Can anti-corruption policies curb political budget cycles?
Evidence from public employment in Brazil*

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Abstract

A vast literature on political cycles has shown that politicians often manipulate policy tools ahead of elections to win votes. Yet much less is known about the effects of policies designed to constrain these cycles. I argue that legal constraints on politicians' discretion over tools like spending, debt, transfers, or hires ahead of elections simply displace –and can even exacerbate– such cycles. I demonstrate these unintended consequences using large, monthly panels of Brazilian municipalities to measure cycles in public employment. Federal laws ban hiring and firing bureaucrats in a 6-month period around elections. Consistent with politicians anticipating and strategically responding to these constraints, hiring decreases during this freeze period, but increases in the months before the ban. Cycles are not restricted to political advisers or to temporary employees – they are present across all levels of the bureaucracy and in the civil service too. These patterns are even more pronounced in localities that experience a randomized anti-corruption audit. These findings highlight how the effectiveness of anti-corruption strategies can be undermined by politicians' strategic responses to them.

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For decades, political scientists and economists have studied how politicians' manipulation of policies in the run-up to elections creates cycles in economic outcomes or government policy. The basic insight of this literature is that incumbent politicians change policy right before elections to increase their chances of staying in office, thus leading to economically suboptimal policy choices that un-smooth government spending and economic activity (Nordhaus, 1975). This generates what are called political business cycles (related to economic output, employment, and inflation) or political budget cycles (related to government policy tools like public spending, intergovernmental transfers, or government debt). Franzese (2002) and Dubois (2016) review this extensive literature, and Philips (2016) reports the results of a meta-analysis.

While this established research agenda has accumulated a significant body of formal models and empirical results demonstrating that politicians often manipulate policy ahead of elections –at least when they have the incentives and ability to do so (Alt and Rose, 2009)–, much less is known about the effectiveness of policies designed to contain these cycles. Politicians' discretion over policy tools is often constrained by laws that seek to curb electoral cycles. For example, Peru's fiscal prudence law establishes stricter limits on spending and deficit for the first semester of an electoral year, and Colombia's electoral law forbids most private sector procurement in the 4 months leading to elections. Do these limits successfully constrain electoral cycles in policy tools?[1]

In contrast to previous research, this article emphasizes anti-cyclical policies constraining governments' discretion around elections and politicians' strategic responses to them. I argue that, especially in contexts where electoral incentives are strong, politicians anticipate their election-related uses of public resources in response to constraints around the electoral calendar. These strategic responses to anti-corruption policies lead to a displacement, rather than an elimination or attenuation of electoral cycles.

Empirically, I focus on political bureaucratic cycles, namely electoral cycles in public employment. Focusing how hiring changes around elections has three main advantages. First, public employment is one of the largest spending categories for governments around the world. On average, the government payroll comprises a quarter of government spending (International Monetary Fund, 2016). Second, the political use of public employment is ubiquitous (Grindle, 2012), and politicians can use hiring towards a variety of political objectives, including mobilizing voters,

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[1]While previous studies have examined how general fiscal rules (such as budget balance rules) dampen political budget cycles (Rose, 2006; Gootjes et al., 2021), we lack systematic evidence of how rules specific to the electoral calendar impact cycles.

rewarding supporters, and controlling the bureaucracy (Toral, 2020). These two features imply that electoral cycles in public employment are likely to have important effects in 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. The third advantage is that focusing on the bureaucracy allows us to examine individual employment decisions, and to examine how cycles differ across job levels (from street-level to managerial positions) and contract types (temporary and civil service).

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. Brazilian laws prohibit hiring and firing employees in the period from 3 months before to 3 months after elections. This policy is designed to protect fiscal discipline and level the playing field between incumbent candidates and their challengers. I exploit the exogenous timing of elections (which are held every 4 years on the first Sunday of October) and the 6-month "freeze period" during which politicians are generally not allowed to hire or fire to identify political bureaucratic cycles. I use large panels of roughly 1 million municipality-month observations, with month fixed effects (to control for seasonality) and municipality-year fixed effects (to finely control for local conditions) to measure how public employment systematically fluctuates around elections.

The results demonstrate that hiring decreases during the freeze period, in compliance with the legal constraints on hiring around elections. However, hiring expands before and after the freeze period. This is consistent with politicians responding strategically to the combination of electoral incentives and legal constraints, and simply moving election-related hiring up rather than refraining from it. Hiring follows similar cyclical patterns for low-skilled, professional, and managerial positions. While these patterns are more pronounced for temporary hiring, I find similar patterns for civil service hires. This finding suggests that civil service hiring is not be as insulated from political influence as is commonly assumed, and draws attention to the relevance of politicians' discretion over the timing of civil service hires.

Heterogeneity analyses provide additional evidence about the electoral rationale of these cycles. First, the pre-electoral expansion of hiring is more pronounced in localities that are electorally more competitive. Second, municipalities with a larger legislature where (as a result of coalitional dynamics) mayors have additional incentives for distributing patronage (Britto and Fiorin, 2020; Mignozzetti et al., 2022) also exhibit stronger increases in hiring ahead of the election. To identify exogenous variation in legislature size I exploit a sudden policy change (driven by a court ruling)

that imposed an arbitrary population-based cutoff to determine the size of municipal legislatures.

Additional heterogeneity analyses provide further evidence of the effect of anti-corruption policies in shaping political bureaucratic cycles. Cycles have intensified over time, as rules have become stricter and their enforcement has strengthened. Second, municipalities randomly exposed to federal anti-corruption audits, which increase the salience and improve the enforcement of rules (Avis et al., 2018), further boost these employment cycles, both further depressing hiring during the freeze period and boosting it immediately beforehand.

Together, these results suggest that, rather than eliminating cycles, legal constraints on policy tools displace –and even exacerbate– them. This paper thus highlights how the effectiveness of policies designed to curb political budget cycles can be undermined by politicians' strategic responses to them. In so doing, the findings in this paper contribute to an emerging literature on the costs of policies that seek to control corruption by constraining politicians' or bureaucrats' discretion (Rich, 2022; Wang, 2022; Jiang et al., 2022; Gerardino et al., 2022).

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 demonstrating that these constraints displace –rather than eliminate– cycles, this paper draws attention to politicians' strategic responses to anti-cyclical policies and the unintended consequences of constraints. In a second contribution, by leveraging month-level panels and uncovering detailed patterns of expansion and contraction in public employment and services during the electoral calendar, this paper draws attention to the month-to-month temporal dynamics of electoral cycles, which year- or quarter-level analyses tend to obscure. Third, by exploiting randomly assigned anti-corruption audits and exogenously determined variation in legislature size, this paper identifies the causal effect of context on cycles. These quasi-experimental strategies improve on previous strategies to measure the effect of context on cycles, which typically rely on splitting the sample or interacting time periods with endogenous covariates (De Haan and Klomp, 2013).

Theory

I argue that legal constraints that limit politicians' discretion over policy tools (such as spending, government debt, or public employment) near elections displace, rather than eliminate, political

budget cycles. As a result of politicians' strategic responses to them, these legal constraints seeking to protect fiscal discipline or electoral fairness shape and potentially lengthen political budget cycles.

Following a fiscal or an electoral rationale, countries around the world have established legal constraints on politicians' discretion over policy tools around elections.[2] Table 1 presents some examples from a diverse sample of Latin American countries with regards to a variety of policy tools. For example, Colombia's electoral law makes it illegal for governments to engage in procurement with the private sector (with few exceptions) in the 4 months leading to elections. In Peru, the fiscal prudence law establishes stricter limits on spending and deficit for the first semester of an electoral year. These kind of policies are not unique to Latin America. In the Philippines public works are banned 45 days before the election, and in Spain the electoral law forbids inaugurating any public works during the campaign.[3]

Table 1: Examples of policies constraining politicians' discretion on policy tools around elections in a sample of Latin American countries

Policy tool	Country	Legal constraint	Period	Legal instrument
Deficit	Peru	No more than forecast deficit	Last 6 months of mandate	Law 27245 (1999)
Procurement	Colombia	No direct purchases	4 months before the election	Law 996 (2005)
Advertisement	Bolivia	No government ads in the media	30 days before the election	Law 026 (2010)
Inaugurations	Guatemala	No inauguration of public works	During the campaign	Decree 1-85 (1986)
Transfers	Brazil	No inter-governmental transfers	3 months before the election	Law 9504 (1997)
Employment	Uruguay	No hiring of civil servants	Last 12 months of mandate	Law 16127 (1990)
	Colombia	No hiring or firing	4 months before the election	Law 996 (2005)
	Brazil	No hiring, firing, or transfers	Last 6 months of mandate	Law 9504 (1997)

Public employment is a key instrument for electioneering by incumbents. In fact, scholars have identified political cycles in public employment in settings as diverse as US states (Cahan, 2019) and municipalities in Indonesia (Pierskalla and Sacks, 2019), Greece (Chortareas et al., 2017), the Philippines (Labonne, 2016) and Finland and Sweden (Dahlberg and Mörk, 2011).

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, which have fewer private sector job opportunities than wealthier contexts, public jobs constitute a valuable political currency.[4] Governments in less developed contexts also have less state capacity to distribute other

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[2]These rules limit incumbents' ability to do things. A separate type of policies limit their ability to speak about them, or to take credit for them (Bueno, 2021).

[3]Philippines' Omnibus Electoral Code of 1985 and Spain's Law of the General Electoral Reform of 1985, as reformed in 2011, respectively.

[4]In fact, in the developing world government jobs enjoy a significant premium over comparable private

benefits (such as human development services or infrastructure works) that voters may value but require more planning, capacity, and coordination with other actors such as higher levels of government or private firms. Government jobs are particularly useful ahead of elections because they are targetable and (when temporary) reversible (Robinson and Verdier, 2013), which helps solve the double credibility issue of clientelistic exchanges (Stokes et al., 2013).

Politicians' ability to influence public employment also tends to be higher in developing contexts, where civil service systems are generally less consolidated. Temporary contracts allow more discretion over hiring, thus facilitating the use of public employment for electoral purposes. While civil service rules constrain politicians' discretion, they often do not do so completely: politicians may not be able to choose *who* gets a civil service job, they can often determine *how many* civil servants are hired, and when. This counters the common assumption that civil service hiring is insulated from political influence.

Given the political value of public employment and politicians' discretion over hiring and firing, several countries restrict governments' discretion to hire or fire bureaucrats in the lead-up to elections (Table 1). For example, Uruguay forbids hiring bureaucrats in the last year of an incumbent's mandate. In Brazil, both the electoral and the fiscal rules restrict hiring in the 6-month period around elections. Again, these laws are also found in other regions. In the Philippines, the hiring and promotion of bureaucrats is forbidden starting 45 days before an election.[5] In Pakistan, the Electoral Commission temporarily banned hiring ahead of the 2018 elections.

I argue that these temporal limits on politicians' discretion over policy tools around elections displace, rather than eliminate, the cycles they seek to curb. Where electoral incentives are sufficiently strong, politicians will respond to these constraints by anticipating the expansion in policy tools such as spending, debt, or hiring, rather than refraining from them. This builds on a key insight from Rogoff, who in an early contribution to the formal study of cycles 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).[6] Rogoff argued that "in practice, an 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"

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sector jobs (Finan et al., 2017).
    [5]Philippines' Omnibus Election Code (1985).
    [6]Tufte (1978, 149) put forward a similar idea.

