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

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Abstract

Prior research has established that politicians often manipulate public resources before elections to win votes. Much less is known about the effects of policies designed to constrain such behavior. I argue that laws limiting politicians' discretion over policy tools during election periods – a common policy approach – displace (and may even intensify) these cycles. I present evidence to support this hypothesis using administrative data on municipalities in Brazil, where federal laws prohibit hiring bureaucrats around elections. Consistent with strategic adaptation, hiring surges before the freeze period, thereby creating anticipatory cycles. I exploit quasi-experimental variation in political incentives and anti-corruption enforcement to demonstrate that incentives and constraints shape these cycles. These findings reveal a paradox: rather than curbing political budget cycles, election-time constraints on government discretion displace them, potentially deepening their costs for fiscal discipline and electoral fairness. Successful anti-corruption policies must thus account for politicians' strategic adaptation to constraints.

Introduction

Politicians often manipulate policy tools – such as government spending, procurement, or debt – before elections to increase their chances of re-election, to either exploit informational asymmetries with citizens or signal their competence and/or priorities. Since Nordhaus' (1975) seminal contribution, dozens of studies published in top political science and economics journals have documented the existence of political budget cycles, and how they vary with contextual characteristics.[1]

However, we know much less about the effectiveness of policies designed to curb political budget cycles. A common policy strategy, especially in the Global South, is to introduce election-time restrictions such as bans on hiring, transfers, or procurement in the weeks or months leading up to an election. These freeze periods seek to protect fiscal discipline and electoral fairness. From a policy perspective, these legal constraints seem appealing because they are easy to codify and enforce. 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 up to elections.

In this article, I argue that constraints on political discretion during the electoral period do not eliminate political budget cycles because they fail to anticipate politicians' ability to strategically adapt to them. When legal constraints are narrowly defined in time, and political incentives are strong, politicians shift their manipulation to just before the ban. This strategic response to well-intentioned laws creates what I call *anticipatory cycles*. Election-time constraints on political discretion may therefore unintentionally extend the temporal range of the manipulation and thus deepen its fiscal and electoral footprint.

I test this argument by examining political budget cycles –shifts in public employment– in Brazilian municipalities, a context in which politicians have significant discretion over hiring, and federal laws constrain hiring during the 6-month window around elections. I use contract-level, administrative data on the universe of municipal employees from 2000 to 2019 (a 20-year period covering 5 elections), aggregated to panels of over 1 million municipality-month observations, to establish that hiring declines during the freeze period but surges beforehand. These analyses finely control for local conditions (with tens of thousands of municipality-year fixed effects) and overall seasonality (with month fixed effects), and are robust to a wide range of alternative specifications.

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[1]Franzese (2002), Alt and Rose (2009), De Haan and Klomp (2013) and Dubois (2016) review this extensive literature, and Philips (2016) reports the results of a meta-analysis.

I find that political bureaucratic cycles are more pronounced among temporary hires, but persist in tenured civil service hiring. This result highlights how politicians' discretion over the timing of civil service hires can be mobilized for political advantage, and challenges the assumption that such hires are insulated from political influence.

I then use observational and quasi-experimental strategies to parse out the role of political incentives and legal enforcement in driving these anticipatory cycles. To identify the causal effect of political incentives on cycles, I leverage a population discontinuity in the size of local city councils imposed by an unexpected judicial decision. Building on previous work that has used this discontinuity to prove that mayors in localities with larger legislatures have more incentives to distribute patronage (Mignozzetti et al., 2024; Frey, 2024), I show that political incentives to use patronage lead to stronger surges in employment ahead of the ban.

To examine the causal effect of legal enforcement on cycles, I leverage the random allocation of federal anti-corruption audits, which previous work has shown lead to increases in legal actions and reductions in corruption (Avis et al., 2018). I find that municipalities exposed to an audit not only experience more marked declines in hiring during the freeze period, but also stronger surges ahead of it.

A final set of results indicates that hiring booms before the freeze period are associated with electoral returns for incumbents. Municipalities that experience stronger employment surges have incumbents that are more likely to run, obtain larger vote shares, and get reelected. While these results are not causal, the stability of estimates when controlling for potential confounders suggests that anticipatory cycles, produced by the combination of incentives and constraints, influence electoral competition.

While this study focuses on public employment in Brazil, the underlying logic of strategic adaptation to legal constraints is likely to apply across policy domains and institutional contexts. Similar dynamics may affect procurement, transfers, or social benefits – at least in settings where legal bans are temporarily defined but political actors retain discretion outside of these freeze periods. More broadly, the findings speak to a central challenge associated with policy reforms: rather than passively complying with rules, office-seeking politicians may adapt to them by shifting manipulation to other periods and/or other policy tools not covered by the rules.

This article makes three contributions. First, my argument and empirical results shift our attention from *whether manipulation exists* to *what reformers can do about it*. This is a key

political economy question that early formal contributions emphasized (Tufte, 1978; Rogoff, 1990) but empirical research has largely overlooked. Recent studies have examined the effect on cycles of permanent fiscal rules (Bonfatti and Forni, 2019; Gootjes et al., 2021; Mair and Mosler, 2025) and bans on credit claiming around elections (Bueno, 2023). To my knowledge, however, this study is the first to examine the impact of election-time constraints on governments' discretion to perform actions such as spend, procure, or hire, despite their prevalence across countries in the Global South.

Second, by focusing on political bureaucratic cycles, the article highlights the political versatility of public employment. Government jobs can serve both clientelistic and programmatic goals, and even civil service systems –typically assumed to depoliticize hiring– sometimes give incumbents discretion over the timing and volume of appointments. I demonstrate that this discretion can be mobilized for electoral advantage, underscoring the need to scrutinize the room for manipulation that civil service systems leave rather than taking their success for granted.

In a third contribution, by uncovering how and why election-time constraints on government discretion can fail (and potentially widen political budget cycles), this article highlights a broader problem for policies and research seeking to curb corruption. Efforts may fail or even backfire if they do not seriously consider how politicians respond strategically to constraints. This study advances an emerging literature on how political and bureaucratic elites' responses to anti-corruption efforts may undermine them and even introduce new distortions (Wang, 2022; Jiang et al., 2022; Rich, 2023; Gerardino et al., 2024).

Theory

Promises and pitfalls of election-time constraints on political discretion

Scholars in political science, economics, and public administration have long documented that politicians often manipulate policy tools ahead of elections, at least when they have the incentives and ability to do so (Alt and Rose, 2009). This work has uncovered electoral cycles in a wide range of policy tools including overall spending (Khemani, 2004), capital expenditures (Pierskalla and Sacks, 2018), social benefits (Bueno, 2023), taxation (Douglas et al., 2025), fines (Su and Buerger, 2025), fees (Bracco et al., 2024), government deficit (Veiga et al., 2017), government

revenue (Alt and Lassen, 2006), and transfers to lower levels of government (Lee et al., 2024). These strategies typically seek to improve incumbents' chances of electoral success, to the detriment of fiscal discipline and electoral fairness.

To prevent such manipulation, countries around the world have established legal constraints on politicians' ability to wield policy tools around elections.[2] These policies are typically seen as leveling the playing field by removing opportunities for self-serving actions during the campaign period, and protecting fiscal discipline at a time when the political incentives to spend and to use the public administration to the incumbent's advantage are particularly high. These concerns are not merely theoretical: recent evidence shows that corruption tends to rise in the lead-up to elections, precisely when the incentives for manipulation peak (Figueroa, 2021; Cooper et al., 2021).

