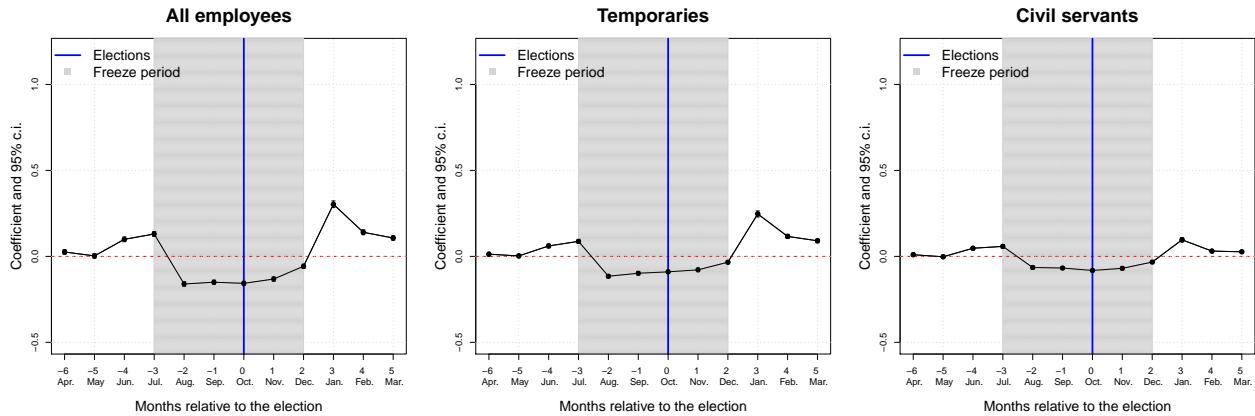
and across time (with pre-election expansion being followed by contraction). Future research might seek to examine whether there are trade-offs across policy sectors as well.

Table 5: Political bureaucratic cycles in medical consultations, by patient age

	0-1 (1)	1-4 (2)	5-9 (3)	10-14 (4)	15-19 (5)	20-39 (6)	40-49 (7)	50-59 (8)	60+ (9)
April	-0.021*** (0.006)	-0.038*** (0.005)	-0.036*** (0.005)	-0.025*** (0.005)	-0.016*** (0.005)	-0.014*** (0.004)	-0.006 (0.004)	-0.008** (0.004)	0.004 (0.005)
May	-0.005 (0.006)	-0.007 (0.005)	-0.017*** (0.005)	-0.0009 (0.005)	0.007 (0.005)	0.015*** (0.005)	0.019*** (0.004)	0.016*** (0.004)	0.013*** (0.005)
June	-0.001 (0.006)	-0.001 (0.005)	-0.009* (0.005)	0.012** (0.005)	0.024*** (0.005)	0.017*** (0.005)	0.013*** (0.004)	0.018*** (0.004)	0.021*** (0.005)
July	-0.033*** (0.006)	-0.037*** (0.005)	-0.052*** (0.005)	-0.048*** (0.005)	-0.035*** (0.005)	-0.028*** (0.005)	-0.014*** (0.005)	-0.015*** (0.005)	-0.025*** (0.006)
August	0.035*** (0.006)	0.041*** (0.005)	0.022*** (0.005)	0.020*** (0.005)	0.035*** (0.005)	0.046*** (0.005)	0.045*** (0.005)	0.044*** (0.005)	0.043*** (0.005)
September	0.0001 (0.006)	-0.007 (0.005)	-0.032*** (0.005)	-0.035*** (0.005)	-0.021*** (0.005)	-0.010* (0.005)	-0.007 (0.005)	-0.010** (0.005)	-0.004 (0.006)
October	-0.035*** (0.006)	-0.056*** (0.006)	-0.068*** (0.006)	-0.061*** (0.006)	-0.048*** (0.005)	-0.064*** (0.005)	-0.058*** (0.005)	-0.053*** (0.005)	-0.057*** (0.006)
November	-0.069*** (0.007)	-0.103*** (0.006)	-0.114*** (0.006)	-0.105*** (0.006)	-0.075*** (0.006)	-0.094*** (0.006)	-0.082*** (0.005)	-0.085*** (0.005)	-0.092*** (0.006)
December	-0.082*** (0.007)	-0.128*** (0.007)	-0.137*** (0.007)	-0.123*** (0.007)	-0.102*** (0.006)	-0.119*** (0.006)	-0.107*** (0.006)	-0.102*** (0.006)	-0.113*** (0.007)
January	-0.050*** (0.007)	-0.067*** (0.006)	-0.070*** (0.006)	-0.069*** (0.006)	-0.062*** (0.006)	-0.062*** (0.006)	-0.052*** (0.005)	-0.046*** (0.005)	-0.051*** (0.006)
February	-0.028*** (0.006)	-0.044*** (0.005)	-0.042*** (0.005)	-0.051*** (0.005)	-0.050*** (0.005)	-0.065*** (0.005)	-0.059*** (0.005)	-0.053*** (0.005)	-0.053*** (0.005)
March	-0.114*** (0.006)	-0.118*** (0.006)	-0.118*** (0.006)	-0.123*** (0.006)	-0.120*** (0.005)	-0.131*** (0.005)	-0.120*** (0.005)	-0.123*** (0.005)	-0.122*** (0.006)
Observations	459,491	476,255	477,047	478,631	480,611	483,119	482,591	482,459	481,535
Municipalities	3,481	3,608	3,614	3,626	3,641	3,660	3,656	3,655	3,648
R ²	0.874	0.886	0.876	0.867	0.877	0.885	0.892	0.897	0.881

All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001.

Figure 9: Political bureaucratic cycles in hires of healthcare professionals, by contract type



Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1.

Conclusion

A vast literature in political science and economics has shown that, at least when they have both the incentives and the ability to do so, politicians often manipulate policy ahead of elections as a way to improve their re-election chances. From a democratic stance point, these patterns are problematic because they imply that incumbents abuse their control over government during electoral campaigns, thus damaging political competition. From a fiscal perspective, the expansion of spending, debt, or public employment ahead of elections is problematic because it can jeopardize fiscal discipline.

In this context, countries often establish laws that specifically constrain incumbents' discretion over policy tools in the months leading up to an election. Operating under either a fiscal or an electoral rationale, these laws typically establish freeze periods around elections under which politicians' discretion is severely constrained.

This paper advances our understanding of political cycles, and of the impact of these anti-cyclical policies, through an empirical analysis of political bureaucratic cycles (namely, cycles in public employment and in the activities government workers perform) in the context of Brazilian municipalities. Leveraging detailed administrative datasets released by the federal government, I build panels of hundreds of thousands of municipality-month observations. Exploiting the exogenous timing of elections (held every 4 years on the first Sunday of October) and the bans on hiring and firing employees during a 6-month window around elections, I examine how hires and public services fluctuate around elections.