([Rogoff](), [1990](), 31).

This paper develops and tests this hypothesis through an empirical study of political cycles in public employment in the Brazilian case. This is a context in which politicians' discretion over policy is severely constrained by a well-established civil service system ([Cortázar Velarde et al.](), [2014]()), by legal limits on hiring and firing around elections, and by strong anti-corruption institutions that enforce these rules.

Institutional setting

Brazilian local governments are an ideal setting in which to examine political bureaucratic cycles and the success of legal constraints at curbing them. Elections are held on a fixed schedule, bureaucracies are relatively large, and politicians have some discretion over public employment. Yet, multiple laws constrain the exercise of such discretion around elections and strong anti-corruption institutions enforce such laws.

Brazil has 5,570 municipalities,[7] most of which are small and poor.[8] Local elections are held every 4 years on the first Sunday of October.[9] State and federal elections are held every 4 years on a separate calendar, 2 years before and after municipal elections. Local elections are generally competitive; almost half (about 49%) of the incumbents who ran in 2016 were defeated.

Mayors are elected through a majoritarian system,[10] and since 1997 are only allowed to run for re-election once. City councilors, in turn, are elected though a proportional, open-list system. Mayors depend on the city council to pass laws, including their budget. In most cases, mayors' parties do not control the council, and therefore mayors need to build and sustain legislative coalitions ([Frey](), [2022]()). As a result, they often use bureaucratic appointments to buy legislature support

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[7]During the period I study, the number of municipal governments ranged 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.

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

[9]The rule was established in 1997, and has applied to all elections since then, with the exception of 2020 when elections were postponed until mid-November because of the COVID-19 pandemic.

[10]Municipalities 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.

(Mignozzetti et al., 2022; Kim, 2020).

The size of local city councils depends on the municipality's population. The 1988 constitution mandated that municipalities have a number of city councilors proportional to its population, but allowed significant discretion for local governments to choose that number within broad brackets. In 2004, following a court case that made it to the Supreme Court, the Supreme Electoral Court established deterministic population cutoffs for each additional city councilor. In the new system, municipalities with less than 47,620 residents were to elect 9 councilors in 2004 whereas those above that threshold would elect 10 councilors.[11] Using a regression discontinuity design, previous studies have shown that one additional city councilor leads to higher levels of corruption (Britto and Fiorin, 2020) and to more hiring (Mignozzetti et al., 2022), arguably because of the heightened bargaining costs for the mayor (Frey, 2022).

Municipal governments have a relatively large workforce because they are responsible for providing primary services in healthcare, education, and social assistance. In 2016, the average municipal government hired 4.9% of the local population and 38.2% of those employed in the formal labor market. Municipal employees enjoy a wage premium relative to the private sector (Colonnelli et al., 2019, 3090). 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 roughly 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 lifetime tenure after a probationary period.[12] Critically, however, the best performers are not automatically appointed. While politicians have no discretion over candidates' ranking, they can decide on the timing and number of civil service hires.

About a third of municipal employees are hired on temporary contracts,[13] which can legally be used to hire political appointees or fill short-term or urgent staffing needs. Temporary employees generally have 1-year contracts (often running until the end of December), which the government

[11]From there, municipalities would add one additional city councilor for every 47,619 residents, up to cities with one million residents.
[12]Tenured civil servants can only be dismissed in extraordinary circumstances such as being convicted of corruption.
[13]I use the term temporary to refer to all non-civil-service contracts. These contracts use a variety of labor regimes, all without tenure.

can terminate much more easily than civil service contracts. In practice, temporary contracts are sometimes used where civil service contracts should be signed instead. Yet this practice is unconstitutional and politicians may be prosecuted for it.

Three Brazilian laws limit the hiring and firing of bureaucrats around elections, creating a 6-month freeze period starting 3 months before the election and lasting until the end of the mayor's term (Figure 1).[14] The 1997 Electoral Law forbids hiring, firing, or transferring bureaucrats 3 months before and after an election[15] to protect candidates' equality of opportunity. The 2000 Fiscal Responsibility Law prohibits personnel expenses from increasing during the 180 days before the end of a government's mandate, i.e., roughly 3 months before and after an election. This provision has the goal of limiting the fiscal impact of governments' electioneering through public employment. Finally, the 1990 Ineligibilities Law prevents public employees from running for office; candidates must take paid leave (if they are tenured) or leave their job (if non-tenured) 3 months before the election.[16]

Figure 1: Timeline of election cycles in Brazil
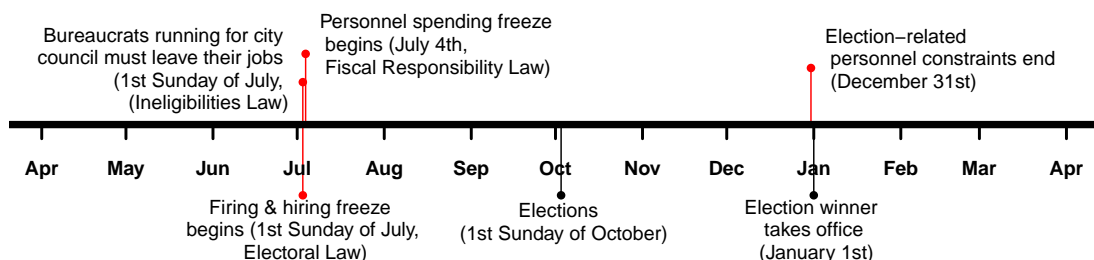
Politicians can be prosecuted for deviating from these legal rules on public employment. If found guilty, they are subject to important penalties, including the loss of their post, having their political rights suspended, substantive fines, and even imprisonment.[17] Prosecution of politicians for corruption charges is not rare. Lambais and Sigstad (2021) estimate that about 7.7% of mayoral election winners or runner-ups are involved in a court case accused of corruption charges. Bento et al. (2021) document 1,716 court cases involving mayors and former mayors between 1992 and 2016 in the state of Rio Grande do Sul (497 municipalities). Additional evidence of the prosecution

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[14]See Appendix A for additional details on legal constraints.

[15]The law allows for hiring, firing or transferring of positions of trust, and the hiring of civil servants who had previously passed competitive exams.

[16]For some positions, they are required to exit 6 months before the election.

[17]See Appendix A.5 for details on the legal penalties for breaches of public employment laws, as detailed in the Constitution, the Administrative Impropriety Law, the Electoral Law, and the Penal Code.

of politicians for violations of employment rules comes from an original dataset of corruption news reports. I scraped all news in the websites of the Prosecutor's Office of the largest states in Brazil's most populous regions (São Paulo in the southeast and Bahia in the northeast, with 645 and 417 municipalities respectively) and found 76 reports from 2013 to 2022 mentioning former mayors and related to violations of public employment laws.

Another key accountability institution overseeing municipal governments is Brazil's federal comptroller's office (CGU, *Controladoria-Geral da União*). CGU has long targeted its audits through lotteries: a team of federal auditors visits randomly selected municipalities to review local spending of federal transfers. 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 are prosecuted (Avis et al., 2018).

Research design

I exploit the exogenous timing of local elections to identify political bureaucratic cycles in Brazilian municipalities. By examining long panels of municipality-month data, covering multiple election cycles, and using thousands of fixed effects to finely control for seasonality and local conditions, I estimate how public employment varies in the months before and after 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, with the following estimating equation:

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

Y_{iym} is a given outcome (for example, the number of hires) corresponding to municipality i in year y in month m. Since outcomes are right-skewed count variables,[18] I use the log on both the dependent variable and its lag, after adding 1 to keep observations with zeroes. α_{iy} are municipality-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,

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[18]Appendix D reports descriptive statistics of the outcomes by month.

which control for monthly shocks common to all municipalities and thus account for underlying seasonality in outcomes. D_{iym}^p is an indicator for whether observation iym is p months away from a municipal election, where p ranges 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 M, I show that my 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, clustering standard errors by municipality and year, using unbalanced panels, and omitting years in which federal and state elections were held.

To identify heterogeneity in political bureaucratic cycles, I expand Equation 1 by adding a binary covariate and interacting it with the month fixed effects and with the election cycle period indicators, using 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 belongs to a subgroup of interest (e.g., those exposed to a federal anti-corruption audit) in year y. Where K_{iy} is exogenous (as in the case of the randomized audits), the δ^p coefficients identify the heterogeneity in the cycles caused by that covariate. Where K_{iy} is not exogenous (e.g., an indicator for more competitive municipalities), the δ^p coefficients simply describe how the cycles differ between the two groups. This specification uses two-way rather than interactive fixed effects because K_{iy} only varies at the municipality-year level.

To identify how cycles vary with legislature size, I restrict the sample to observations between 2005 and 2008, namely the term for which the Supreme Electoral Court's ruling about legislature size first applied. In 2009, a constitutional amendment changed the rules about the relationship between population and legislature size, establishing maxima to replace the Supreme Electoral Court's deterministic rule. Using this sample, I apply the following estimating equation:

$$Y_{iym} = \lambda_y + \theta_m + \sum_{p=-6}^{5} \beta^p D_{iym}^p + \left(\zeta + \phi_m + \sum_{p=-6}^{5} \delta^p D_{iym}^p \right) L_i \qquad (3)$$

$$+ \pi P_i + \rho L_i P_i + \gamma Y_{iym-1} + \varepsilon_{iym}$$

L_{iy} corresponds to an indicator for municipalities assigned to having one additional city councilor based on the first population threshold at 47,620 residents.[19] Following the logic of regression discontinuity designs (Cattaneo et al., 2019), in this model I also include as controls the municipality's population, recentered around the cutoff (P_i), and its interaction with the indicator for localities above the threshold. Critically for the validity of this design, the distribution of population figures is continuous around the threshold (Appendix C) and no other policies kick in at that threshold (Eggers et al., 2018). The δ^p coefficients thus identify how cycles vary as a result of having one more city councilor. This design drops the municipality fixed effects because they are perfectly collinear with population, which acts as the regression discontinuity's forcing variable. As in all other models, I cluster standard errors at the level of the municipality. I do not further restrict the sample by population, but results are similar when excluding municipalities with more than 95,238 residents (that is, above the cutoff at which one additional seat is added).

Data

I leverage administrative data on public employment, population, 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 required to report all their contracts to the Ministry of the Economy every year.[20] RAIS therefore contains data on the universe of municipal employees, including contract

[19]I focus on the first discontinuity for two reasons. First, it concentrates most municipalities in Brazil; over 95% of them had less than 95,238 residents in 2003. Second, at the first cutoff is where the increase in the size of the legislature (from 9 to 10 seats) is larger in relative terms, and thus where the change in mayors' bargaining costs is most notable.

[20]Appendix B reports additional details of the labor dataset.

type, start and end dates, salary, reason for termination, and professional category, among other variables.[21] Using RAIS, I generate counts of hires, dismissals,[22] and other employment outcomes, by type of contract, for each municipality and each month.

To determine municipalities' assigned number of legislators for the term 2005-2008, I use the official population statistic for 2003 produced by Brazil's Institute for Geography and Statistics. These are the figures that the Supreme Electoral Court mandated were to be used when determining the number of city councilors to be elected in each municipality in the 2004 election. Since the ruling was published in 2004, municipalities could not have manipulated their population to select into the group with less or more city council seats.