Table 1 presents examples of election-time constraints on government discretion from a diverse sample of Latin American countries. These policies target a variety of policy tools. For example, Colombia's electoral law makes it illegal for governments to sign procurement contracts with the private sector (with few exceptions) in the 4 months leading up 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 an election, and Spain's electoral law forbids inaugurating any public works during a campaign.[3]

I argue that these constraints shift the timing of policy manipulation rather than prevent it. When the political incentives are strong and the legal constraints are rigid but time-bound, politicians are likely to engage in any discretionary actions earlier –before the restrictions become binding– rather than abstain from them altogether. As a result of politicians' strategic responses to them, these legal constraints seeking to protect fiscal discipline or electoral fairness displace rather than eliminate cycles, and may expand their temporal coverage and consequences. Such time-bound constraints are likely to induce strategic anticipation, which can extend the cycle's reach rather than shorten or dampen it. This builds on a key theoretical insight from Rogoff (1990), who

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[2]In this article, I focus on rules limiting incumbents' ability to do things. A separate type of policies limit their ability to speak about them, or to take credit for them – for example through government ads, which are often banned or restricted during campaigns. Bueno (2023) shows that these restrictions on credit claiming can dampen political budget cycles because they limit politicians' ability to signal competence and attribution to voters.

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

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
Deficit	Peru	No more than forecast deficit	Last 6 months of mandate
Procurement	Colombia	No direct purchases	4 months before the election
Transfers	Brazil	No inter-governmental transfers	3 months before the election
Social benefits	Dominican Republic	No new beneficiaries or increases	During the campaign
Inaugurations	Guatemala	No inauguration of public works	During the campaign
	Uruguay	No hiring of civil servants	Last 12 months of mandate
Employment	Colombia	No hiring or firing	4 months before the election
	Brazil	No hiring, firing, or transfers	Last 6 months of mandate

Note: Legal basis for constraints – Peru: Law 27245 (1999); Colombia: Law 996 (2005); Brazil: Law 9504 (1997); Dominican Republic: Law 20-23 (2023); Guatemala: Decree 1-85 (1986); Uruguay: Law 16127 (1990).

argued that efforts to curb political budget cycles could induce other, potentially costlier forms for politicians to signal to voters.[4]

By disregarding politicians' strategic adaptation to legal constraints, policies that curb government discretion in a temporal window around elections may create new problems. These anti-corruption policies seem to be premised on the assumption that politicians will comply and abstain from manipulating policy tools during the months or weeks leading up to the election. Yet, if politicians face strong political incentives, they will respond to these legal constraints by engaging in their manipulation right before the freeze period. This anticipation of expansionary policies (e.g., boosting spending, increasing debt, or cutting taxes) may therefore amplify political budget cycles' fiscal and electoral footprint.

A key implication of this argument is that policies that seek to curb political budget cycles need to consider how office-seeking politicians will strategically respond to them. When the constraints are rigid but temporally narrow, politicians are unlikely to passively comply. Instead, they may simply engage in their actions before the freeze period begins. To mitigate this possibility, policy reformers may consider broader or more continuous regulations – such as year-round fiscal rules or more structural constraints on hiring or procurement– that limit discretion more fully and make it harder to circumvent the rules.[5]

[4]Tufte (1978, 149-154) advanced a similar critique of policies constraining politicians around elections.
[5]Several studies have found that fiscal rules can dampen political budget cycles (Bonfatti and Forni, 2019; Gootjes et al., 2021), especially when rules have sanctions attached (Mair and Mosler, 2025).

Why analyzing public employment offers leverage for testing anti-cyclical constraints

In this article, I focus on what I call *political bureaucratic cycles* – political cycles in the hiring of government personnel. This focus on public employment has several advantages to study political budget cycles and the effectiveness of election-time constraints.

Public employment is politically consequential. On average, the government payroll comprises a quarter of government spending (International Monetary Fund, 2016). Through this spending, governments mobilize labor to pursue a variety of policy goals, including ensuring order, providing services, and responding to citizen demands. Because government jobs imply recurring transfers to employees, who in turn are expected to work for the state, public employment can be exploited for a variety of political purposes, including mobilizing voters (Oliveros, 2021), campaign financing (Sigman, 2022), party building (Grindle, 2012), and rewarding supporters (Colonnelli et al., 2020).

Public employment can be expanded ahead of elections based on both a clientelistic and a programmatic logic. For citizens, government jobs are often highly valued – especially in contexts where private sector jobs are scarce or where government jobs are better paid or more stable (Finan et al., 2017). That –together with the fact that jobs are targetable, often revocable, and distributed directly by governments without the need for brokers– makes them particularly powerful assets in clientelistic exchanges in which citizens may receive jobs in exchange for political support (Robinson and Verdier, 2013). Government jobs can alsobe used programmatically to boost service delivery in the lead-up to elections, particularly in more visible jobs or more salient policy areas, thus improving voters' perceptions of the incumbent's competence or priorities. Research on German states, for example, has identified political cycles in the hiring of teachers and police officers because education and security are salient and visible areas of government activity (Tepe and Vanhuysse, 2009, 2013).

Due to the political versatility of government jobs, political bureaucratic cycles have been detected in a wide range of settings, including OECD countries (Aaskoven, 2021), US states (Cahan, 2019), German states (Tepe and Vanhuysse, 2013, 2009) and municipalities in Indonesia (Pierskalla and Sacks, 2020), Greece (Chortareas et al., 2017), the Philippines (Labonne, 2016), and Finland and Sweden (Dahlberg and Mörk, 2011). Together, these findings suggest that political bureaucratic cycles are not a pathology of a particular political, economic, or institutional environment but rather a common feature of electoral politics in settings where politicians have the discretion to expand hiring.

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 incumbents from hiring bureaucrats during the last year of their term. Brazil's electoral and fiscal rules restrict hiring in the 6- month period around elections. In the Philippines, the Election Code bans hiring and promoting bureaucrats starting 45 days before an election. In Pakistan, the Electoral Commission temporarily banned hiring ahead of the 2018 elections.

Focusing on public employment allows me to measure political budget cycles with a high level of granularity because employment decisions are ultimately made (and potentially observed) at the contract or employee level. This matters because examining how election-timed constraints shape manipulation requires data on disaggregated decisions that can be tied to a particular date to determine their proximity to the freeze period. Public procurement and employment data is often detailed enough to allow such temporal analysis; other spending decisions are often reported in quarterly or yearly fiscal datasets. Where contract-level data is available, a focus on public employment also has the advantage of allowing the political rationales of the cycles to be tested. If the pre-election expansion in hiring is politically motivated, it should be more pronounced for low-skilled employees (for whom the clientelistic logic is more likely to apply) and for those in policy areas that are more visible and/or salient to voters (in line with the programmatic logic).

Examining public employment under election-timed constraints offers a revealing test of my theory of anticipatory cycles driven by the strategic displacement of manipulation. Of the various policy tools subject to electoral manipulation, government jobs are unusually visible to oversight institutions, politically salient, and legally regulated. If politicians still manage to shift manipulation earlier in the cycle despite restrictions such as those outlined in Table 1, that would offer strong evidence that such constraints displace, rather than dampen, political budget cycles. Since government jobs are often year-long, or even permanent if in the civil service, an earlier expansion of hiring may have more dire consequences for electoral fairness and fiscal discipline. Public employment therefore represents a high-stakes and high-clarity domain for evaluating how legal rules interact with electoral incentives.

A final advantage is that focusing on public employment allows me to compare the effectiveness of election-timed constraints with that of permanent civil service limits on government discretion over hiring. When well implemented, civil service reforms typically remove political discretion over the allocation of government jobs, thus fostering state capacity to the detriment of patronage

(Geddes, 1994; Aneja and Xu, 2024). In that sense, civil service systems can be seen as permanent (rather than time-bound) constraints on government discretion, similar to year-long fiscal rules, which have been shown to dampen political cycles in spending (Bonfatti and Forni, 2019; Gootjes et al., 2021; Mair and Mosler, 2025). Comparing cycles in temporary and civil service hiring sheds light on how different types of constraints –time-bound bans versus more structural and year-long civil service protections– shape political manipulation, offering insights into what types of anti-cyclical policies are most effective.