The results show that while hiring contracts during the freeze period (in line with the legal constraints), it also expands both before and after. This suggests that politicians respond strategically to both electoral incentives and legal constraints, and rather than refraining from cyclical uses of public employment they anticipate the expansion of the payroll until right before the ban. Cycles are present for both temporary and civil service hiring, which calls into question the common assumption of civil service hiring as completely insulated from political influence, and draws attention to the relevance of politicians' discretion over the timing of civil service hires. Cycles have become more pronounced over time (as rules and their enforcement have intensified) and are significantly more marked in municipalities randomly exposed to an anti-corruption audit. Together, these results suggest that, rather than eliminating cycles, legal constraints on policy tools displace and even exacerbate them.

The results also show marked cycles in bureaucratic outputs, namely in the delivery of health-care services. Some services (such as medical consultations or household visits by community health agents) expand before the election, consistent with politicians using service delivery as a signaling device towards voters. On the other hand, other services contract during that same period, and all of them tend to contract starting one month before the election. These patterns suggest that there are important trade-offs in election-driven government responsiveness.

The paper makes three contributions, both theoretical and empirical, to the literature on political cycles. First, the paper advances our understanding of the temporal dynamics of cycles. It does so by highlighting how incentives and constraints vary throughout the electoral calendar, and by using administrative data at the monthly level (rather than the more typical yearly or quarterly panels). Second, the paper challenges the common policy prescription of constraining politicians' discretion over policy tools around elections, by showing that under the strict limits of Brazilian legislation, cycles persist and in fact are shaped to those constraints. Third, by linking data on bureaucratic inputs (jobs) to bureaucratic outputs (results of how that labor force is used), the paper advances our understanding of how politicians' resource allocations and policy outcomes are connected. While other papers have examined cycles in bureaucratic outputs ([Kailthya et al., 2022](#); [Dipoppa and Grossman, 2020](#); [Khemani, 2004](#)), this is to my knowledge the first paper examining both bureaucratic inputs and outputs in the same empirical setting.

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Appendices

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A Additional details on legal constraints around elections

Rules in the Fiscal Responsibility Law (LRF) concerning personnel expenses

The Fiscal Responsibility Law (LRF, Complementary Law 101, approved on May 4, 2000) includes seven main rules designed for controlling personnel expenses and their use as patronage in electoral years.⁴⁰ 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). Third, compliance with this limit is verified at the end of every quadrimestre or four-month period. If personnel expenses are over 90% of the limit (i.e. over 51.3%), 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,⁴¹ 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). 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](#)).

Rules in the Electoral Law concerning the hiring and firing of bureaucrats

Brazil's Electoral Law (Law 9,504, approved on September 30, 1997)⁴² establishes a number of rules constraining the behavior of public officials in order to ensure the fair competition of candidates. These rules include a number of provisions regarding the hiring and firing of bureaucrats.

⁴⁰The whole law can be found at http://www.planalto.gov.br/ccivil_03/leis/lcp/lcp101.htm.

⁴¹Voluntary transfers are transfers from other levels of government that are not related to the healthcare system or mandated by the constitution.

⁴²The whole law can be found at http://www.planalto.gov.br/ccivil_03/leis/l9504.htm.

First, bureaucrats cannot be hired, dismissed with no fair cause (*sem causa justa*), or transferred, from 3 months before the election up to January 1st, with the exception of positions of trust, the hiring of people who passed a civil service examination before the beginning of the period (article 73.V), or hiring of positions necessary for the delivery of *essential* services (which the jurisprudence of the Supreme Electoral Court has clarified do not include education). Second, wages cannot be increased beyond adjustments that allow employees to recover any purchasing power lost during the election year (article 73.VIII). Municipalities cannot receive voluntary transfers from the federal or state government during the 3 months before and the 3 months after the period, with the exception of those destined to emergency situations (article 73.VI.a).

The law also establishes a number of strong penalties for breaches, including fines (to be paid by the candidate and/or their party), the suspension of the electoral candidacy of those benefited by the decision, the loss of access to the party financing system (*Fundo Partidário*), and the penalties established in the Law of Administrative Impropriety (including the loss of any public position, the suspension of political rights between 3 and 5 years, and payment of a fine up to 100 times the wage received as official).

Rules in the Law of Ineligibilities concerning the incompatibility of holding a bureaucratic position and running for election

Brazil's Law of Ineligibilities (Complementary Law 64, approved on May 18, 1990),⁴³ establishes certain limits on who can run for office, and allows for some time windows before the election in which "incompatibilities" can be fixed. The limits vary by the office a person is running for and the position they hold, but for city councilor art. 1.V establishes that public employees (with or without tenure) should be removed from their post up to 3 months before the election, except those involved in tax collection who should be removed from their posts 6 months before the election. Those who are tenured can simply leave their posts until the election, with pay. Those who are hired with temporary contracts or in positions of trust must leave their jobs.⁴⁴

⁴³The whole law can be found at http://www.planalto.gov.br/ccivil_03/leis/lcp/lcp64.htm.

⁴⁴The supreme electoral court has a varied jurisprudence on the issue, which can be consulted at <http://www.tse.jus.br/eleicoes/desincompatibilizacao/desincompatibilizacao>.

B Administrative labor market data

I leverage the anonymized RAIS, made available by Brazil's Ministry of the Economy. In it, I identify municipal employees using the legal nature of the employer and the municipality.⁴⁵ Descriptive statistics for the data on municipal employees are reported in Table ???. Between 2000 and 2019 the number of municipal government contracts has increased by about 3.9 million or 131%, but the share of civil service employees has remained roughly constant at about two thirds.⁴⁶ I code as civil service contracts those in the *regime jurídico único de servidores públicos*, and as temporary all other employees, who are hired through a variety of legal regimes.⁴⁷

Municipal governments (like all formal employers) are legally required⁴⁸ to report data for all its employees⁴⁹ to the Ministry of the Economy through the RAIS system. Yet, a minority of them (between 0.84 and 3.09% in the years I use) do not show up in the data. Technical staff at the Ministry confirmed that some municipalities fail to report employment data to RAIS, and associated it to capacity issues and/or corruption.

To understand the kind of municipalities that are not reporting employment data to RAIS, I examine the 89 municipalities that do not show up in the data in 2016,⁵⁰ and compare them to all 5,569 municipalities.⁵¹ As can be seen in Figure 10, municipalities failing to report employment data tend to be smaller, poorer, and less developed. This is consistent with both capacity and corruption mechanisms driving attrition. To the extent that municipal development correlates with the political use of public employment (Colonnelli et al., 2019; Barbosa and Ferreira, 2021), their exclusion from the data is biasing the results. This bias, however, is likely to be in the direction of

⁴⁵I consider only employees hired by municipal executive governments and their foundations and other dependent entities.