To examine whether the increased salience and enforcement of rules shapes cycles, I leverage data from the CGU on the randomly assigned audits conducted between 2006 and 2015.[23] I define municipalities as exposed to an audit from the year they were selected until 3 years later, i.e. for a 4-year period (the length of a mayoral term).[24] These audits are highly salient at the local level and have been shown to have real consequences for political elites, including a higher probability of incurring legal actions or being the target of a police crackdown (Avis et al., 2018). They can therefore be seen as an enforcement shock that increases the salience and the enforcement of anti-corruption rules.

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[21]As shown in Appendix B, a small number of municipalities (between 1 and 10 percent) do not report having any employees in a given year during my study period. Municipalities that fail to report employment data to the government are generally smaller, poorer, and less developed. The analyses presented in this paper are therefore not representative of the whole country but of municipalities that reported data to RAIS every year from 2000 to 2019. This selection plausibly biases the results towards zero, since poorer and less developed municipalities –where the clientelistic use of public employment is more common, and bureaucracies are smaller and less professionalized– are likely to experience more pronounced cycles.

[22]I consider dismissals to be contract terminations initiated by the employer (*exonerações a iniciativa do empregador*); resignations are terminations initiated by the employee (*exonerações a pedido*).

[23]In 2016 the CGU started targeting some audits by criteria other than audit; it does not report which municipalities were selected via lottery and which were not.

[24]The results are similar if I consider them exposed to an audit for 8 years, or for the whole period since they were selected via lottery.

Results

The regression results demonstrate that the hiring of bureaucrats displays marked cyclical patterns consistent with politicians responding to both the electoral incentives and the legal constraints around hiring. When discussing these results, I focus mostly on the pre-electoral period. The patterns after the election are partly driven by the result of the election, which I explore using a close-races regression discontinuity design in a separate paper (Toral, 2022).

Hiring declines during the freeze period, but expands before it – even in the civil service

Figure 2 depicts how the hiring of municipal employees fluctuates in the months around elections, compared to the same months in non-election years. These results, detailed in Table 2, are consistent with hiring cycles 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 closely with politicians, such as managers, advisors, or assistants. Similar patterns are observed for low-skilled employees and professionals (Appendix E).

Hiring increases in the pre-freeze period, compared to the same time period in non-election years. For example, in June of an election year, the hiring of employees is 33.5% higher than in June of a non-election year ($p < 0.001$).[25] Hires on July 1 are not covered by the freeze period unless the day falls on a Sunday, which is likely why we observe that the expansion of hiring persists in July, with 21.71% more hires than in that same month in a non-election year ($p < 0.001$).[26] The expansion of hiring before the freeze period is consistent with politicians anticipating the legal constraints limiting their discretion to hire and fire.

Hiring decreases during the freeze period, which is consistent with politicians responding to the legal constraints on hiring around elections. For example, hires are 30.5% less common in August of an election year, compared to a non-election year ($p < 0.001$).[27] This decline is more pronounced before an election, and less pronounced in the last quarter of the year, when the incidence of hiring

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[25]On average, there are 12.09 hires in the month of June in a non-electoral year.
[26]On average, there are 14.03 hires in the month of July in a non-electoral year.
[27]On average, there are 16.63 hires in August in a non-electoral year.

Figure 2: Political bureaucratic cycles in hires, by contract type
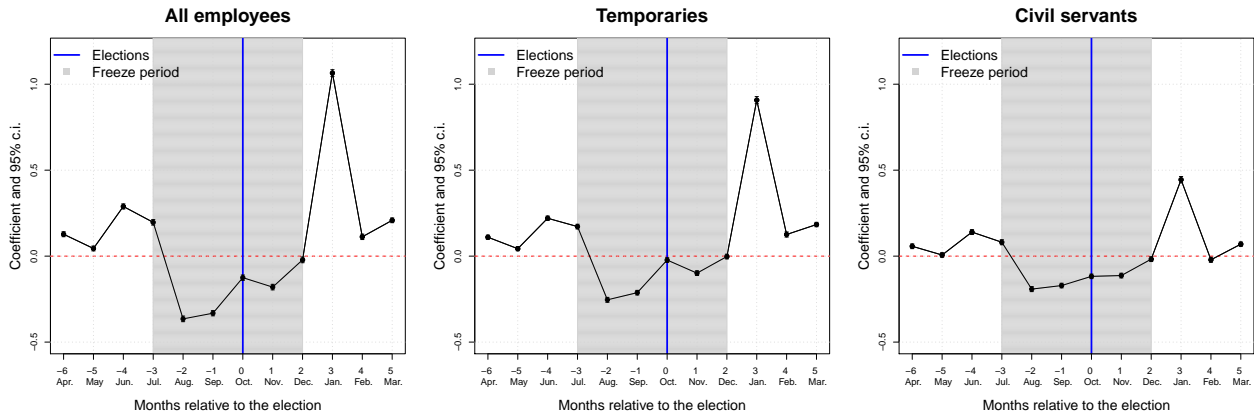
Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1.

is generally lower.[28]

Hiring expands significantly after the election-related constraints on hiring are lifted. The effect is most pronounced in January, once the freeze is over: we observe 190% more hires compared to a January that does not follow local elections ($p < 0.001$). This effect is particularly pronounced considering that in January of a non-electoral year hires are quite common (41.59 in the average municipality), since many temporary contracts start in that month. The post-election expansion in the bureaucracy continues after January. For example, there are 23.12% more hires in March after an election than in the same month in non-election years.[29] The large expansion in hiring after the freeze period is likely driven by two mechanisms that coincide in time: the end of the election-related constraints and the beginning of a new political mandate.

The panels in the center and right of Figure 2 (and the corresponding columns in Table 2) demonstrate that there is significant heterogeneity in the intensity of the cycles by contract type. Unsurprisingly, the cycles are more pronounced for temporary contracts, over which politicians have more discretion. However, there are also cycles in the hiring of civil servants. For example, civil service hiring is 15.01% greater in June, 17.45% lower in August, and 55.98% higher in January around an election compared to the same months in a regular year ($p < 0.001$).[30]

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

[29]There are on average 31.33 hires in March in non-election years.

[30]Civil service hiring is much less common. On 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***	0.110***	0.057***
	(0.007)	(0.006)	(0.007)
May	0.044***	0.043***	0.007
	(0.007)	(0.006)	(0.007)
June	0.289***	0.220***	0.140***
	(0.008)	(0.007)	(0.007)
July	0.196***	0.172***	0.081***
	(0.008)	(0.007)	(0.007)
August	-0.365***	-0.254***	-0.192***
	(0.008)	(0.007)	(0.007)
September	-0.332***	-0.213***	-0.171***
	(0.008)	(0.007)	(0.007)
October	-0.125***	-0.023***	-0.118***
	(0.008)	(0.007)	(0.007)
November	-0.180***	-0.099***	-0.113***
	(0.008)	(0.007)	(0.007)
December	-0.022**	-0.002	-0.018**
	(0.008)	(0.007)	(0.007)
January	1.07***	0.907***	0.445***
	(0.010)	(0.011)	(0.009)
February	0.112***	0.126***	-0.021***
	(0.008)	(0.008)	(0.007)
March	0.208***	0.184***	0.070***
	(0.007)	(0.007)	(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. $^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

The existence of political cycles in the hiring of civil servants has important research and policy implications. In cases like Brazil, the civil service limits (or removes) politicians' discretion in the targeting of jobs, but it does not eliminate their power over the quantity and 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 empirically examine whether such hiring is as insulated from political influence as is typically assumed.

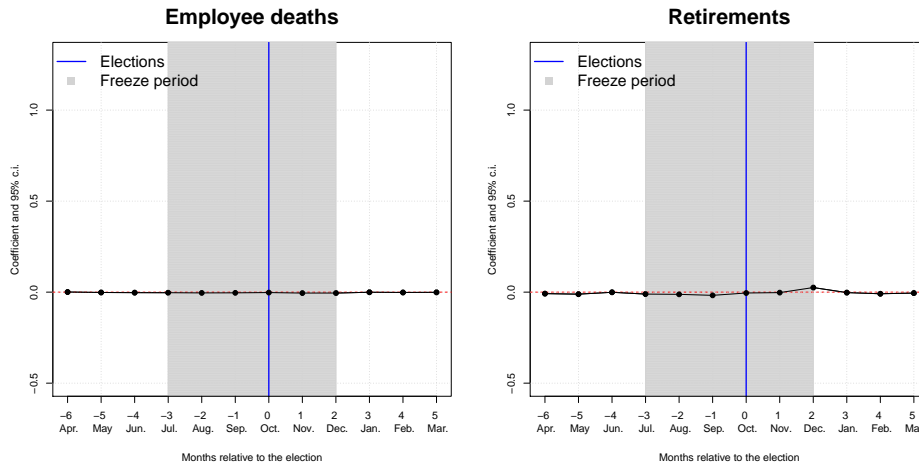Figure 3: Political bureaucratic cycles in resignations, by contract type
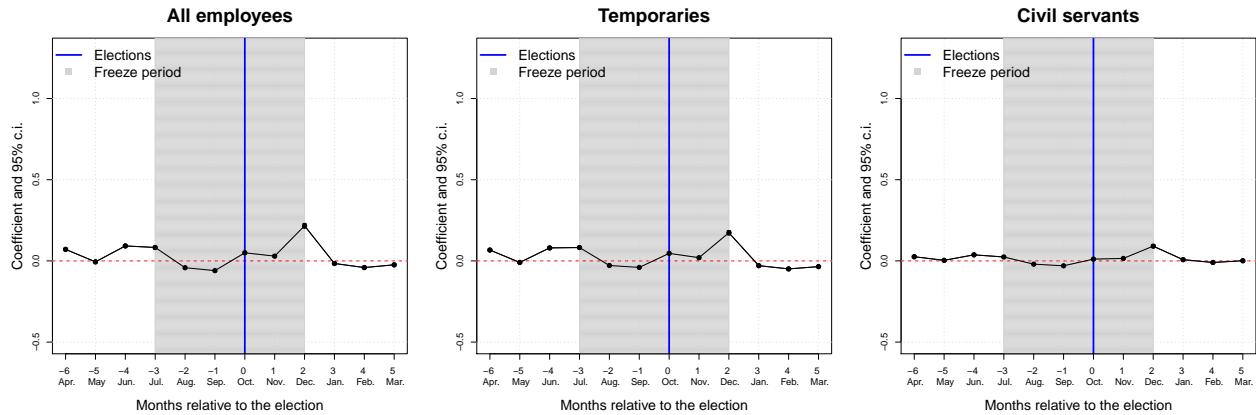
Points and their confidence intervals (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1.
Regression details are in Appendix F.

The pre-freeze period is also characterized by an uptick in resignations, as shown in Figure 3.[31] For example, resignations are 9.65% higher in June of an election year than they are in a non-election year ($p < 0.001$). This increase in resignations is likely due to the legal requirement that bureaucrats who are running for office resign 3 or 6 months before the October election, depending on their post. Still, resignations are rare. In June of a non-electoral year, there are on average 0.57 resignations. Therefore, these outflows of experienced bureaucrats can only explain drive a small part of the increase in hires documented in Figure 2.