Institutional setting

I test this theory through an empirical study of political cycles in public employment in Brazilian municipalities. Brazilian local governments are an ideal setting in which to examine political bureaucratic cycles and legal constraints' ability to curb them. Elections are held on a fixed schedule, bureaucracies are relatively large, and politicians have significant 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 5,569 municipal governments, most of which are small and poor.[6] Local elections are held every 4 years on the first Sunday of October.[7] 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 of the incumbents who ran in 2016 were defeated. Mayors are elected through a majoritarian system,[8] and since 1997 are only allowed to run for re-election once. City councilors are elected though a proportional, open-list system. Mayors depend on the city council to pass laws, including their budget. In most cases, the mayor's party does not have a majority in the council; therefore they need to build and sustain legislative coalitions (Frey, 2024), which they often do through bureaucratic appointments (Kim, 2020; Mignozzetti et al., 2024).

Municipal governments have a relatively large workforce because they are responsible for provid-

[6]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.

[7]This 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.

[8]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.

ing 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., 2020, 3090). Mayors and the secretaries they appoint have significant discretion over the hiring of bureaucrats in all policy areas. This discretion differs significantly between the civil service and other hiring modes that have fewer employment protections.

Brazil has a well-established civil service system and civil servants comprise roughly two-thirds of the municipal labor force. The federal constitution requires all permanent staffing needs to be filled with civil service contracts. Top performers on competitive examinations are eligible for positions, which have lifetime tenure after a probationary period. Critically, however, the best performers are not automatically hired. While politicians have no discretion over candidates' ranking, they can decide on the timing and number of civil service hires.[9] This opens space for the manipulation of civil service hiring ahead of elections. Political discretion over hiring is higher for temporary contracts, which can legally be used to hire political appointees or fill short-term or urgent staffing needs. In practice, temporary contracts are sometimes used where civil service contracts should be signed instead.

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). The 1997 Electoral Law forbids hiring, firing, or transferring bureaucrats 3 months before or after an election to protect candidates' equality of opportunity.[10] The 2000 Fiscal Responsibility Law (LRF, *Lei de Responsabilidade Fiscal*) 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 seeks to limit 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 untenured) generally 3 months before the election. Appendix A reports additional details on legal constraints and the penalties for breaches.

Politicians can be prosecuted for violating these rules; the penalties include losing their post, having their political rights suspended, substantive fines, and even imprisonment. Prosecution of

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[9]This is not unique to Brazil: Mexico for example uses a similar system.

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

Figure 1: Timeline of election cycles in Brazil
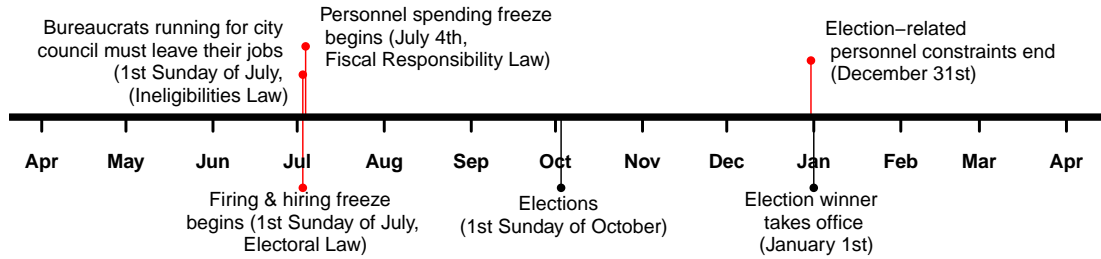
politicians for corruption charges is not rare. Lambais and Sigstad (2023) estimate that 7.7% of mayoral election winners or runners-up have been charged with corruption. Bento et al. (2021) document 1,716 court cases involving mayors and former mayors between 1992 and 2016 in the state of Rio Grande do Sul, which contains 497 municipalities. I created an original dataset of corruption news reports (scraped from the websites of the prosecutor's offices of the states of São Paulo and Bahia, the largest states in Brazil's two most populous regions) which provides additional evidence that politicians are prosecuted for violating employment rules. In the 10-year period between 2013 and 2022, I found 76 reports relating to violations of public employment laws that mentioned former mayors.

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 how they are spending federal transfers. It then releases the results of these 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 (Avis et al., 2018).

Research design

Measuring political bureaucratic cycles

I exploit the exogenous timing of local elections to identify political bureaucratic cycles in Brazilian municipalities. I examine panels of over 1 million municipality-month observations, covering five

election cycles. I use tens of thousands of fixed effects to finely control for seasonality and local conditions, and estimate how public employment varies in the months before elections compared to the same months in non-election years. Baseline specifications use linear regression with the following estimating equation:

$$Y_{iym} = \alpha_{iy} + \theta_m + \sum_{p=-6}^{0} \beta^p D_{iym}^p + \gamma Y_{iym-1} + \varepsilon_{iym} \tag{1}$$

Y_{iym} is a given outcome (e.g., the number of hires) corresponding to municipality i in year y in month m. Since the outcomes are right-skewed count variables, 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, which control for monthly shocks common to all municipalities and thus account for underlying seasonality in public employment. [11] 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 0 (October, the month when the election is held). β^p are the coefficients corresponding to those 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, where employment decisions are made, to allow for arbitrary serial correlation.

I estimate how hiring differs in the months leading up to an election rather than a wider window covering periods before and after the election because election results shape the post-election dynamics. Whether the incumbent wins or loses their bid for reelection significantly affects public employment, both during the lame-duck period (October - December) and after the winner is sworn in in January (Toral, 2024). Focusing on the 6-month window preceding the election allows me to cover the month of the election, 3 months of the freeze, and 3 months before the freeze to determine whether the constraints trigger any anticipatory manipulation.

I employ log-linear ordinary least squares (OLS) models as the baseline estimation because they provide easily interpretable estimates of percentage changes in hires, which is the relevant quantity of interest given my focus on relative pre-election surges in public employment ahead of elections. While the dependent variable is a count, it has a wide range and is right-skewed. In these cases, OLS with a logged dependent variable is widely used in the literature.

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[11]Appendix C shows the distribution of municipal hiring by month.

In Appendix L, I show that my results are robust to a range of alternative specifications. These include using Poisson models rather than linear regression,[12] measuring electoral cycle effects for the a 12-month window around elections, estimating each electoral cycle effect (the β^p terms in Equation 1) in a separate regression, using different transformations of the dependent variable (dropping zeros, using the inverse hyperbolic sine, or using a binary indicator), omitting the lagged dependent variable, using two-way fixed effects, using a balanced panel, and excluding years with federal or state elections. I also demonstrate that the findings are similar using more conservative strategies of statistical inference: clustering standard errors by municipality and year, or clustering them by municipality and month. Across all these specifications, the estimated cycles remain strong and statistically significant, confirming that the results are not driven by modeling assumptions.

Identifying how political incentives affect cycles

I use observational and quasi-experimental approaches to measure heterogeneity in political bureau-cratic cycles and test whether political incentives intensify them.

I first approach this observationally by measuring whether cycles are more pronounced in contexts that are more electorally competitive. If cycles are driven by politicians' strategic manipulation of public employment in the lead-up to an election, rather than by administrative necessities, they should be stronger where the previous mayoral election was tighter because the may perceive a stronger need to manipulate public employment ahead of the election. To test this hypothesis, I expand Equation 1 by adding a binary indicator of whether the previous municipal election had an electoral concentration in the lowest quartile, and interact it with the month fixed effects and the election cycle period indicators.[13]

$$
\begin{aligned}
Y_{iym} = \mu_i + \lambda_y + \theta_m + \sum_{p=-6}^{0} \beta^p D_{iym}^p + \gamma Y_{iym-1} + \\
\left(\zeta + \phi_m + \sum_{p=-6}^{0} \delta^p D_{iym}^p \right) K_{iy} + \varepsilon_{iym}
\end{aligned}
\tag{2}
$$

K_{iy} is an indicator for whether municipality i had a low level of electoral concentration in the most recent election relative to year y. Since that covariate is not exogenous, δ^p coefficients

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[12]Results are also similar using overdispersed Poisson models.