⁴⁶This share is the same in the data about municipal employees collected through government surveys by the Brazilian Institute of Geography and Statistics (IBGE, *Instituto Brasileiro de Geografia e Estatística*).

⁴⁷Unfortunately, RAIS does not allow a reliable identification of temporary workers who are politically appointed (e.g., *cargo comissionado, função de confiança*).

⁴⁸Entities failing to comply with the obligation to report employment data to RAIS or reporting inaccurate data are subject to fines. Moreover, employers have a direct incentive to comply since employees who do not appear in RAIS are not eligible for PIS-PASEP, a well-known and constitutionally-enshrined program that complements the wages of formal workers who make less than twice the minimum wage. In 2017, about half of municipal government labor contracts were below that threshold.

⁴⁹Elected officials, interns, and very transitory workers (*eventuais*) are not considered employees for the purposes of RAIS.

⁵⁰Results are similar when analyzing the municipalities not reporting data in 2004.

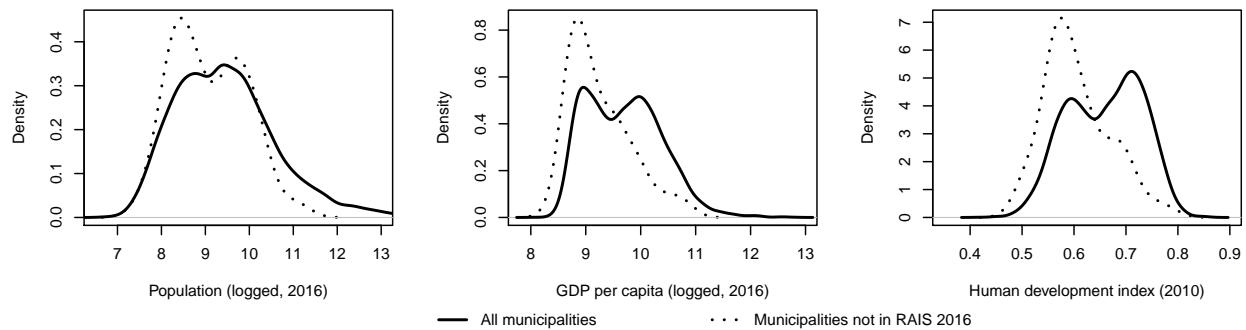
⁵¹I exclude Brasília because it does not have a municipal government.

attenuating results (i.e. bringing them closer to zero). In any case, results are not representative of the overall population of municipalities, but rather of those complying with the RAIS reporting requirement.

Table 6: Descriptive statistics for municipal employees as identified in RAIS

Year	Municipalities	% of total	Millions of contracts	Share civil service
2019	5496	98.69	6.76	0.65
2018	5512	98.98	6.62	0.66
2017	5522	99.16	6.60	0.67
2016	5480	98.40	6.42	0.67
2015	5516	99.05	6.49	0.66
2014	5521	99.14	6.50	0.65
2013	5499	98.74	6.50	0.64
2012	5513	99.08	6.09	0.65
2011	5509	99.01	6.09	0.64
2010	5522	99.25	5.72	0.63
2009	5497	98.80	5.61	0.64
2008	5481	98.51	5.33	0.65
2007	5497	98.81	5.02	0.66
2006	5501	98.89	4.75	0.66
2005	5459	98.13	4.41	0.66
2004	5387	96.91	4.06	0.69
2003	5370	96.60	3.90	0.69
2002	5306	95.45	3.62	0.69
2001	5209	93.70	3.31	0.68
2000	4978	90.41	2.92	0.65
1999	4891	88.83	2.73	0.65
1998	4864	88.34	2.61	0.66
1997	4377	79.50	2.48	0.66
1996	4296	78.02	2.34	0.64
1995	4159	83.63	2.31	0.62

Figure 10: Socioeconomic characteristics of municipalities not reporting employment data in 2016



C Administrative healthcare data

I leverage two sources of administrative data on healthcare bureaucracies and the services they provide. Both can be accessed through the Ministry of Healthcare's [DATASUS](#) portal.

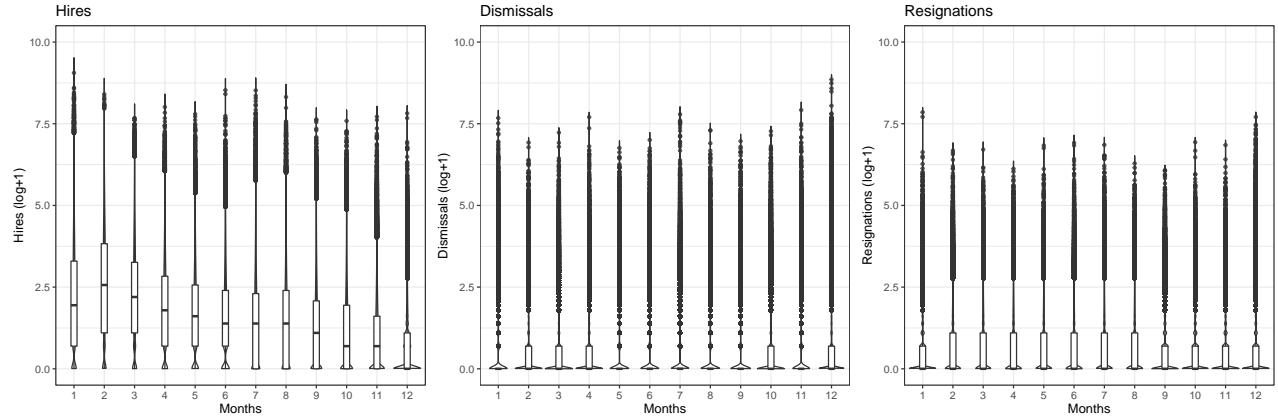
To measure the effects on public service delivery I use data from the Ministry of Health's Basic Healthcare Information System (SIAB, *Sistema de Informação da Atenção Básica*). The data are collected by municipal secretariats of healthcare, consolidated by state governments, and published by the federal government at the municipality-month level from 2004 to 2014.⁵² I use SIAB to generate counts of a number of healthcare services for each municipality in each quarter around elections.

Municipal governments are legally required to compile and submit the corresponding data to SIAB ([Ministério da Saúde, 2012d](#)). The quality of health data collected by the Ministry of Healthcare has been examined empirically by researchers who have generally found them to be reliable despite some errors ([Piccolo, 2018](#); [Rocha et al., 2018](#)).