One potential concern with these results is that they may be driven by cycles not in public employment itself but record-keeping by bureaucrats. To address this concern, I run a placebo test where I examine cycles in two outcomes that are reported in the same dataset but should not be impacted by the electoral calendar – employee deaths and retirements. As shown in Figure 4, these two variables barely oscillate around elections compared to the same months in non-election 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.

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[31] The regression table is in Appendix ??.

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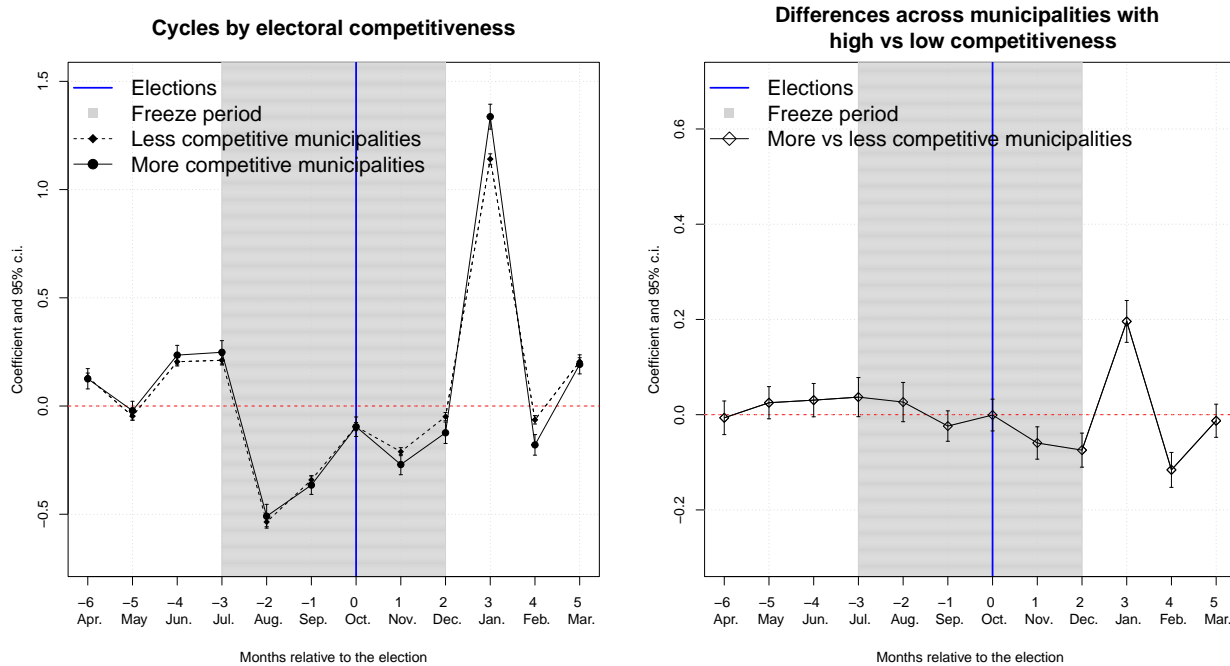

Points and their confidence intervals (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1. Regression details are in Appendix G.

Cycles are more pronounced where the political incentives to engage in patronage are stronger

Heterogeneity analyses support the idea that these cycles are partly driven by political incentives. First, cycles are more pronounced in localities where the previous election had higher levels of electoral competitiveness. I measure electoral competitiveness by the concentration of mayoral votes in the previous election, and define more competitive municipalities as those where the electoral fragmentation is in the lowest quartile. As shown in Figure 5, localities where the previous election was more competitive appear to have a stronger pre-electoral expansion of hiring. This difference is marginally insignificant at conventional levels (the p-values for the coefficients of June and July are 0.09 and 0.08, respectively), yet the direction and magnitude of the coefficients suggests that when the incumbents were elected in a more competitive race they make more use of political bureaucratic cycles.

Whereas measuring heterogeneity in political cycles by interacting period dummies with endogenous covariates is standard in the literature (De Haan and Klomp, 2013), empirical results of the sort presented in Figure 5 may be driven by confounders. For example, localities with more competitive elections may be systematically different on a number of observable and unobservable characteristics that could shape political bureaucratic cycles.

Figure 5: Political bureaucratic cycles in total hires, by level of electoral competitiveness in the previous election
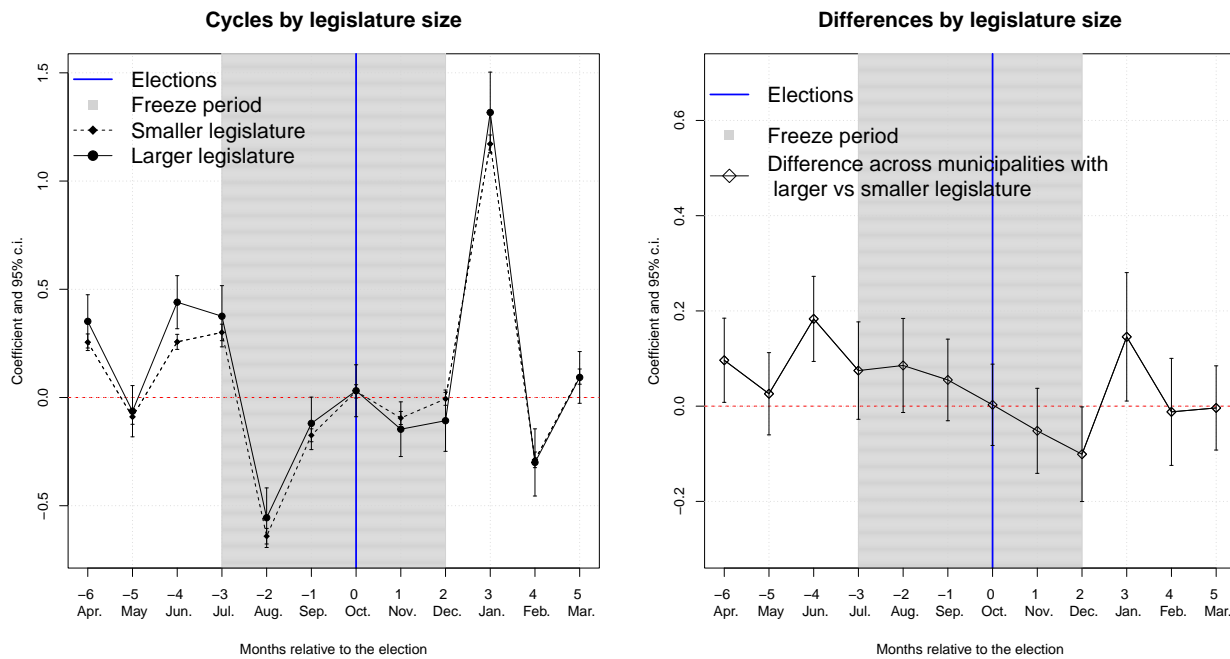

In the plot on the left, points and their confidence intervals (c.i.) correspond to $\hat{\beta}$ coefficients (triangles) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients (squares and circles) in Equation 2. In the plot on the right, points and their c.i. correspond to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

To causally identify the effect of political incentives on cycles, I make use of the 2004 Supreme Electoral Court ruling that established deterministic population cutoffs for selecting the size of municipal legislatures. Restricting the sample to observations between 2005 and 2008 (when this rule applied), we can examine how municipalities mandated to elect 9 councilors (those with less than 47,620 residents) compared to those mandated to elect more.

Figure 6 presents the results of this quasi-experimental strategy to measure heterogeneity in cycles. Consistent with a larger legislature increasing the mayor's bargaining costs (Britto and Fiorin, 2020; Frey, 2022), electoral cycles are stronger in municipalities assigned to elect more than 9 city councilors. In particular, in June of an electoral year these cities expand hiring by an additional 19.7% ($p < 0.001$) compared to those with a smaller legislature and relative to June of a non-electoral year. These results build on the findings of (Mignozzetti et al., 2022), who find that adding one city councilor causes an increase in political appointments, and suggest that political

Figure 6: Political bureaucratic cycles in total hires, by size of the legislature (2005-2008)

In the plot on the left, points and their confidence intervals (c.i.) correspond to $\hat{\beta}$ coefficients (triangles) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients (squares and circles) in Equation 3. In the plot on the right, points and their c.i. correspond to $\hat{\delta}$ coefficients in Equation 3. Regression details are in Appendix J.

incentives are an important driver of political bureaucratic cycles.

Stronger law enforcement is associated with further declines in hiring during the freeze period as well as increases before it

Two additional pieces of evidence lend support to the hypothesis that the shape of the cycles (an 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 on hiring around elections.

First, cycles in hiring have intensified over time. Figure 7 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 elections[32] 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.[33] 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 2010s, when the salience and enforcement of these rules increased, we observe more intense cycles in hiring.

Figure 7: Political bureaucratic cycles in total hires, by decade (1995-2019)
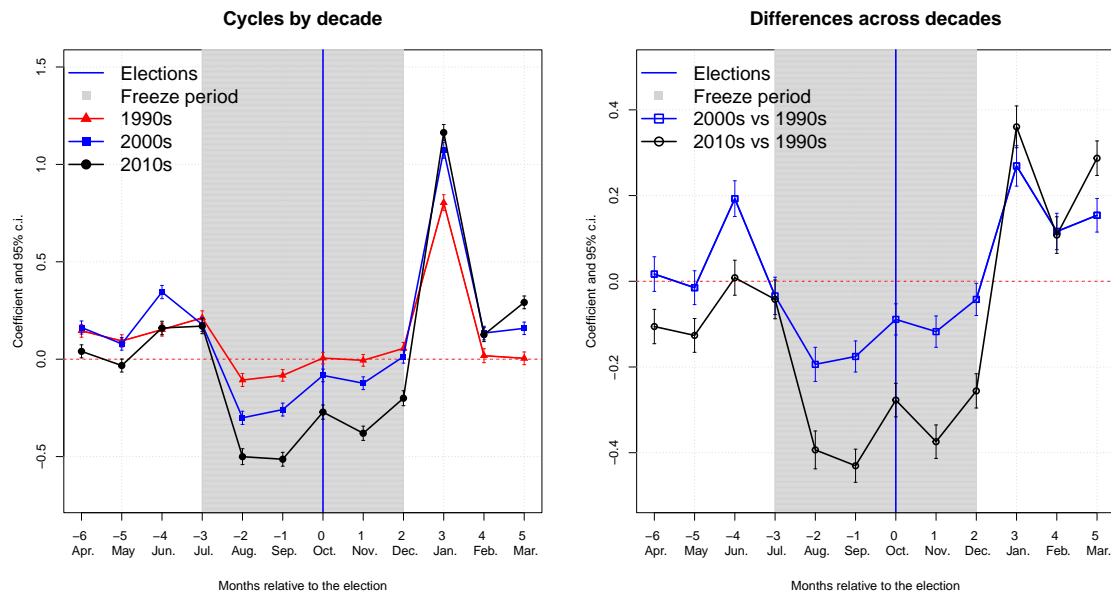
In the plot on the left, points and their confidence intervals (c.i.) correspond to $\hat{\beta}$ coefficients (triangles) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients (squares and circles) in Equation 2. In the plot on the right, points and their c.i. correspond to $\hat{\delta}$ coefficients in Equation 2.
Regression details are in Appendix K.