[13]I measure electoral concentration using the sum of squared vote shares across all candidates in a given municipal election. This Herfindahl-style index captures how fragmented or dominant the race was.

describe how the cycles differ between more and less competitive settings.[14] This specification uses two-way fixed effects ($\mu_i + \lambda_y$) rather than interactive fixed effects (α_{iy} in Equation 1), to preserve identifying variation in the covariate K_{iy}, which is constant within each municipality-year and would therefore be absorbed by α_{iy}.

Although measuring heterogeneity in political cycles by interacting period dummies with endogenous covariates is standard in the literature (De Haan and Klomp, 2013), confounders may bias estimates of this kind. Localities with more competitive elections may be systematically different on a number of observable and unobservable characteristics that could shape political bureaucratic cycles, including the quality of government and social accountability.

I therefore complement the observational strategy with a more reliable quasi-experimental analysis that exploits exogenous variation in legislature size. The 1988 constitution stipulated that the number of city councilors in each municipality should be proportional to its population, but gave local governments significant discretion to choose that number within broad guidelines. In 2004, the Supreme Electoral Court established population cutoffs for each additional city councilor. Under the new system, municipalities with fewer than 47,620 residents were to elect 9 councilors, whereas those above that threshold would elect 10. From there, municipalities would add one additional city councilor for every 47,619 residents, up to a million residents. Because Brazilian mayors depend on the city council to pass legislation, an additional councilor implies they need the loyalty of more individuals to obtain the same degree of support in the council. That is, an exogenous increase in the size of the local legislature led to an increase in mayors' "cost of political brokerage" (Frey, 2024) and incentives to use patronage. Previous studies have used a regression discontinuity design to establish that one additional city councilor leads to higher levels of corruption (Britto and Fiorin, 2020) and the hiring of more political appointees (Mignozzetti et al., 2024) due to the heightened bargaining and transaction costs for the mayor (Frey, 2024).

To identify how cycles vary with this exogenous increase in legislature size, and therefore mayors' incentive to manipulate public employment to build and sustain political coalitions, I restrict the sample to observations between 2001 and 2004, when the exogeneity of the treatment is clearest. The Supreme Electoral Court's March 2004 ruling about legislature size mandated that the official 2003 population statistics be used to determine the number of city councilors to be elected in

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[14]To avoid confounding cycle dynamics with group-specific seasonality, I allow the monthly fixed effects to vary by group. This ensures that the δ^p coefficients capture differential electoral dynamics, not differences in baseline monthly hiring patterns.

October.[15] Using this sample, I apply the following estimating equation:

$$Y_{iym} = \lambda_y + \theta_m + \sum_{p=-6}^{5} \beta^p D_{iym}^p + \gamma Y_{iym-1} + \tag{3}$$
$$\left(\zeta + \phi_m + \sum_{p=-6}^{5} \delta^p D_{iym}^p \right) L_i + \pi P_i + \rho L_i P_i + \varepsilon_{iym}$$

L_{iy} is an indicator for municipalities assigned to have one additional city councilor based on the first population threshold of 47,620 residents. I focus on the first discontinuity for two reasons. First, it accounts for most municipalities in Brazil: over 95% had fewer than 95,238 residents in 2003. Second, the first cutoff is where the increase in the size of the legislature (from 9 to 10 seats) is largest in relative terms, and thus where the change in mayors' bargaining costs is most notable.[16]

Following the logic of regression discontinuity designs (Cattaneo et al., 2019), in this model I also control for the municipality's population, recentered around the cutoff (P_i), and its interaction with the indicator for localities above the threshold L_{iy}. Critically for the validity of this design, the distribution of population figures is continuous around the threshold (Appendix D) and no other policies kick in at that threshold (Eggers et al., 2018). In this design, the δ^p coefficients identify the effect of having one additional city councilor on cycles. This design drops the municipality fixed effects because they are perfectly collinear with the 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.

Identifying how legal constraints shape cycles

I also use observational and quasi-experimental approaches to test whether political bureaucratic cycles are more pronounced in settings with more anti-corruption constraints.

First, I examine whether cycles became more pronounced after the passage of the Fiscal Responsibility Law (LRF) in 2000,. In response to to pressures for monetary and fiscal discipline and

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[15]Results are similar when including the 2008 election cycle. In 2009, a constitutional amendment changed the rules about the relationship between population and legislature size.

[16]Results are similar when excluding municipalities over 95,238 residents, the cutoff at which another seat was added.

to problems of fiscal asymmetry between the three levels of the federation (Loureiro and Abrucio, 2004), the LRF banned mayors from increasing their personnel expenses dyuring the last 180 days of a their term. While the 1997 Electoral Law and an earlier 1974 law constrained hiring around elections, the LRF provided a stronger legal mandate and stipulated clear penalties for breaches. The penalties, articulated in the 2000 reform of the penal code, include 1-4 imprisonment. Combined, these legal reforms in 2000 raised the expected costs of using public employment opportunistically from early July in an election year. I compare cycles before and after the LRF's passage to assess whether legal constraints shape and strengthen political bureaucratic cycles.

I use Equation 2 using an indicator for years 2000 onward as K_{iy}. This analysis includes observations since 1995 (i.e., one cycle of elections before the LRF).[17] I do not include years before 2000 in baseline specifications because those years have much higher levels of underreporting in the employment dataset (Appendix B) and weaker legal mandates, but the comparison is useful to test whether legal constraints shape the cycles. If the decrease in hiring during the freeze period was less pronounced before the 2000 legal reforms, and the expansion of hiring less marked in the lead-up to the freeze period, that would be consistent with constraints displacing and shaping the cycle.

I complement these observational estimates with a quasi-experimental design that leverages the random assignment of municipal governments to federal anti-corruption audits conducted by the CGU. I define municipalities as exposed to an audit from the year they were selected via lottery until 3 years later, i.e. for a 4-year period (the length of a mayoral term).[18] 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). The randomized federal government audits can be seen as an exogenous shock that uncovers potential irregularities in the use of public funds while providing information and incentives that strengthen the work of other accountability actors including the local opposition, the media, and prosecutors.

To estimate the effect of anti-corruption audits on cycles, I employ Equation 2 using as K_{iy} an indicator of whether the municipality was recently exposed to a CGU audit. Since the audits are randomly assigned, the δ^p coefficients identify how cycles vary as a result of exposure to federal

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[17]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.

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

audits.

Measuring the association between the strength of cycles and incumbents' electoral performance

Finally, to assess whether pre-election hiring surges influence electoral outcomes, I examine their association with the incumbent's decision to run for reelection, their margin of victory, and whether they win. I focus on incumbent (rather than party) electoral performance because Brazilian local politics feature weak partisan attachments (Boas et al., 2019) and pervasive party switching by mayors (Klašnja and Titiunik, 2017). Nearly a third (30.4%) of the mayors who ran for reelection in 2008 did so under a different party than the one they were elected with in 2004.

I measure of the pre-election hiring boom as the average residualized logged number of hires in April-June of election years. These residuals are obtained from a regression controlling for month fixed effects, the lagged dependent variable, and municipality-year fixed effects, with standard errors clustered at the municipality level (as in Equation 1 but omitting the D_{iym}^p indicators). I take the average of the residuals corresponding to the pre-freeze months in election years, and standardize it for ease of interpretation so that it has a mean of 0 and a standard deviation of 1. I then use that measure of the hiring boom in a given municipality-election cycle as the key independent variable in the following specification:

$$Y_{iy} = \lambda_y + \eta_i + \sigma \tilde{H}_{iy} + X_{iy}'\xi + \varepsilon_{iy} \tag{4}$$

Y_{iy} is an electoral outcome (whether the incumbent runs and, conditional on them running, their electoral margin and whether they win) for municipality i in election cycle y. λ_y and η_i are election cycle and state fixed effects, respectively. σ identifies the association between the standardized measure of the hiring boom ($\tilde{H}_{iy}$) and each of the outcomes. To assuage endogeneity concerns, I include a series of municipality covariates X_{iy}: logged GDP per capita, logged population, and indicators for whether the incumbent mayor belongs to one of the three major parties at the time (MDB, PSDB and PT). This specification clusters standard errors clustered at the state level to account for electoral and enforcement correlations across municipalities in a state.