⁵²The 2016 election cycle is thus excluded from these analyses.

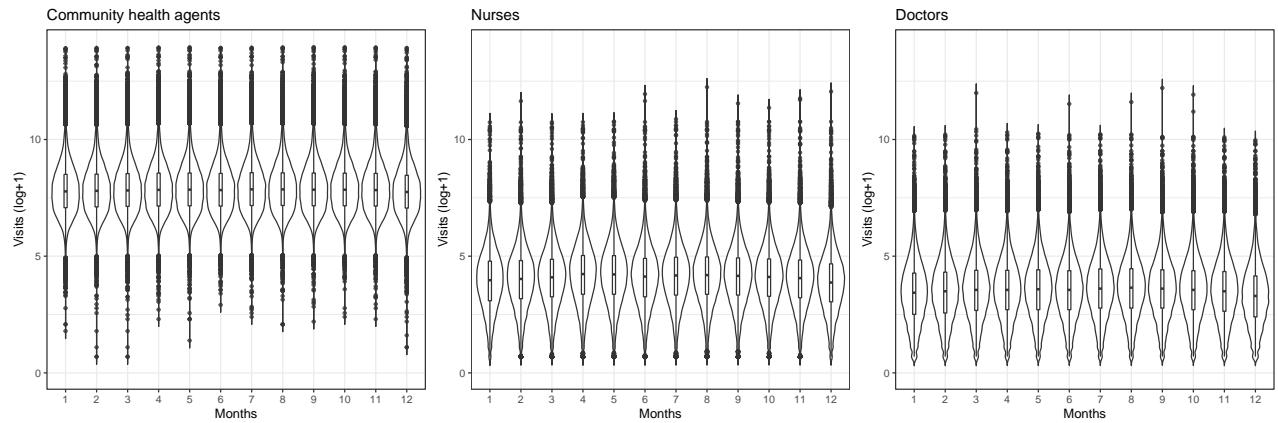
D Distributions of outcome data

Figure 11: Distribution of hires, dismissals, and resignations, by month



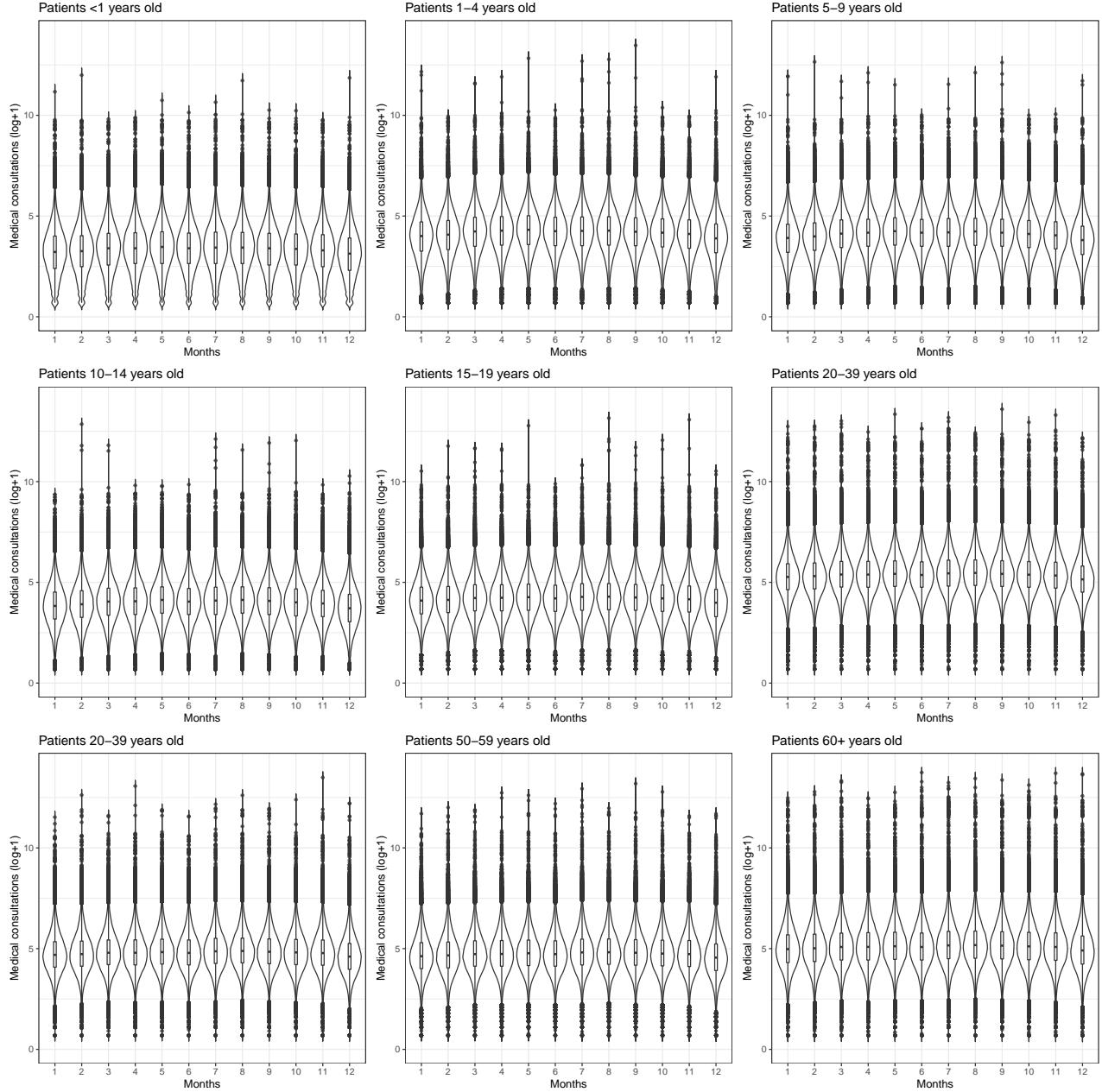
Horizontal lines correspond to the median. Boxes cover the interquartile range.

Figure 12: Distribution of healthcare household visits, by professional category and month



Horizontal lines correspond to the median. Boxes cover the interquartile range.

Figure 13: Distribution of medical consultations, by patient's age and month



Horizontal lines correspond to the median. Boxes cover the interquartile range.

E Political bureaucratic cycles in hires, by job skill level

I use the Brazilian Classification of Occupations (CBO, *Classificação Brasileira de Ocupações*), as reported by RAIS. CBO 1 corresponds to managers; CBO 2 corresponds to “scientific and artistic professionals”; CBO 3 to mid-level technicians; and CBO 4+ corresponds to administrative services and other low-skill workers.