Again, heterogeneity across time may be partly driven by other factors. To get at causal evidence of the impact of enforcement on political bureaucratic cycles I exploit the CGU randomized audits. While rules apply homogenously to all municipalities *de jure*, their enforcement likely

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[32]The 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.

[33]Although 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 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.

varies with oversight. The randomized federal government audits can be seen as an enforcement shock that not only uncovers potential irregularities in the use of public funds, but also provides information and incentives that strengthen the work of other accountability actors (including the local opposition, the media, and the prosecutors' office).

Figure 8 shows that randomized anti-corruption audits intensify political bureaucratic cycles in hiring – with more pronounced increases in hiring before and after the freeze period, and more pronounced decreases during the freeze period. For example, audits trigger 7.65% more hires in June of an electoral year compared to municipalities not exposed to an audit ($p < 0.05$), and lead to a 13.14% larger decrease in hiring during the freeze period ($p < 0.001$). In January, after the freeze, hiring expands 11% more in audited than in unaudited municipalities ($p < 0.05$). These patterns are consistent with the legal constraints on hiring around elections (and their enforcement) displacing and shaping –rather than eliminating– political bureaucratic cycles.

Figure 8: Political bureaucratic cycles in total hires, by exposure to an anti-corruption audit
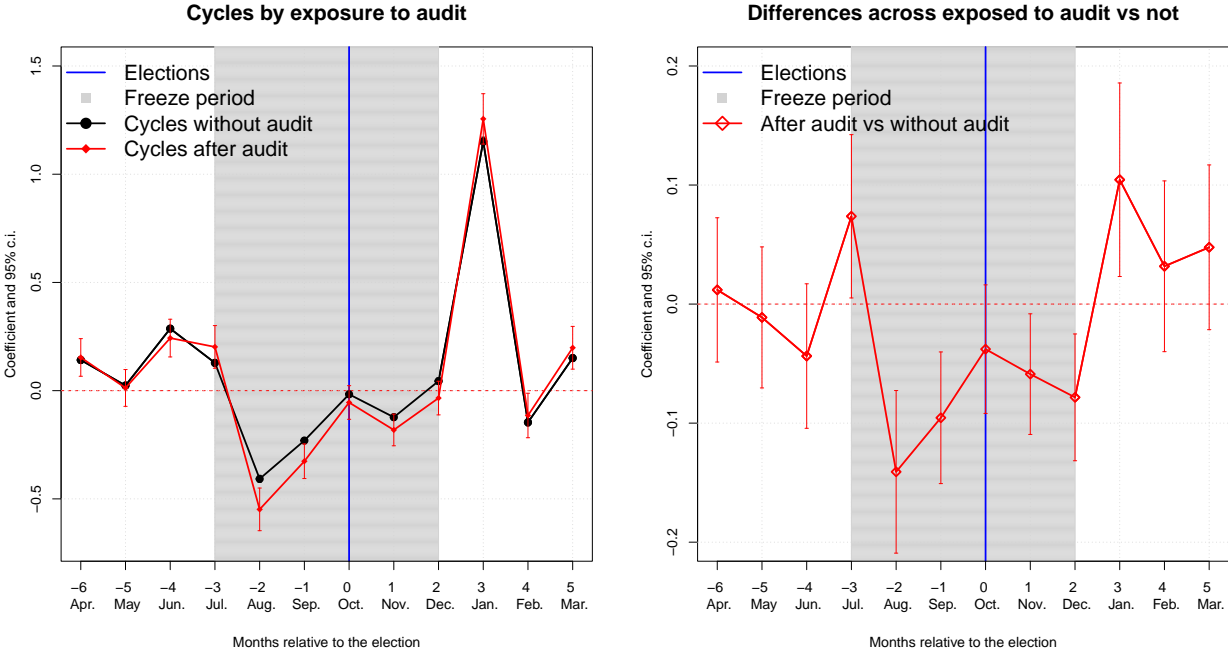
In the plot on the left, points and their confidence intervals (c.i.) correspond to $\hat{\beta}$ coefficients (dots) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients (diamonds) in Equation 2. In the plot on the right, points and their c.i. correspond to $\hat{\delta}$ coefficients in Equation 2. Regression details are in Appendix L.

All in all, these results demonstrate that in Brazilian municipalities, hiring follows marked cycli-

cal patterns around elections: it decreases in the 6-month window around elections and increases both before and after this period, compared to the same months in non-election years. These patterns are consistent with politicians anticipating the legal constraints on their discretion. Cycles have intensified 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.

Conclusion

A vast literature in political science and economics has shown that politicians often manipulate policy in the run-up to elections to improve their chances of re-election. From a democratic standpoint, these patterns are problematic because they imply that incumbents abuse their control over the government during electoral campaigns, which damages political competition. From an economic 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 enact laws to constrain incumbents' discretion over policy tools in the months leading up to an election. Following either a fiscal or electoral rationale, these laws typically establish freeze periods around elections that severely constrain politicians' discretion.

This paper advances our understanding of political cycles and of the impact of these anti-cyclical policies by empirically analyzing political bureaucratic cycles in Brazilian municipalities. Leveraging detailed administrative datasets released by the federal government, I build panels of roughly 1 million municipality-month observations. Exploiting the exogenous timing of elections and the bans on hiring and firing employees during a 6-month window around elections, I examine how public employment fluctuates around elections.

The results demonstrate that while hiring decreases during the freeze period (in line with the legal constraints), it also increases before and after. This suggests that politicians respond strategically to both electoral incentives and legal constraints: 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 that civil service hiring is 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 the rules and their enforcement have intensified) and are significantly more marked in municipalities that are randomly subject to an anti-corruption audit. Together, these results demonstrate that, rather than eliminating cycles, legal constraints on policy tools displace and even exacerbate them.

The paper makes three theoretical and empirical contributions to the literature on political cycles. First, it advances our understanding of the temporal dynamics of cycles by highlighting how incentives and constraints vary throughout the electoral calendar, and by using monthly-level administrative data (rather than yearly or quarterly panels, which are more typically used in the literature). 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 are even intensified by those constraints. Third, the paper advances our understanding of how context shapes political budget cycles by exploiting quasi-experimental designs rather than simply interacting electoral periods with endogenous covariates.

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Appendices

A   Additional details on legal constraints around elections

A.1   Rules in the Federal Constitution concerning civil service and temporary hiring

Brazil's Federal Constitution (promulgated on October 5, 1988) includes several rules constraining politicians' discretion over public employment.[34] Article 37.II mandates that hiring be made through civil service exams (*concurso público*), and that those who are approved in an exam be given priority for hiring. At the same time, it allows for the hiring of public employees under temporary contracts, be it for management and leadership positions, or in cases of "temporary need based on extraordinary public interest" (article 37.IX).

A.2   Rules in the Fiscal Responsability Law concerning personnel expenses

The Fiscal Responsibility Law (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.[35] 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,[36] or get credit or guarantees (article 23). Fifth, up to 30 days after the end of every quadrimestre the government must issue a

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[34]The constitution can be found at http://www.planalto.gov.br/ccivil_03/constituicao/constituicao.htm.

[35]The Fiscal Responsibility Law can be found at http://www.planalto.gov.br/ccivil_03/leis/lcp/lcp101.htm.

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

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).

The Fiscal Responsibility Law also forbids, during the last 8 months of the mayor's mandate, entering into any spending obligation that cannot be paid in full by the end of the year, or that has any installments to be paid in the following year unless the municipal government has sufficient cash to do so (article 42). Considering that personnel expenses are by the largest spending category, this rule further constraints politicians' discretion over public employment during the election year.

A.3 Rules in the Electoral Law concerning the hiring and firing of bureaucrats around elections

Brazil's Electoral Law (Law 9,504, approved on September 30, 1997)[37] 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. 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.[38] There are exceptions for dismissing employees in 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. 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).

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[37]The Electoral Law can be found at http://www.planalto.gov.br/ccivil_03/leis/l9504.htm.
[38]A similar provision existed since the military dictatorship, as per Law 6,091, approved on August 15, 1974. The law is available at https://www.planalto.gov.br/ccivil_03/leis/l6091.htm.

A.4 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),[39] 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.

A.5 Legal rules on penalties for breaches of public employment laws

The Federal Constitution establishes a strong basis for prosecuting politicians who break the rules concerning public employment. In its Article 37.4, it establishes that "acts of administrative impropriety will imply the suspension of political rights, the loss of public service, the unavailability of assets and reimbursement to the public purse, in the form and gradation provided for by the laws, without prejudice to the appropriate criminal prosecution."

The Administrative Impropriety Law (Law 8,429, approved on June 2, 1992) includes important penalties for decisions that intentionally hurt public finances, illicitly increase leaders' wealth, or deviate from the principles of honesty, impartiality, or legality.[40] Penalties include 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 when in office.

The Penal Code (Decree-Law 2,848, approved December 7, 1940) includes penalties for ordering expenses not authorized by law (e.g., the kinds of personnel expenses forbidden by the Fiscal Responsibility Law).[41] In particular, those are subject to between 1 and 4 years in prison (article

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[39]The Law of Ineligibilities can be found at http://www.planalto.gov.br/ccivil_03/leis/lcp/lcp64.htm.

[40]The Administrative Impropriety Law can be found at http://www.planalto.gov.br/ccivil_03/leis/l8429.htm.

[41]The Penal Code can be found at http://www.planalto.gov.br/ccivil_03/decreto-lei/del2848compilado.htm.

359-D). The same penalty applies for increases in personnel expenses in the last 180 days of the mayor's mandate (article 359-G).

The Electoral Law establishes a number of strong penalties for deviations from its rules, including fines (to be paid by the candidate and/or their party), the suspension of the electoral candidacy of those benefited by the decision, and the loss of access to the party financing system.

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.[42] Descriptive statistics for the data on municipal employees are reported in Table 3. 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.[43] 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.[44]

Municipal governments (like all formal employers) are legally required[45] to report data for all its employees[46] 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 problems of capacity and 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,[47] and compare them to all 5,569 municipalities.[48] 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 biases the results. This bias, however, is likely to be in the direction of

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

[43]This 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*).

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

[45]Entities 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.

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

[47]Results are similar when analyzing the municipalities not reporting data in 2004.

[48]I exclude Brasília because it does not have a municipal government.

attenuating results (i.e. bringing them closer to zero) because cycles are arguably more pronounced in municipalities not submitting contract data to RAIS. In any case, results are not representative of the overall population of municipalities, but rather of those complying with the RAIS reporting requirement.