16

Data

I leverage administrative data on public employment, elections, population, and anti-corruption audits in Brazilian municipalities. I focus on monthly variation to identify political bureaucratic cycles with a high level of granularity.[19]

To measure changes in public employment across the electoral calendar, I use the federal government's Annual Social Information Report (RAIS, *Relação Anual de Informações Sociais*) from 2000 to 2019, which covers 5 elections. Municipal governments –like all employers in the formal sector– are legally required to report all of their contracts to the Ministry of the Economy every year. RAIS therefore contains data on the universe of municipal employees, including contract type, start and end dates, salary, reason for termination, and professional category.[20] I generate counts of hires, by type of contract, for each municipality and each month.[21] I also use the occupation codes available in RAIS since 2002 to measure employment across skill levels and for professionals in the healthcare versus education sectors. These analyses allow me to test whether cycles are more pronounced for low-skill employment and for jobs in more salient or visible policy areas.

For heterogeneity analyses I employ administrative data on election outcomes, population (which determined the number of legislators to be elected in 2004), and anti-corruption audits. To define more electorally competitive settings, I use data from the Supreme Electoral Court to identify municipalities in which the level of electoral concentration in the previous election was in the bottom quartile. To determine municipalities' assigned number of legislators in the 2004 election, I use the official population count for 2003 produced by Brazil's Institute for Geography and Statistics. To examine whether the increased salience and enforcement of rules shapes cycles, I leverage CGU data on the audits conducted between 2006 and 2015, which were randomly assigned.[22]

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[19]Results are similar when analyzing the data at the quarter level.

[20]As shown in Appendix B, a small number of municipalities (1-10% in 2000-2019) do not report having any employees in a given year during my study period. The analyses are therefore not representative of the whole country but of municipalities that reported data to RAIS every year from 2000 to 2019. Those that fail to report employment data are generally smaller and poorer. This selection plausibly biases the results towards zero, since 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.

[21]I include all contracts, but results are similar when restricting it to those of at least 40 hours a week.

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

Results

The regression results demonstrate that the hiring of bureaucrats displays marked cyclical patterns consistent with politicians responding to both the electoral incentives and legal constraints related to hiring. This section presents four sets of results. First, hiring decreases during the freeze period, but expands in the months leading up to it. Second, cycles are more pronounced in localities with more competitive elections, and in municipalities that were induced to elect an additional city councilor, thus boosting the incumbent's incentives for patronage. Third, cycles became more pronounced after 2000 (when the Fiscal Responsibility Law and other legal reforms strengthened the ban and established strict penalties), and in localities exposed to a federal anti-corruption audit. Finally, incumbents experience better electoral performance in municipalities with a more noticeable expansion of hiring ahead of the freeze period. Table 2 synthesizes the relationship between theoretical expectations, research design, and the empirical results.

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 leading up to elections, compared to the same months in non-election years, after controlling for seasonality and local conditions. These results, detailed in Table 3, suggest that politicians engage in anticipatory cycles in hiring, driven by both electoral incentives and laws that constrain the expansion of hiring around elections. Results are similar when using a wide range of alternative specifications (Appendix L).

Hiring decreases markedly during the freeze period: there are 21.24% fewer hires in August of an election year compared to the same month in a non-election year ($p < 0.001$).[23] This fall in hiring attenuates as the election gets closer but is sustained until October. These declines in hiring in the lead-up to the election are consistent with politicians responding to the legal constraints.

However, hiring markedly increases in the pre-freeze period, compared to the same period in non-election years. In June of an election year, 49.33% more employees are hired than in June of a non-election year ($p < 0.001$). Hires on July 1 are not covered by the freeze period unless the day falls on a Sunday, which is why we observe that the expansion in hiring persists in July, with

[23]On average, there are 14.47 hires in August in a non-election year.

Table 2: Mapping of theoretical expectations, research design, and results

Theoretical claim	Key variation exploited by the design	Evidence
Election-timed constraints on public employment induce political bureaucratic cycles, with declines in hiring during the freeze period and surges beforehand	Within-municipality-month variation across electoral and non-electoral years	Figure 2, Table 3
Political incentives shape the intensity of the expansion in hiring ahead of the freeze period	*Observational*: Variation in cycles between localities with more versus less competitive races	Figure 3
	Quasi-experimental: Variation in cycles in localities that elect one more city councilor because of a population discontinuity (mayors therefore have more pressures to use patronage)	Figure 4
Legal constraints drive the declines during the freeze period as well as the surge in hiring ahead of the ban	*Observational*: Variation in cycles before and after the approval of the Fiscal Responsibility Law and corresponding penalties	Figure 5
	Quasi-experimental: Variation in cycles in localities exposed to randomly assigned federal anti-corruption audits (which therefore experience an enforcement shock)	Figure 6
The pre-freeze expansion in hiring gives incumbents an electoral advantage	Correlation between hiring booms and incumbents' electoral performance	Table 4

38.16% more hires than in the same month in a non-election year ($p < 0.001$).[24] The expansion of hiring in the lead-up to the freeze period suggests that, rather than dampening manipulation, the constraints motivate incumbents to strategically time their hiring decisions to circumvent them.

Together, both sets of results suggest that politicians in charge of hiring decisions respond to both the political incentives and the constraints they face, leading to anticipatory cycles rather than preventing manipulation. Politicians do depress hiring during the freeze period, yet they anticipate the ban and expand hiring in the months leading up to it. This illustrates how election-timed constraints on government discretion shape and displace, but do not eliminate, political budget cycles. Because government jobs in this setting typically last until the end of the calendar year

[24]On average, there are 10.31 hires in June and 11.92 in July in a non-election year.

Figure 2: Political bureaucratic cycles in hiring, by contract type
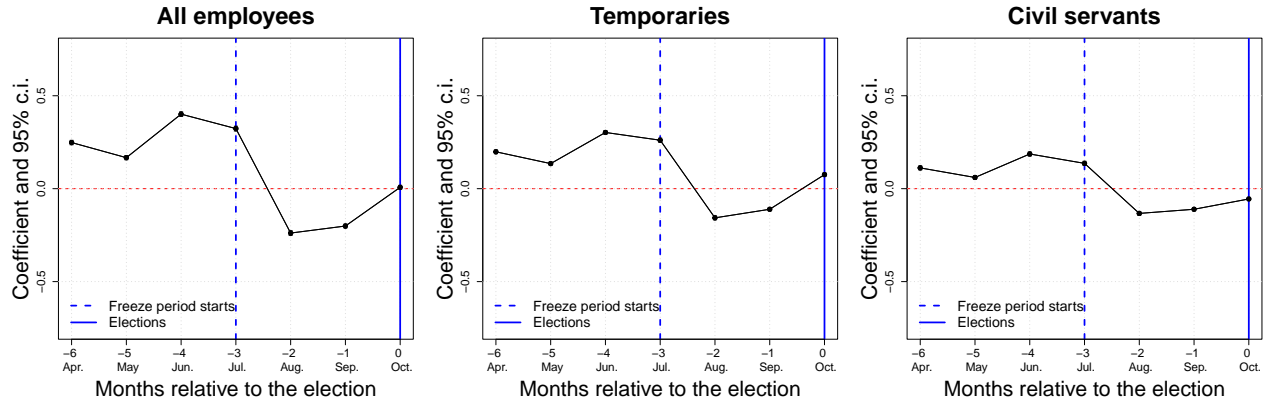
Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1. Coefficients represent log-point changes in hiring relative to the same month in non-election years.