Figure 14: Political bureaucratic cycles in hires: Managers (CBO 1)

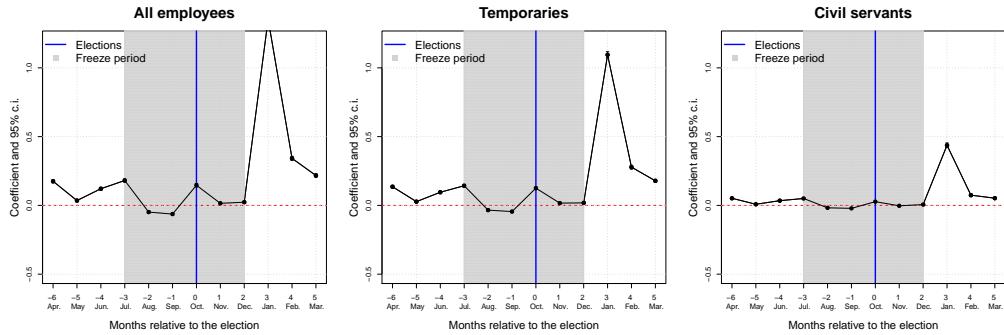


Figure 15: Political bureaucratic cycles in hires: Professionals (CBO 2 or 3)

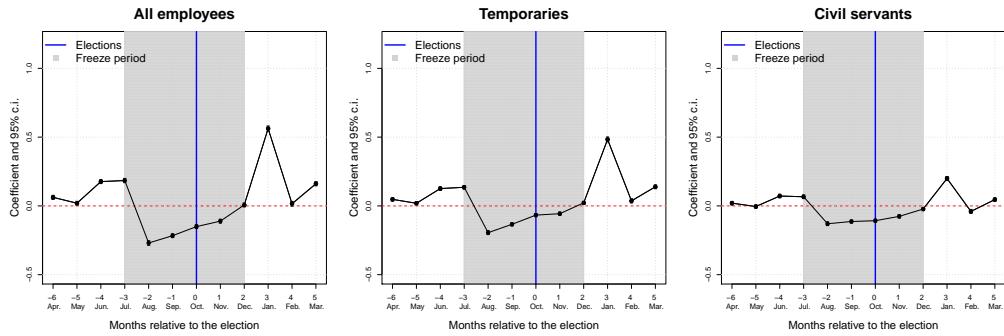
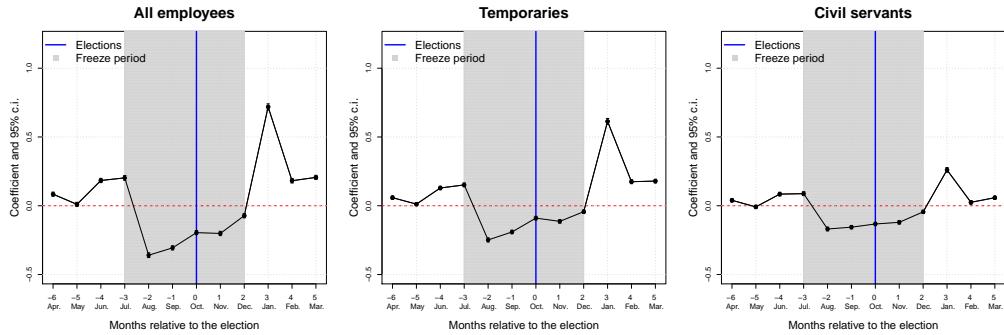


Figure 16: Political bureaucratic cycles in hires: Low-skill employees (CBO 4+)



Points and their confidence intervals (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1.

F Political bureaucratic cycles in placebo outcomes: Employee deaths and retirements

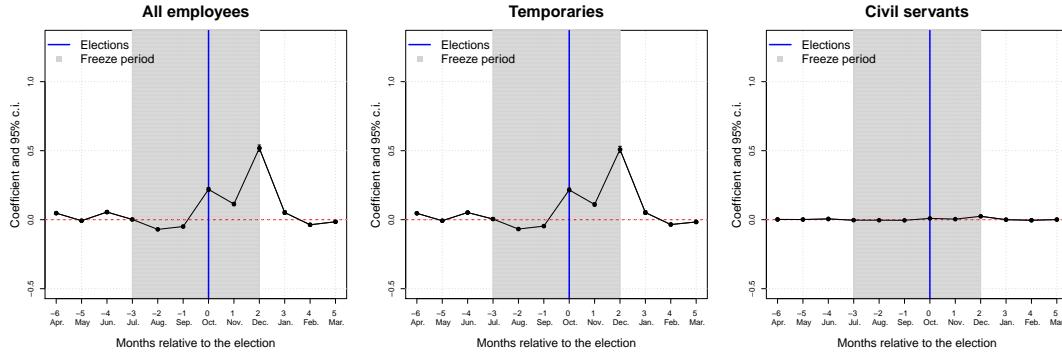
Table 7: Political bureaucratic cycles in placebo outcomes: Employee deaths and retirements

	Deaths (1)	Retirements (2)
April	0.0007 (0.002)	-0.008*** (0.003)
May	-0.002 (0.002)	-0.011*** (0.003)
June	-0.003 (0.002)	-0.0008 (0.003)
July	-0.003 (0.002)	-0.011*** (0.003)
August	-0.004** (0.002)	-0.012*** (0.003)
September	-0.004** (0.002)	-0.018*** (0.003)
October	-0.002 (0.002)	-0.005 (0.003)
November	-0.005*** (0.002)	-0.003 (0.003)
December	-0.005** (0.002)	0.025*** (0.003)
January	-0.0006 (0.002)	-0.003 (0.003)
February	-0.002 (0.002)	-0.009*** (0.003)
March	-0.001 (0.002)	-0.005* (0.003)
Observations	998,640	998,640
Municipalities	4,161	4,161
R ²	0.406	0.603

All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. * $p<0.05$; ** $p<0.01$; *** $p<0.001$.

G Political bureaucratic cycles in dismissals

Figure 17: Political bureaucratic cycles in dismissals, by contract type



Points and their confidence intervals (c.i.) corresponds to the β coefficients in Equation 1.