Table 3: 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 9: Socioeconomic characteristics of municipalities not reporting employment data in 2016

All municipalities ⋯⋯ Municipalities not in RAIS 2016
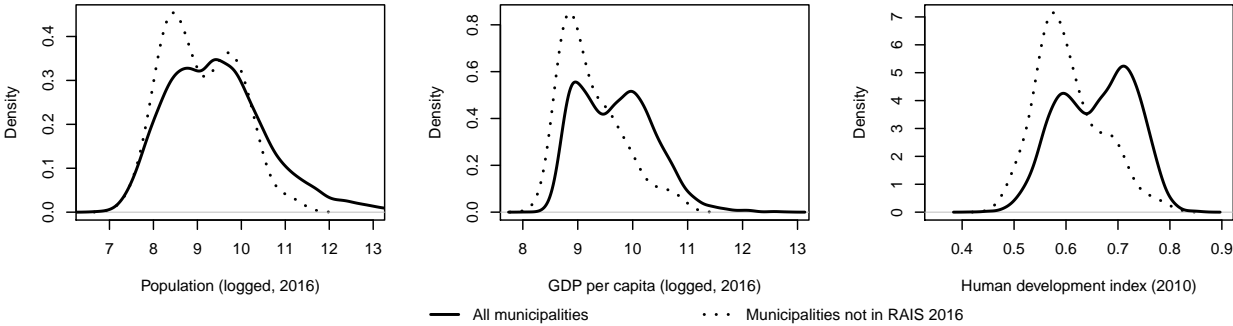
C Continuity of municipal population around the threshold

Figure 10: McCrary density test for the continuity of municipal population count for 2003 around the threshold of 47,619 residents
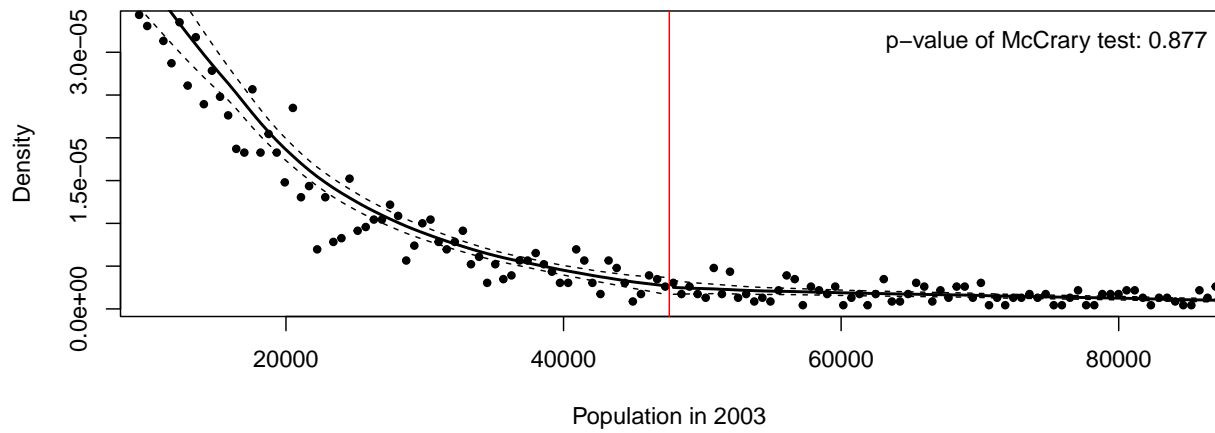
D Distributions of outcome data

Figure 11: Distribution of hires, dismissals, and resignations, by month
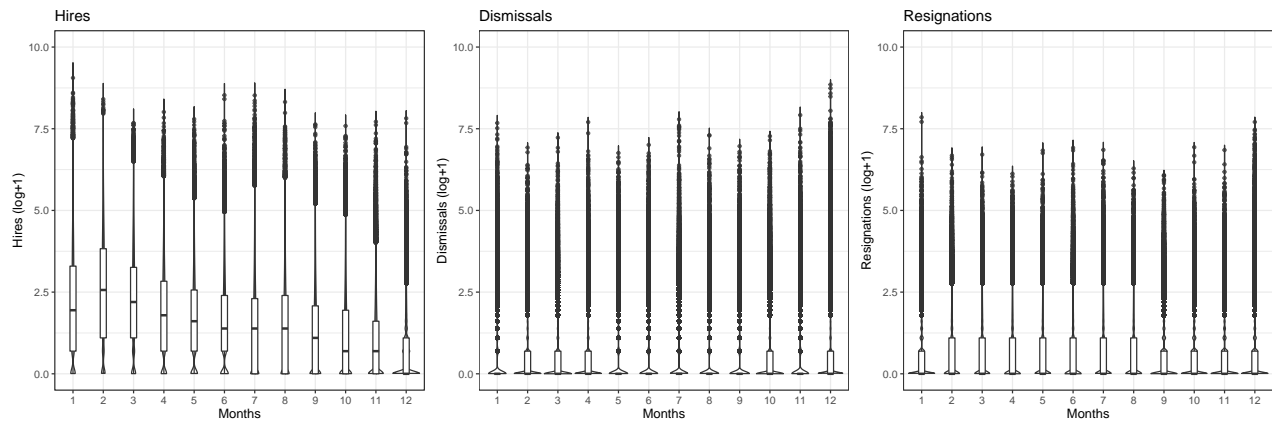
Horizontal lines correspond to the median. Boxes cover the interquartile range.

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 12: Political bureaucratic cycles in hires: Managers (CBO 1)
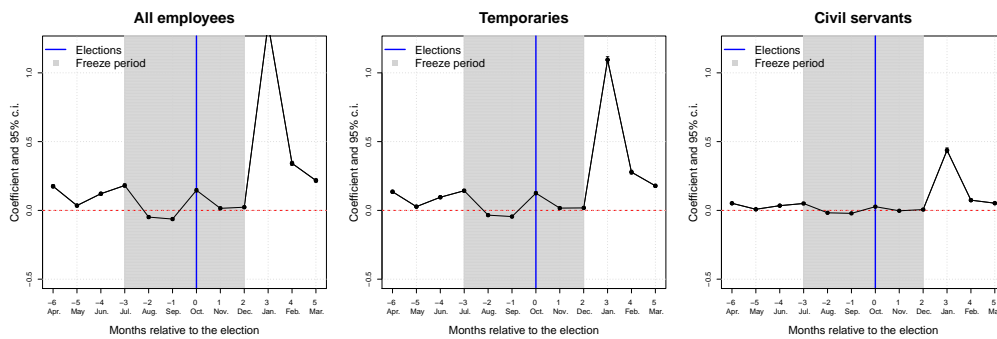
Figure 13: Political bureaucratic cycles in hires: Professionals (CBO 2 or 3)
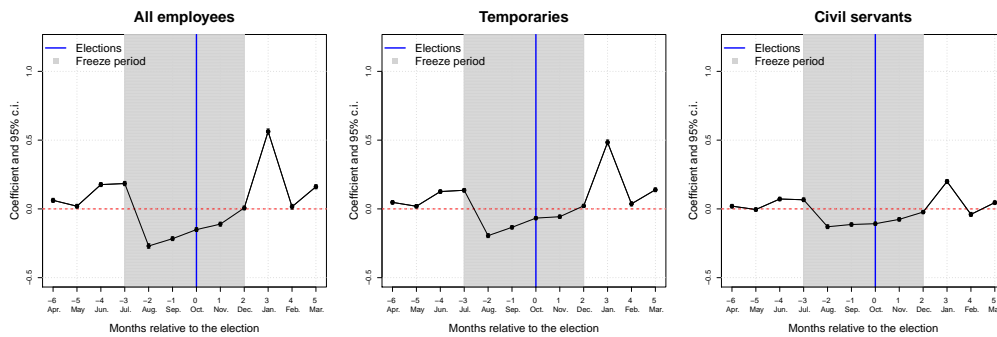
Figure 14: Political bureaucratic cycles in hires: Low-skill employees (CBO 4+)
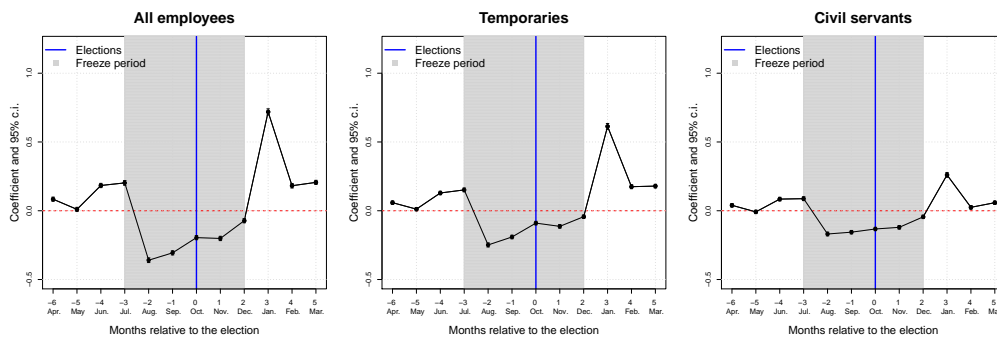
Points and their confidence intervals (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1.

F Political bureaucratic cycles in resignations

Table 4: Political bureaucratic cycles in resignations, by contract type

|	Total (1)	Temporaries (2)	Civil servants (3)
April	0.071***	0.067***	0.026***
	(0.004)	(0.004)	(0.003)
May	-0.006	-0.009***	0.003
	(0.004)	(0.004)	(0.003)
June	0.092***	0.080***	0.037***
	(0.004)	(0.004)	(0.003)
July	0.083***	0.082***	0.024***
	(0.005)	(0.004)	(0.004)
August	-0.042***	-0.028***	-0.020***
	(0.004)	(0.004)	(0.003)
September	-0.060***	-0.040***	-0.030***
	(0.004)	(0.004)	(0.003)
October	0.050***	0.046***	0.011***
	(0.005)	(0.004)	(0.004)
November	0.030***	0.020***	0.015***
	(0.005)	(0.004)	(0.003)
December	0.216***	0.173***	0.091***
	(0.008)	(0.007)	(0.005)
January	-0.016***	-0.029***	0.008**
	(0.005)	(0.004)	(0.003)
February	-0.041***	-0.049***	-0.010***
	(0.004)	(0.004)	(0.003)
March	-0.024***	-0.035***	0.001
	(0.004)	(0.003)	(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. $^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

G   Political bureaucratic cycles in placebo outcomes

Table 5: Political bureaucratic cycles in placebo outcomes: Employee deaths and retirements

	Deaths (1)	Retirements (2)
April	0.0007	-0.008***
	(0.002)	(0.003)
May	-0.002	-0.011***
	(0.002)	(0.003)
June	-0.003	-0.0008
	(0.002)	(0.003)
July	-0.003	-0.011***
	(0.002)	(0.003)
August	-0.004**	-0.012***
	(0.002)	(0.003)
September	-0.004**	-0.018***
	(0.002)	(0.003)
October	-0.002	-0.005
	(0.002)	(0.003)
November	-0.005***	-0.003
	(0.002)	(0.003)
December	-0.005**	0.025***
	(0.002)	(0.003)
January	-0.0006	-0.003
	(0.002)	(0.003)
February	-0.002	-0.009***
	(0.002)	(0.003)
March	-0.001	-0.005*
	(0.002)	(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. *$p<0.05$; **$p<0.01$; ***$p<0.001$.