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

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.248***	0.198***	0.112***
	(0.006)	(0.005)	(0.005)
May	0.167***	0.135***	0.060***
	(0.006)	(0.005)	(0.005)
June	0.401***	0.302***	0.187***
	(0.006)	(0.005)	(0.006)
July	0.323***	0.261***	0.136***
	(0.007)	(0.006)	(0.006)
August	-0.239***	-0.157***	-0.133***
	(0.006)	(0.005)	(0.005)
September	-0.201***	-0.111***	-0.111***
	(0.005)	(0.005)	(0.005)
October	0.007	0.075***	-0.055***
	(0.005)	(0.005)	(0.004)
Observations	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568
R^2	0.684	0.711	0.612

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

(when contracts are temporary) or are permanent (for civil service contracts), these anticipatory cycles may actually widen the fiscal and electoral footprint of cycles.

The middle and right-hand panels of Figure 2 (and the corresponding columns in Table 3) demonstrate that the intensity of the cycles varies significantly by contract type. Unsurprisingly, the cycles are more pronounced for temporary contracts; politicians have more discretion over these type of hires. However, there are also cycles in the hiring of civil servants. For example, in June of an election year hiring in the civil service is 20.52% higher compared to the same month in a non-election year ($p < 0.001$).[25]

The existence of political cycles in civil service hiring has three major implications. First, in settings like Brazil –where civil service regulations constrain who can be hired, but not how many, or when– formal protections are insufficient to prevent political manipulation. As shown in the right-hand panel of Figure 2, mayors can strategically manipulate the volume and timing of civil service appointments ahead of elections. While in some countries this discretion is more limited, in many contexts it is worth empirically testing whether civil service hiring is as insulated from political influence as is commonly assumed.

In a second implication, these results suggest that clientelism is not the sole mechanism driving electoral cycles in hiring. Civil service contracts are not revocable, and cannot be easily targeted to supporters or brokers – making them unsuitable for clientelistic exchange (Robinson and Verdier, 2013). Instead, political cycles in civil service hiring may reflect two alternative strategies. One is programmatic: governments may expand hiring in visible sectors to signal competence or responsiveness. The other is strategic entrenchment: by hiring tenured staff shortly before an election, incumbents can reduce the incoming administration's discretion to appoint their own supporters (Lewis, 2008; Toral, 2024).

Finally, from a policy perspective, these results suggest that efforts to insulate bureaucracies from political cycles must go beyond ensuring meritocratic recruitment. If politicians retain discretion over the number and timing of civil service appointments, they may still exploit those tools for electoral gain. Strengthening bureaucratic insulation may therefore require limiting this discretion directly and expanding the prevalence of (more strictly regulated) civil service contracts in the public sector.

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[25]Civil service hires are less common – on average, municipalities hire 3.56 civil service employees in June.

Additional evidence of the political rationales for the pre-electoral expansion of hiring comes from examining variation in cycles across categories of municipal employees (Appendix E). First, the pre-election expansion in hiring is more pronounced among low-skill employees. For example, in June of an election year the hiring of low-skill employees expands by 34.35% relative to June of a non-election year, compared to 26.28% among professionals ($p < 0.001$) – a difference of about 30%. Two main factors may jointly explain this gap. First, low-skilled jobs may be used to boost outputs that are more easily visible to voters, such as cleaning the streets or painting public buildings. Second, low-skilled jobs may be more easily targeted as clientelistic handouts, since they usually correspond to lower-income households which may be more amenable to sell their vote (Bobonis et al., 2022).

Political bureaucratic cycles are also more pronounced among healthcare professionals than among education professionals. Education and healthcare are the largest sectors employing municipal workers, but while education services only directly benefit families with children, healthcare services are directly relevant to most voters. In fact, healthcare is the most salient policy area for voters in municipal elections (Boas et al., 2019, 395). Accordingly, the pre-freeze boost in hiring is more pronounced among healthcare than education professionals. For example, in June of an election year and compared to the same month in a non-election year, healthcare workers hiring expands by 17.93%, compared to 14.51% for education employees ($p < 0.001$) – a difference of about 19%.

The cyclical patterns depicted in Figure 2 are unlikely to be driven by the replacement of bureaucrats who resign to run for city council. There is an uptick in resignations ahead of the freeze period (Appendix F), consistent with the legal requirement that bureaucrats running for office resign before the election. But the absolute numbers of resignations are just too small to explain the relative and absolute sizes of the overall employment cycles. For example, in June of an electoral year resignations increase by 3.41% ($p < 0.001$), over a baseline of 1.91 resignations on average. Therefore, these outflows of experienced bureaucrats can only explain a very small part of the large increases in hires documented in Figure 2.

Another potential concern is that these results may be driven by cycles in record-keeping or data reporting rather than real changes in employment. To address this concern, I examine placebo outcomes – employee deaths and retirements. While municipalities report both variables through the same system, and both should be subject to the same reporting artifacts, neither should plausibly respond to electoral incentives or anti-corruption constraints. As shown in Appendix G,

these outcomes display no consistent cyclical pattern around elections. Coefficients for employee deaths are tightly around zero and statistically insignificant. Those for retirements are slightly negative and statistically significant, which suggests that retirement decisions may be delayed for strategic or administrative reasons. Yet these coefficients are very small compared to the large swings in hiring. For example, in June of an electoral year retirements are 0.48% lower ($p < 0.05$), over a baseline of 0.50 retirements on average. These placebo tests demonstrate that the large election-related cycles in hiring documented in Figure 2 are unlikely to be driven by record-keeping by bureaucrats or other mechanical features of the data generating process.

Political incentives intensify pre-election hiring

Heterogeneity analyses across localities lend further support to the hypothesis that political incentives are a key driver of political budget cycles. First, cycles are more pronounced in localities where the previous election had higher levels of electoral competitiveness. Figure 3 shows observationally that localities where the previous election had an electoral concentration index in the lowest quartile exhibit a more prominent expansion of hiring. In June of an electoral year, the boost in hiring is 3.7% more pronounced under incumbents that were elected in more competitive races compared to those where the previous election was less competitive ($p < 0.01$). This is consistent with incumbents' electoral incentives being a driving force in political bureaucratic cycles. Still, these heterogeneity results could well be confounded by other political or socioeconomic characteristics of localities with more competitive elections.

To causally identify the effect of political incentives on cycles, I use exogenous variation in city council size induced by the 2004 Supreme Electoral Court decision that established deterministic population thresholds (based on official 2003 population figures) for the number of city councilors to be elected that year. Figure 4 shows that municipalities above the threshold of 47,619 residents, which were required to elect one additional city councilor, experienced more marked pre-election expansions in hiring. In June of an election year, municipalities that were to elect a larger city council expanded hiring by an additional 19.01% ($p < 0.001$) compared to those with a smaller legislature and relative to June of a non-election year.

These results build on previous scholarly findings that a larger legislature increases mayors' bargaining costs, leading them to increase political appointments and corruption (Mignozzetti et al., 2024; Britto and Fiorin, 2020; Frey, 2024). With this in mind, the results in Figure 4 suggest that

Figure 3: Political bureaucratic cycles in total hires, by level of electoral competitiveness in the previous election
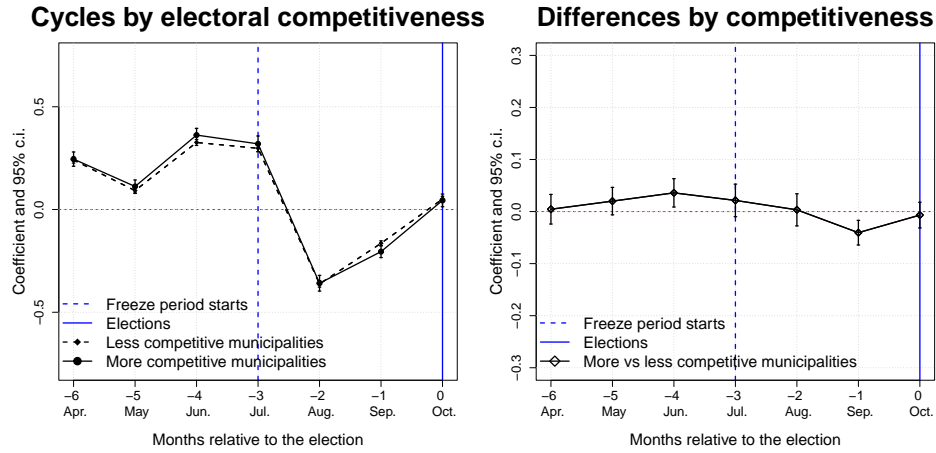
Left: points and their confidence intervals (c.i.) correspond to $\hat{\beta}$ coefficients (diamonds) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients (circles) in Equation 2. Right: points and their c.i. correspond to $\hat{\delta}$ coefficients in Equation 2. Regression details are in Appendix H.