Table 8: Political bureaucratic cycles in fires, by contract type

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.047*** (0.004)	0.046*** (0.004)	0.002 (0.001)
May	-0.008* (0.004)	-0.007* (0.004)	0.001 (0.001)
June	0.055*** (0.004)	0.052*** (0.004)	0.005*** (0.001)
July	0.002 (0.004)	0.005 (0.004)	-0.003** (0.001)
August	-0.070*** (0.004)	-0.068*** (0.004)	-0.004*** (0.001)
September	-0.050*** (0.004)	-0.047*** (0.004)	-0.005*** (0.001)
October	0.220*** (0.007)	0.216*** (0.007)	0.009*** (0.002)
November	0.113*** (0.006)	0.110*** (0.006)	0.004*** (0.002)
December	0.518*** (0.012)	0.509*** (0.012)	0.025*** (0.002)
January	0.052*** (0.007)	0.052*** (0.007)	2.79×10^{-5} (0.001)
February	-0.037*** (0.004)	-0.035*** (0.004)	-0.004*** (0.001)
March	-0.015*** (0.004)	-0.017*** (0.004)	0.0008 (0.001)
Observations	998,640	998,640	998,640
Municipalities	4,161	4,161	4,161
R ²	0.647	0.650	0.466

All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. * $p<0.05$; ** $p<0.01$; *** $p<0.001$.

H Political bureaucratic cycles in hires, by decade

Table 9: Political bureaucratic cycles in hires by decade and by contract type

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.146*** (0.017)	0.083*** (0.013)	0.087*** (0.015)
May	0.094*** (0.016)	0.041*** (0.013)	0.053*** (0.013)
June	0.152*** (0.017)	0.086*** (0.013)	0.075*** (0.014)
July	0.212*** (0.018)	0.122*** (0.015)	0.117*** (0.015)
August	-0.107*** (0.017)	-0.076*** (0.013)	-0.057*** (0.014)
September	-0.083*** (0.015)	-0.049*** (0.012)	-0.041*** (0.012)
October	0.006 (0.015)	0.011 (0.012)	-0.005 (0.013)
November	-0.006 (0.015)	-0.0006 (0.012)	-0.008 (0.013)
December	0.056*** (0.016)	0.037*** (0.012)	0.019 (0.013)
January	0.803*** (0.021)	0.519*** (0.019)	0.435*** (0.018)
February	0.018 (0.018)	0.0001 (0.016)	-0.023 (0.015)
March	0.004 (0.017)	0.036** (0.014)	-0.027** (0.013)
2000s × April	0.017 (0.021)	0.019 (0.016)	0.030 (0.018)
2000s × May	-0.015 (0.020)	0.0005 (0.016)	-0.011 (0.017)
2000s × June	0.193*** (0.021)	0.141*** (0.017)	0.123*** (0.018)
2000s × July	-0.034 (0.022)	-0.004 (0.018)	-0.011 (0.019)
2000s × August	-0.194*** (0.020)	-0.130*** (0.016)	-0.112*** (0.018)
2000s × September	-0.175*** (0.018)	-0.124*** (0.015)	-0.084*** (0.016)
2000s × October	-0.089*** (0.018)	-0.037** (0.015)	-0.067*** (0.016)
2000s × November	-0.117*** (0.019)	-0.084*** (0.016)	-0.056*** (0.016)
2000s × December	-0.042** (0.019)	-0.042*** (0.015)	0.0009 (0.017)
2000s × January	0.269** (0.024)	0.373*** (0.022)	0.009 (0.022)
2000s × February	0.116*** (0.022)	0.172*** (0.019)	-0.025 (0.019)
2000s × March	0.154*** (0.020)	0.142*** (0.017)	0.037** (0.017)
2010s × April	-0.106** (0.020)	-0.037** (0.017)	-0.077*** (0.019)
2010s × May	-0.126*** (0.020)	-0.061*** (0.017)	-0.065*** (0.018)
2010s × June	0.008 (0.021)	0.041** (0.017)	0.010 (0.018)
2010s × July	-0.042* (0.023)	0.046** (0.019)	-0.043** (0.020)
2010s × August	-0.393*** (0.022)	-0.326*** (0.019)	-0.148*** (0.019)
2010s × September	-0.430*** (0.020)	-0.339*** (0.017)	-0.185*** (0.017)
2010s × October	-0.277*** (0.020)	-0.145*** (0.017)	-0.177*** (0.017)
2010s × November	-0.374*** (0.020)	-0.268*** (0.017)	-0.188*** (0.017)
2010s × December	-0.256*** (0.020)	-0.196*** (0.017)	-0.100*** (0.018)
2010s × January	0.360*** (0.025)	0.571*** (0.024)	-0.017 (0.024)
2010s × February	0.108*** (0.022)	0.147*** (0.020)	0.023 (0.020)
2010s × March	0.287*** (0.021)	0.227*** (0.018)	0.135*** (0.017)
Observations	963,300	963,300	963,300
Municipalities	3,211	3,211	3,211
R ²	0.714	0.739	0.635

All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001.

I Political bureaucratic cycles in hires, by audit

Table 10: Political bureaucratic cycles in hires by anti-corruption audit and by contract type

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.142*** (0.008)	0.134*** (0.007)	0.050*** (0.008)
May	0.024*** (0.007)	0.024*** (0.006)	-0.016** (0.007)
June	0.287*** (0.007)	0.218*** (0.006)	0.132*** (0.007)
July	0.128*** (0.008)	0.104*** (0.007)	0.031*** (0.007)
August	-0.408*** (0.008)	-0.305*** (0.007)	-0.225*** (0.007)
September	-0.231*** (0.007)	-0.112*** (0.006)	-0.115*** (0.006)
October	-0.017** (0.007)	0.082*** (0.006)	-0.053** (0.006)
November	-0.123*** (0.008)	-0.059*** (0.006)	-0.065*** (0.006)
December	0.044*** (0.008)	0.055*** (0.006)	0.028*** (0.006)
January	1.15*** (0.010)	1.03*** (0.011)	0.454*** (0.010)
February	-0.146*** (0.008)	-0.176*** (0.008)	-0.162** (0.008)
March	0.151*** (0.007)	0.101*** (0.007)	0.054*** (0.007)
Audit × April	0.012 (0.031)	0.036 (0.028)	0.026 (0.031)
Audit × May	-0.011 (0.030)	-0.041 (0.028)	0.036 (0.029)
Audit × June	-0.044 (0.031)	-0.041 (0.028)	0.012 (0.032)
Audit × July	0.074** (0.035)	0.061* (0.031)	0.044 (0.032)
Audit × August	-0.141*** (0.035)	-0.124*** (0.030)	-0.066** (0.032)
Audit × September	-0.096*** (0.028)	-0.058** (0.025)	-0.031 (0.024)
Audit × October	-0.038 (0.028)	-0.002 (0.025)	-0.022 (0.023)
Audit × November	-0.059** (0.026)	-0.060** (0.024)	-0.004 (0.021)
Audit × December	-0.078*** (0.027)	-0.051** (0.022)	-0.034 (0.023)
Audit × January	0.104** (0.042)	0.136*** (0.049)	0.0005 (0.044)
Audit × February	0.032 (0.037)	0.002 (0.038)	0.021 (0.037)
Audit × March	0.048 (0.035)	0.041 (0.035)	0.015 (0.032)
Observations	998,640	998,640	998,640
Municipalities	4,161	4,161	4,161
R ²	0.630	0.637	0.520

All models include municipality-year fixed effects, month fixed effects, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. *p<0.05; **p<0.01; ***p<0.001.