H Political bureaucratic cycles in dismissals

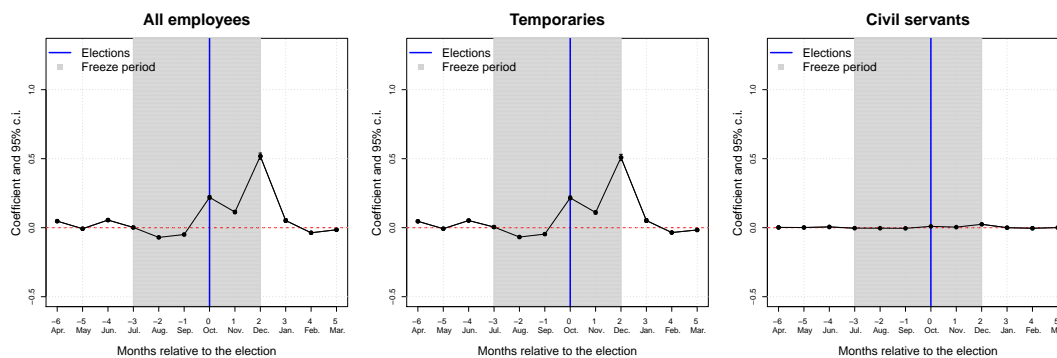Figure 15: Political bureaucratic cycles in dismissals, by contract type

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

Table 6: Political bureaucratic cycles in dismissals, by contract type

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

I Political bureaucratic cycles in hires, by level of electoral competitiveness

Table 7: Political bureaucratic cycles in hires by decade and by contract type

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.133***	0.119***	0.055***
	(0.010)	(0.009)	(0.010)
May	-0.047***	-0.042***	-0.043***
	(0.010)	(0.008)	(0.009)
June	0.205***	0.153***	0.098***
	(0.010)	(0.009)	(0.010)
July	0.211***	0.163***	0.073***
	(0.011)	(0.010)	(0.010)
August	-0.536***	-0.421***	-0.281***
	(0.011)	(0.010)	(0.010)
September	-0.341***	-0.200***	-0.158***
	(0.009)	(0.008)	(0.008)
October	-0.095***	0.026***	-0.086***
	(0.010)	(0.009)	(0.008)
November	-0.211***	-0.136***	-0.095***
	(0.009)	(0.008)	(0.008)
December	-0.049***	-0.038***	0.004
	(0.010)	(0.008)	(0.008)
January	1.14***	1.04***	0.446***
	(0.012)	(0.014)	(0.012)
February	-0.064***	-0.104***	-0.122***
	(0.010)	(0.010)	(0.009)
March	0.205***	0.142***	0.082***
	(0.009)	(0.009)	(0.009)
Competitive × April	-0.007	0.016	-0.020
	(0.018)	(0.017)	(0.019)
Competitive × May	0.025	0.027*	-0.0007
	(0.017)	(0.016)	(0.018)
Competitive × June	0.031*	0.028*	0.037**
	(0.018)	(0.016)	(0.018)
Competitive × July	0.037*	0.050**	0.042**
	(0.021)	(0.019)	(0.020)
Competitive × August	0.026	-0.056***	0.046**
	(0.021)	(0.019)	(0.019)
Competitive × September	-0.024	-0.065***	0.012
	(0.016)	(0.015)	(0.015)
Competitive × October	-0.0007	0.009	-0.003
	(0.017)	(0.016)	(0.015)
Competitive × November	-0.059***	-0.066***	-0.027*
	(0.017)	(0.015)	(0.015)
Competitive × December	-0.074***	-0.051***	-0.033**
	(0.018)	(0.015)	(0.015)
Competitive × January	0.196***	0.251***	0.035
	(0.022)	(0.027)	(0.025)
Competitive × February	-0.116***	-0.131***	-0.066***
	(0.019)	(0.019)	(0.020)
Competitive × March	-0.013	0.006	-0.017
	(0.018)	(0.018)	(0.018)
Observations	748,980	748,980	748,980
Municipalities	4,161	4,161	4,161
R^2	0.642	0.649	0.535

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

J Political bureaucratic cycles in hires, by legislature size

Table 8: Political bureaucratic cycles in hires by legislature size and by contract type

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.255***	0.186***	0.124***
	(0.020)	(0.018)	(0.019)
May	-0.089***	-0.086***	-0.088***
	(0.018)	(0.015)	(0.017)
June	0.257***	0.150***	0.149***
	(0.018)	(0.015)	(0.017)
July	0.301***	0.185***	0.155***
	(0.019)	(0.017)	(0.018)
August	-0.641***	-0.478***	-0.381***
	(0.018)	(0.016)	(0.017)
September	-0.175***	-0.099***	-0.092***
	(0.015)	(0.013)	(0.013)
October	0.028*	0.072***	-0.036***
	(0.016)	(0.014)	(0.014)
November	-0.095***	-0.087***	-0.046***
	(0.015)	(0.013)	(0.013)
December	-0.006	-0.043***	0.015
	(0.015)	(0.012)	(0.014)
January	1.17***	0.948***	0.484***
	(0.021)	(0.023)	(0.020)
February	-0.288***	-0.275***	-0.220***
	(0.018)	(0.017)	(0.017)
March	0.097***	0.061***	0.018
	(0.018)	(0.017)	(0.016)
Larger legislature × April	0.096**	0.148***	0.149***
	(0.045)	(0.046)	(0.055)
Larger legislature × May	0.026	-0.021	0.018
	(0.044)	(0.041)	(0.053)
Larger legislature × June	0.183***	0.142***	0.238***
	(0.046)	(0.043)	(0.054)
Larger legislature × July	0.075	0.121**	0.055
	(0.052)	(0.049)	(0.059)
Larger legislature × August	0.085*	-0.101**	0.074
	(0.050)	(0.050)	(0.053)
Larger legislature × September	0.055	-0.076*	0.100**
	(0.044)	(0.041)	(0.046)
Larger legislature × October	0.003	0.055	-0.031
	(0.044)	(0.044)	(0.043)
Larger legislature × November	-0.052	-0.068	0.009
	(0.045)	(0.042)	(0.044)
Larger legislature × December	-0.101**	-0.133***	0.017
	(0.051)	(0.046)	(0.042)
Larger legislature × January	0.146**	0.310***	0.017
	(0.069)	(0.081)	(0.079)
Larger legislature × February	-0.012	-0.034	-0.058
	(0.057)	(0.056)	(0.063)
Larger legislature × March	-0.004	-0.014	0.037
	(0.045)	(0.047)	(0.057)
Observations	199,728	199,728	199,728
Municipalities	4,161	4,161	4,161
R^2	0.551	0.577	0.453

These models include year fixed effects, month fixed effects (and their interaction with the larger legislature indicator), the municipality's population recentered around the threshold, its interaction with the larger-legislature indicator, the baseline larger legislature indicator, and a lag of the dependent variable. Municipality-clustered standard errors in brackets. *$p<0.05$; **$p<0.01$; ***$p<0.001$.

K 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***	0.083***	0.087***
	(0.017)	(0.013)	(0.015)
May	0.094***	0.041***	0.053***
	(0.016)	(0.013)	(0.013)
June	0.152***	0.086***	0.075***
	(0.017)	(0.013)	(0.014)
July	0.212***	0.122***	0.117***
	(0.018)	(0.015)	(0.015)
August	-0.107***	-0.076***	-0.057***
	(0.017)	(0.013)	(0.014)
September	-0.083***	-0.049***	-0.041***
	(0.015)	(0.012)	(0.012)
October	0.006	0.011	-0.005
	(0.015)	(0.012)	(0.013)
November	-0.006	-0.0006	-0.008
	(0.015)	(0.012)	(0.013)
December	0.056***	0.037***	0.019
	(0.016)	(0.012)	(0.013)
January	0.803***	0.519***	0.435***
	(0.021)	(0.019)	(0.018)
February	0.018	0.0001	-0.023
	(0.018)	(0.016)	(0.015)
March	0.004	0.036**	-0.027**
	(0.017)	(0.014)	(0.013)
2000s × April	0.017	0.019	0.030
	(0.021)	(0.016)	(0.018)
2000s × May	-0.015	0.0005	-0.011
	(0.020)	(0.016)	(0.017)
2000s × June	0.193***	0.141***	0.123***
	(0.021)	(0.017)	(0.018)
2000s × July	-0.034	-0.004	-0.011
	(0.022)	(0.018)	(0.019)
2000s × August	-0.194***	-0.130***	-0.112***
	(0.020)	(0.016)	(0.018)
2000s × September	-0.175***	-0.124***	-0.084***
	(0.018)	(0.015)	(0.016)
2000s × October	-0.089***	-0.037**	-0.067***
	(0.018)	(0.015)	(0.016)
2000s × November	-0.117***	-0.084***	-0.056***
	(0.019)	(0.016)	(0.016)
2000s × December	-0.042**	-0.042***	0.0009
	(0.019)	(0.015)	(0.017)
2000s × January	0.269***	0.373***	0.009
	(0.024)	(0.022)	(0.022)
2000s × February	0.116***	0.172***	-0.025
	(0.022)	(0.019)	(0.019)
2000s × March	0.154***	0.142***	0.037**
	(0.020)	(0.017)	(0.017)
2010s × April	-0.106***	-0.037**	-0.077***
	(0.020)	(0.017)	(0.019)
2010s × May	-0.126***	-0.061***	-0.065***
	(0.020)	(0.017)	(0.018)
2010s × June	0.008	0.041**	0.010
	(0.021)	(0.017)	(0.018)
2010s × July	-0.042*	0.046**	-0.043**
	(0.023)	(0.019)	(0.020)
2010s × August	-0.393***	-0.326***	-0.148***
	(0.022)	(0.019)	(0.019)
2010s × September	-0.430***	-0.339***	-0.185***
	(0.020)	(0.017)	(0.017)
2010s × October	-0.277***	-0.145***	-0.177***
	(0.020)	(0.017)	(0.017)
2010s × November	-0.374***	-0.268***	-0.188***
	(0.020)	(0.017)	(0.017)
2010s × December	-0.256***	-0.196***	-0.100***
	(0.020)	(0.017)	(0.018)
2010s × January	0.360***	0.571***	-0.017
	(0.025)	(0.024)	(0.024)
2010s × February	0.108***	0.147***	0.023
	(0.022)	(0.020)	(0.020)
2010s × March	0.287***	0.227***	0.135***
	(0.021)	(0.018)	(0.017)
Observations	963,300	963,300	963,300
Municipalities	3,211	3,211	3,211
R²	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.

L 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***	0.134***	0.050***
	(0.008)	(0.007)	(0.008)
May	0.024***	0.024***	-0.016**
	(0.007)	(0.006)	(0.007)
June	0.287***	0.218***	0.132***
	(0.007)	(0.006)	(0.007)
July	0.128***	0.104***	0.031***
	(0.008)	(0.007)	(0.007)
August	-0.408***	-0.305***	-0.225***
	(0.008)	(0.007)	(0.007)
September	-0.231***	-0.112***	-0.115***
	(0.007)	(0.006)	(0.006)
October	-0.017**	0.082***	-0.053***
	(0.007)	(0.006)	(0.006)
November	-0.123***	-0.059***	-0.065***
	(0.008)	(0.006)	(0.006)
December	0.044***	0.055***	0.028***
	(0.008)	(0.006)	(0.006)
January	1.15***	1.03***	0.454***
	(0.010)	(0.011)	(0.010)
February	-0.146***	-0.176***	-0.162***
	(0.008)	(0.008)	(0.008)
March	0.151***	0.101***	0.054***
	(0.007)	(0.007)	(0.007)
Audit × April	0.012	0.036	0.026
	(0.031)	(0.028)	(0.031)
Audit × May	-0.011	-0.041	0.036
	(0.030)	(0.028)	(0.029)
Audit × June	-0.044	-0.041	0.012
	(0.031)	(0.028)	(0.032)
Audit × July	0.074**	0.061*	0.044
	(0.035)	(0.031)	(0.032)
Audit × August	-0.141***	-0.124***	-0.066**
	(0.035)	(0.030)	(0.032)
Audit × September	-0.096***	-0.058**	-0.031
	(0.028)	(0.025)	(0.024)
Audit × October	-0.038	-0.002	-0.022
	(0.028)	(0.025)	(0.023)
Audit × November	-0.059**	-0.060**	-0.004
	(0.026)	(0.024)	(0.021)
Audit × December	-0.078***	-0.051**	-0.034
	(0.027)	(0.022)	(0.023)
Audit × January	0.104**	0.136***	0.0005
	(0.042)	(0.049)	(0.044)
Audit × February	0.032	0.002	0.021
	(0.037)	(0.038)	(0.037)
Audit × March	0.048	0.041	0.015
	(0.035)	(0.035)	(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. *$p<0.05$; **$p<0.01$; ***$p<0.001$.