Figure 4: Political bureaucratic cycles in total hires, by legislature size
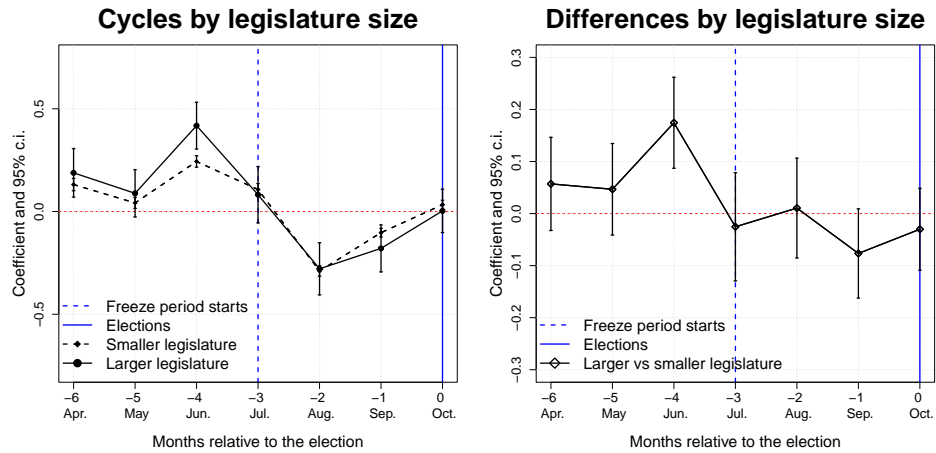
Left: points and their confidence intervals (c.i.) correspond to $\hat{\beta}$ coefficients (diamonds) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients (circles) in Equation 3. Right: points and their c.i. correspond to $\hat{\delta}$ coefficients in Equation 3. Regression details are in Appendix I.

having a larger legislature leads to more pronounced cycles in hiring because mayors in those municipalities face greater political pressure or coordination demands, and use hiring to build and sustain alliances in the lead-up to elections. By isolating exogenous variation on mayors' political

incentives to engage in patronage, this design provides quasi-experimental evidence that incentives are a key contributor to the patterns revealed in Figure 2.

Better law enforcement intensifies anticipatory cycles

Two additional pieces of evidence support the hypothesis that the legal constraints in hiring around elections shape the cycles documented in Figure 2. First, cycles in hiring intensified after the LRF was passed in 2000. Figure 5 illustrates that whereas hiring declined during the freeze period and expanded before it in the period 1995-1999, both patterns became more pronounced after the passage of the LRF and the accompanying reform of the penal code. For example, in June of an election year hiring expanded by an additional 25.73% compared to that same effect before 2000 and relative to June of a non-election year ($p < 0.001$). While these results are merely correlational, the fact that both the decline in hiring during the freeze period and its expansion in the months leading up to it became more pronounced after the passage of the LRF suggests that cycles are indeed shaped by the legal mandates and the legal penalties associated with breaches. Since government jobs typically last until the end of the year (if they are temporary contracts) or are permanent (if in the civil service), and absolute numbers of hires are larger in the second than in the third quarter of the year (Appendix C), the widening of the pre-freeze hiring boom implies a larger fiscal footprint that is not offset by the more marked declines during the freeze period.

To obtain causal evidence of how enforcement affects political bureaucratic cycles, I exploit the CGU randomized audits, which constitute an exogenous shock to the salience and enforcement of anti-corruption rules, including the laws that seek to curb electoral cycles. Figure 6 shows that randomized anti-corruption audits intensify political bureaucratic cycles in hiring: audits cause more pronounced expansions in hiring ahead of the freeze and stronger declines during the freeze. Audited municipalities experience an additional 8.44% increase in hiring in July and an additional 8.24% decline in hiring in September of an electoral year, relative to municipalities that were not previously exposed to an audit ($p < 0.001$).

The finding that municipalities exposed to a federal anti-corruption audit experience not only sharper declines in hiring during the freeze period (as one would expect with better enforcement of rules) but also a stronger increase in hiring ahead of the ban supports the idea that legal constraints shape the cycles documented in Figure 2. This builds on research by Avis et al. (2018), who show that Brazil's federal audits increase local government officials' perceived judicial costs of engaging

Figure 5: Political bureaucratic cycles in total hires, before and after the passage of the 2000 Fiscal Responsibility Law
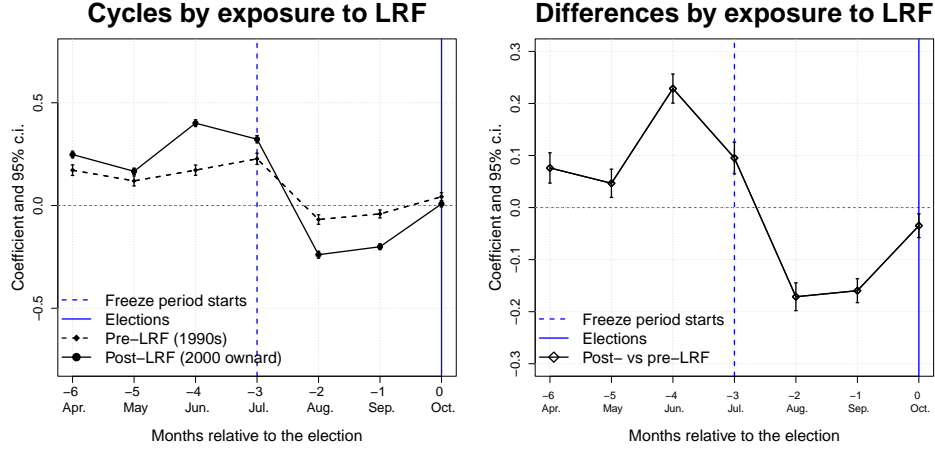
Left: points and their confidence intervals (c.i.) correspond to $\hat{\beta}$ coefficients (diamonds) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients (circles) in Equation 2. Right: points and their c.i. correspond to $\hat{\delta}$ coefficients in Equation 2. Regression details are in Appendix J.

Figure 6: Political bureaucratic cycles in total hires, by exposure to an anti-corruption audit
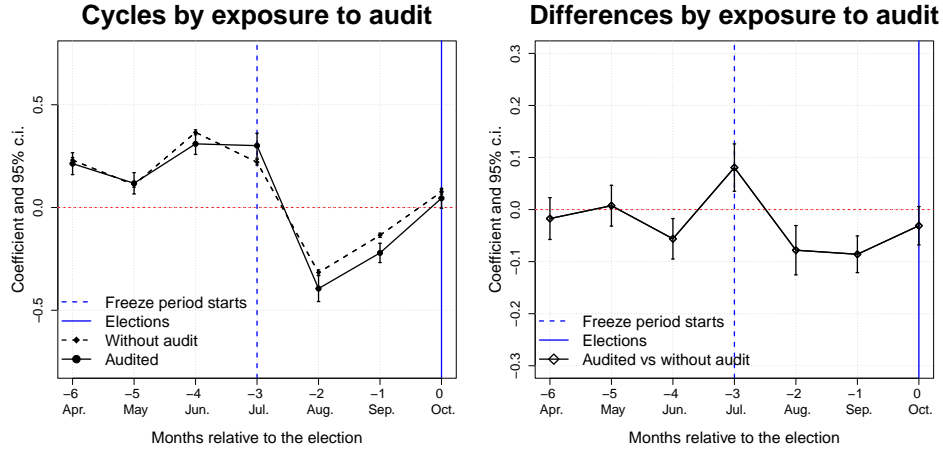
Left: points and their confidence intervals (c.i.) correspond to $\hat{\beta}$ coefficients (diamonds) and to the linear combination of $\hat{\beta}$ and $\hat{\delta}$ coefficients (circles) in Equation 2. Right: points and their c.i. correspond to $\hat{\delta}$ coefficients in Equation 2.
Regression details are in Appendix K.

in corruption, leading to reductions in corruption. The results depicted in Figure 6 add to these prior findings by highlighting how politicians respond strategically to these heightened perceptions of risk

not by abstaining from boosting employment ahead of elections but, at least partly, displacing it to a period not covered by the ban.