J Alternative specifications

This appendix shows that results on political bureaucratic cycles for hires are robust to the following alternative specifications: using different transformations of the dependent variable (dropping observations where the unlogged outcome equals zero, taking the inverse hyperbolic sine transformation, or transforming employment outcomes into a binary measure of whether the count is larger than zero), omitting the lagged dependent variable, using municipality and year fixed effects instead of interactive fixed effects, clustering standard errors at the municipality and year level, clustering standard errors at the municipality and month level, using unbalanced panels, and omitting years with federal and state elections.

Figure 18: Political bureaucratic cycles in hires, by contract type
Log, without adding 1

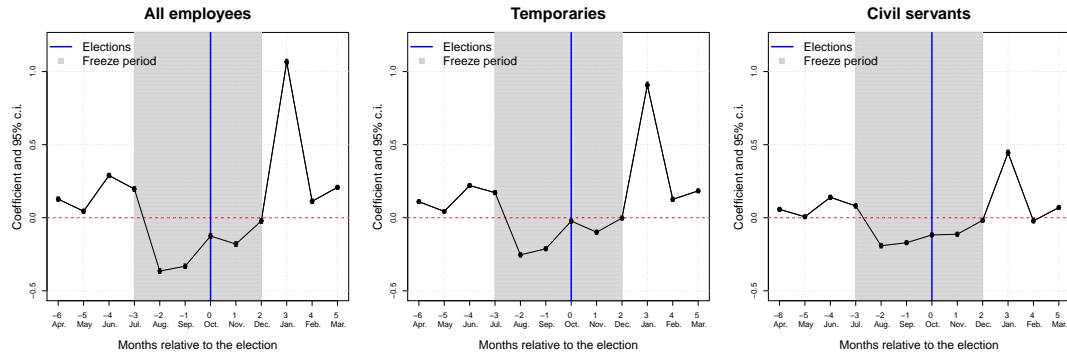
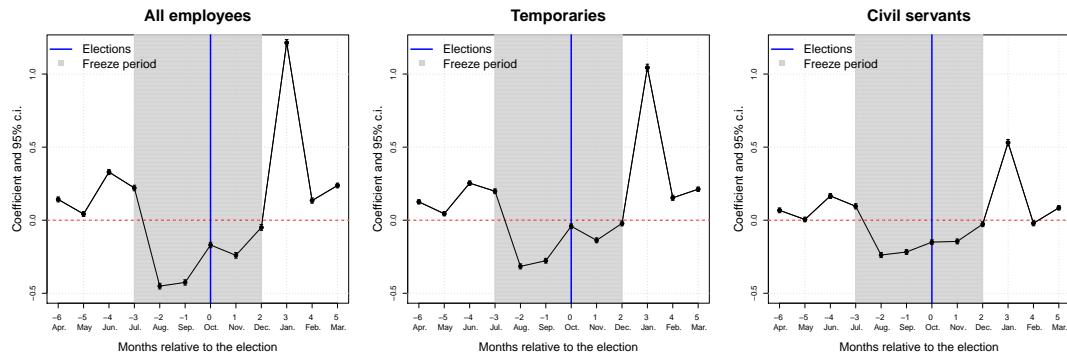


Figure 19: Political bureaucratic cycles in hires, by contract type
Inverse hyperbolic sine transformation



Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1.

Figure 20: Political bureaucratic cycles in hires, by contract type
 Binary measure of whether there are any hires

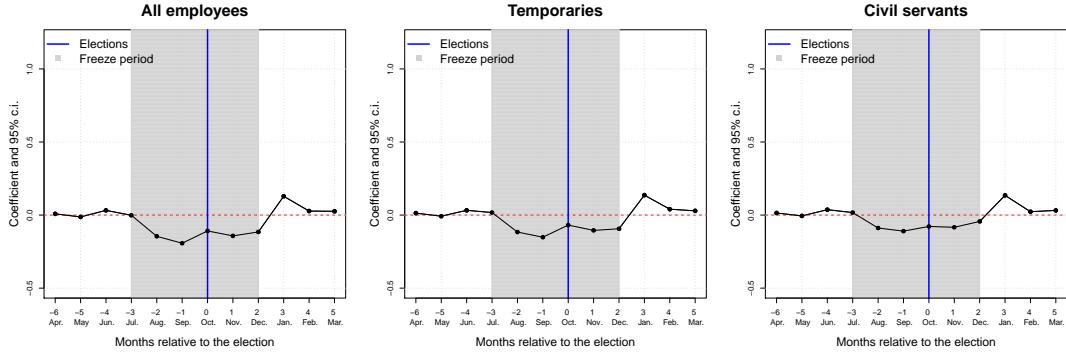


Figure 21: Political bureaucratic cycles in hires, by contract type
 Omitting the lag of the dependent variable

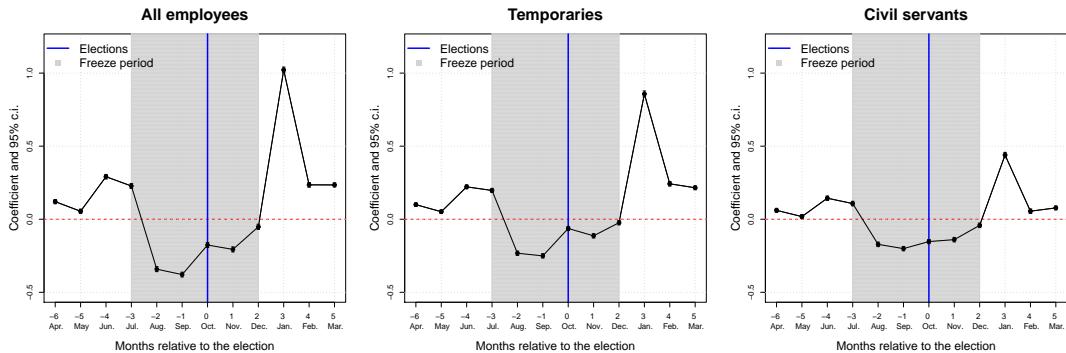
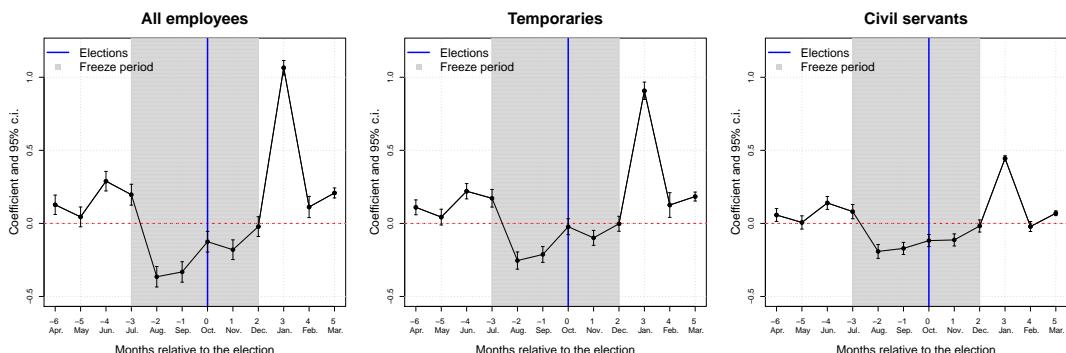