M   Alternative specifications

This appendix shows that results on political bureaucratic cycles for hires are robust to the follwoing 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 16: Political bureaucratic cycles in hires, by contract type
Log, without adding 1
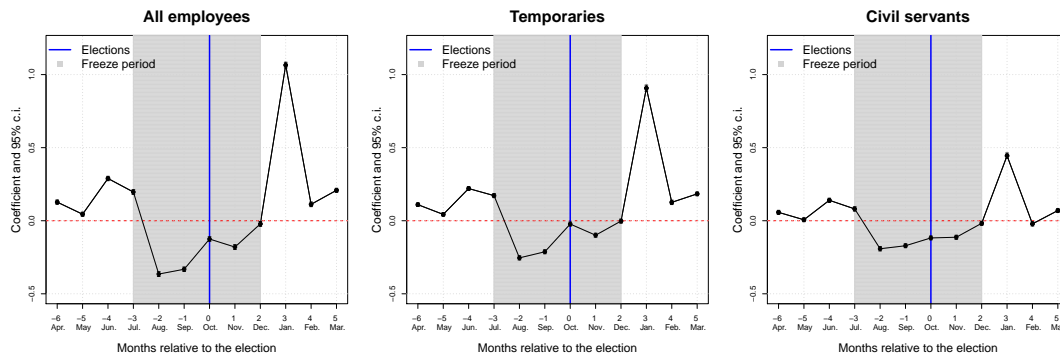
Figure 17: Political bureaucratic cycles in hires, by contract type
Inverse hyperbolic sine transformation
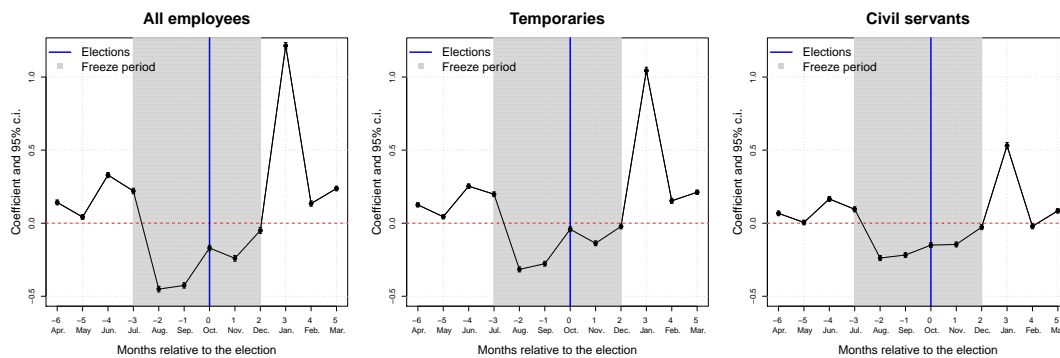
Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1.

Figure 18: Political bureaucratic cycles in hires, by contract type
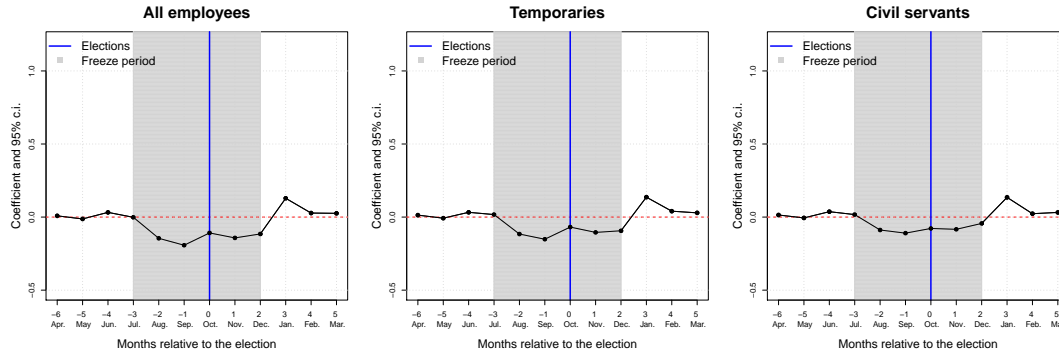Binary measure of whether there are any hires

Figure 19: Political bureaucratic cycles in hires, by contract type
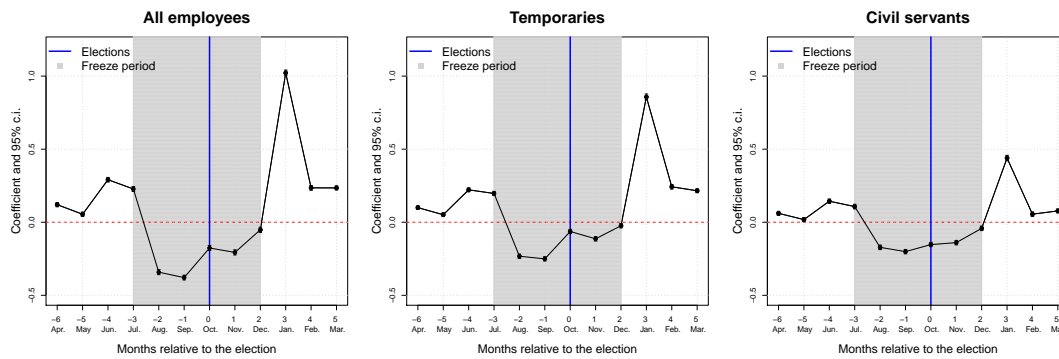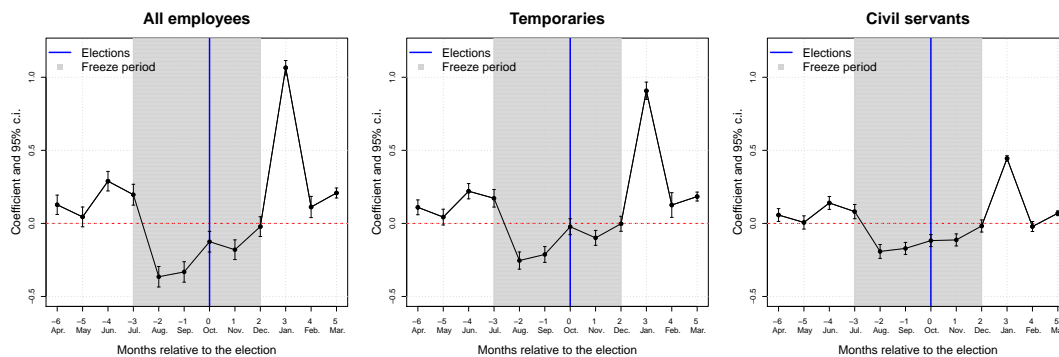Omitting the lag of the dependent variable

Figure 20: 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 21: Political bureaucratic cycles in hires, by contract type
Standard errors clustered by municipality and by year
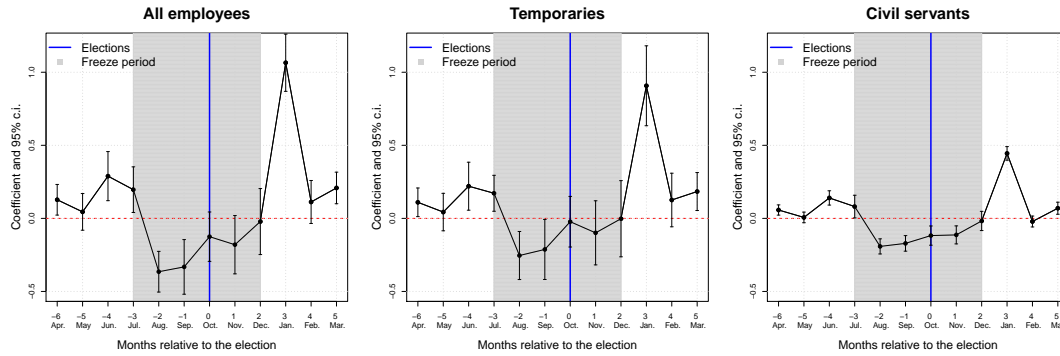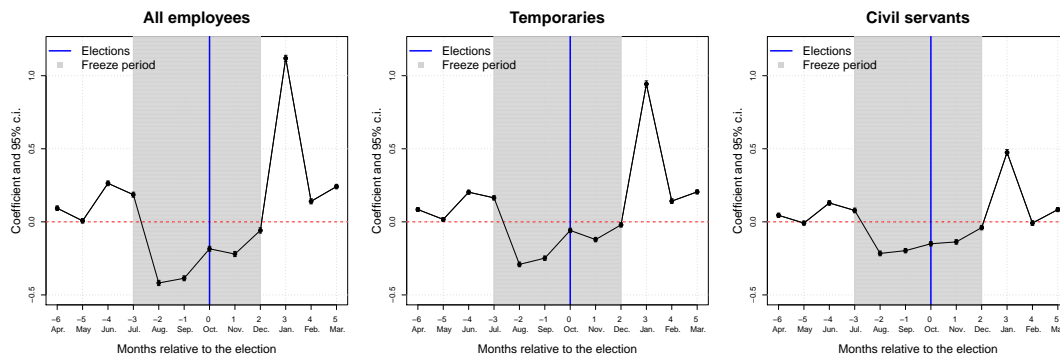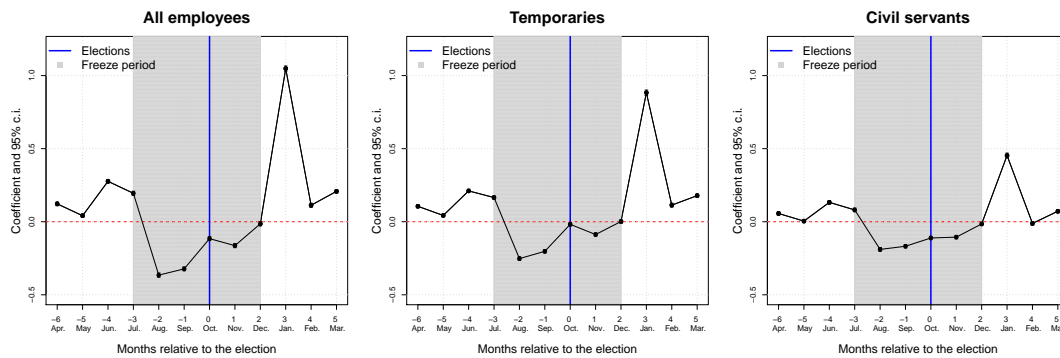

Figure 22: Political bureaucratic cycles in hires, by contract type
Omitting years with state and federal elections

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

Figure 23: 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.