Figure 22: Political bureaucratic cycles in hires, by contract type
 Standard errors clustered by municipality and by month



Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1.

Figure 23: Political bureaucratic cycles in hires, by contract type
 Standard errors clustered by municipality and by year

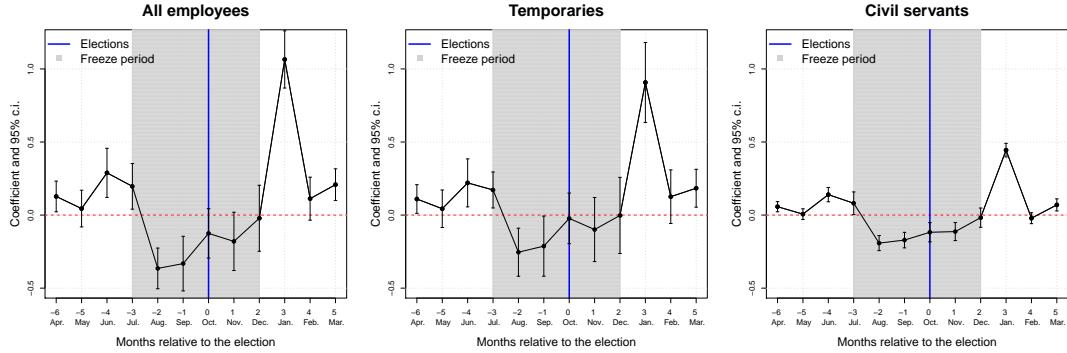


Figure 24: Political bureaucratic cycles in hires, by contract type
 Two-way fixed effects

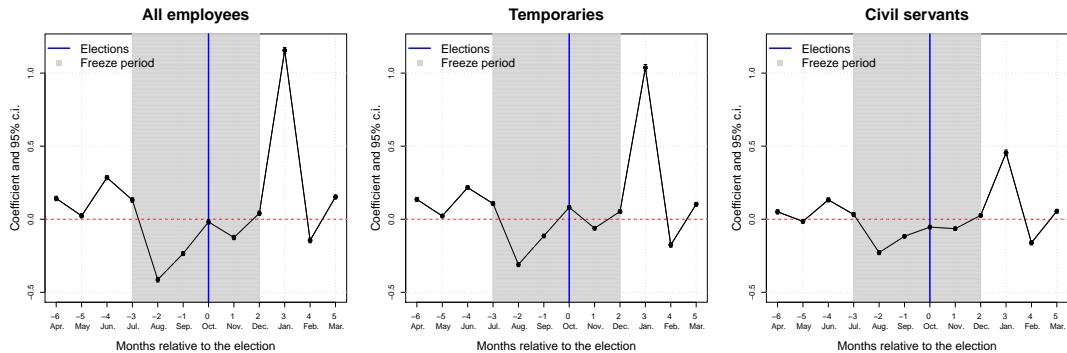
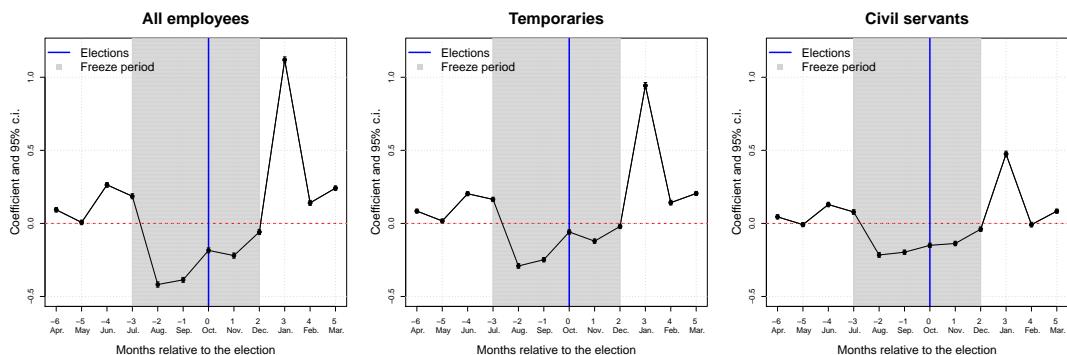
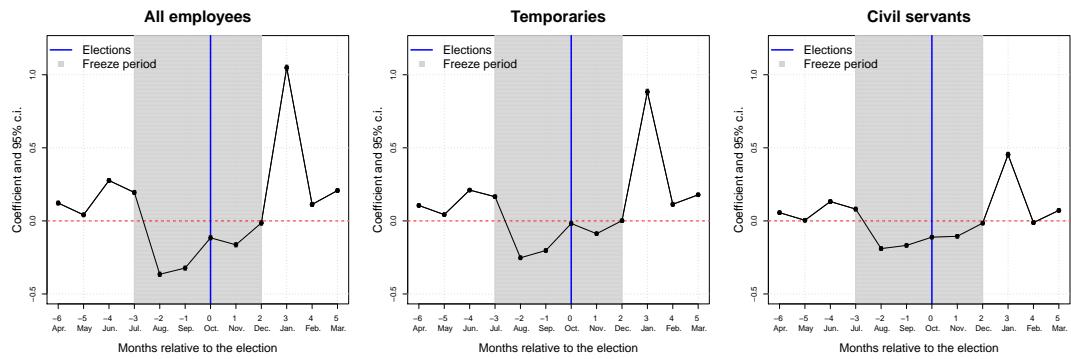


Figure 25: Political bureaucratic cycles in hires, by contract type
 Omitting years with state and federal elections



Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1.

Figure 26: Political bureaucratic cycles in hires, by contract type
Unbalanced panels



Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1.