Electoral returns to pre-election hiring booms

Do the cycles in municipal hiring, as shaped by the legal constraints politicians face during the freeze period, influence electoral outcomes? While I cannot identify the causal effect of hiring booms, observational analyses suggest that they do improve mayors' electoral performance. Models 1, 4, and 7 in Table 4 show that a one-standard-deviation increase in the pre-freeze hiring boom is associated with a 2.5 percentage-points boost in the probability that the mayor runs for reelection ($p < 0.001$) and, conditional on them running, increases of 0.7 percentage points in their electoral margin ($p < 0.01$) and of 2.1 percentage points in their probability of winning ($p < 0.001$). Results are similar in magnitude and precision when including state and election cycle fixed effects (models 2, 5, and 8) as well as municipal-level covariates (models 3, 6, and 9). While these results are observational rather than causal, the stability of the estimates when including covariates suggests they are not merely driven by unobserved confounders.

Table 4: Association between political bureaucratic cycles and the incumbent's electoral performance

	Incumbent ran			Incumbent margin			Incumbent won		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hiring boom	0.025***	0.026***	0.027***	0.007**	0.007**	0.006*	0.021***	0.020***	0.023***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)	(0.005)	(0.005)	(0.005)
Fixed effects		✓	✓		✓	✓		✓	✓
Covariates			✓			✓			✓
Observations	21,498	21,498	21,447	9,996	9,996	9,994	9,996	9,996	9,994
R^2	0.003	0.023	0.025	0.0009	0.029	0.032	0.002	0.031	0.036

Models with year and state fixed effects use state-clustered fixed effects. Models with covariates control for the municipality's population and GDP per capita (logged) as well as incumbent party fixed effects for the 3 main parties at the time (MDB, PSDB and PT). *p<0.05; **p<0.01; ***p<0.001.

The associations reported in Table 4 matter because they suggest that the pre-freeze expansion of hiring induced by the election-timed constraints in government discretion help incumbents not only circumvent the regulation but also gain undue electoral advantages. While the coefficients may appear small, they constitute substantive improvements in incumbents' electoral prospects in

relative terms. For example, incumbent vote margins in these election cycles average 3.7 points and only 56.2% of those who ran were re-elected.

Conclusion

A vast literature in political science, public administration, and economics has established that politicians often manipulate policy tools (e.g., spending, procurement, or transfers) in the run-up to elections to improve their chances of reelection. From a democratic standpoint, these political budget cycles are worrisome because incumbents use them to abuse their control over the government to the detriment of electoral fairness. From an economic perspective, this manipulation is problematic because it can jeopardize fiscal discipline. In response, countries sometimes adopt laws that restrict political discretion in the weeks or months before elections – restricting the use of tools such as spending, procurement, or hiring during certain periods.

This article argues that election-time legal constraints on political discretion can displace, rather than eliminate, political budget cycles. When reelection incentives are strong, politicians adapt strategically to these constraints by moving their opportunistic manipulation earlier rather than abstaining from it. Combined, legal constraints and politicians' strategic adaptation to them create what I term anticipatory cycles. As a result, these well-meaning legal constraints on political discretion around elections may ultimately widen the cycles they seek to contain, which may further jeopardize electoral fairness and fiscal discipline.

I test this argument with an analysis of political bureaucratic cycles –political cycles in the hiring of government employees– in Brazilian municipalities, where politicians are prohibited by law from hiring in the months leading up to elections. Using panels of over 1 million municipality-month observations built with administrative data, I show that while hiring drops during the freeze period, it surges in the months just before it. I then use observational and quasi-experimental strategies to establish that these anticipatory cycles are more pronounced where political incentives are stronger and where the legal constraints are more reliably enforced. Additional analyses indicate that these hiring booms are associated with electoral gains for incumbents.

These findings matter for both scholars and policymakers. While we know a great deal about the existence of (and variation in) political budget cycles, we know much less about the effectiveness of institutional strategies to constrain them. Freeze-period policies are attractive to reformers: they

are simple to implement, easy to monitor, and symbolically powerful. But this article demonstrates that such rules can induce early manipulation rather than prevent it. This insight extends beyond Brazil and beyond public employment. When legal constraints are narrow in time but incumbents have discretion over timing and intensity, strategic adaptation is likely. Similar patterns may affect other tools –procurement, transfers, or benefits– and in many other institutional contexts.

This article makes three theoretical and empirical contributions to the literature on political budget cycles and anti-corruption efforts. First, it advances our understanding of the temporal dynamics of cycles by highlighting how incentives and constraints vary throughout the electoral calendar, and illustrating meaningful month-by-month variation, which yearly analyses would conceal. Second, the article highlights that election-time constraints on government discretion can backfire because politicians can strategically respond by anticipating, rather than abstaining from, manipulation. Third, it uses quasi-experimental designs to move beyond correlational heterogeneity analyses and identify the causal effect of political incentives and law enforcement on cycles.

Finally, these results suggest that policymakers and institutional reformers should take strategic adaptation to anti-corruption policies seriously. While election-time constraints are appealing for their simplicity and ease of implementation, they are unlikely to be effective in contexts where political incentives are strong and incumbents retain discretion over the timing of manipulation. More structural constraints on government discretion –such as continuous fiscal rules, robust procurement oversight, or binding civil service rules– may be more effective at curbing opportunistic manipulation by incumbents. Yet even those must be designed to address not just *what* politicians can do, but also *when* and *how* they can do it. Ultimately, this article calls for paying closer attention to how political elites respond to formal rules – and for reform strategies that anticipate not just manipulation but strategic adaptation to the anti-corruption rules.

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Appendices

A   Additional details on legal constraints around elections

A.1   Rules in the Constitution on public employment

Brazil's Federal Constitution (promulgated on October 5, 1988) includes several rules constraining politicians' discretion over public employment.[26] 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 on 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.[27] 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,[28] 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

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

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

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

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

Brazil's Electoral Law (Law 9,504, approved on September 30, 1997)[29] 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.[30] 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).

A.4   Rules in the Law of Ineligibilities

Brazil's Law of Ineligibilities (Complementary Law 64, approved on May 18, 1990),[31] 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

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[29]The Electoral Law can be found at http://www.planalto.gov.br/ccivil_03/leis/l9504.htm.

[30]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.

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

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

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.[32] 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; and reformed by Law 10,028, approved on October 19, 2000) includes penalties for ordering expenses not authorized by law (e.g., the kinds of personnel expenses forbidden by the Fiscal Responsibility Law).[33] In particular, those are subject to between 1 and 4 years in prison (article 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.

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

[33]The Penal Code can be found at http://www.planalto.gov.br/ccivil_03/decreto-lei/del2848compilado.htm. The 2000 reform that introduced prison penalties for increasing personnel expenses in the lead-up to the elections is at https://www.planalto.gov.br/ccivil_03/LEIS/L10028.htm.

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.[34] Descriptive statistics for the data on municipal employees are reported in Table 5. 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.[35] 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.[36]

Municipal governments (like all formal employers) are legally required[37] to report data for all its employees[38] 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,[39] and compare them to all 5,569 municipalities.[40] As can be seen in Figure 11, 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, their exclusion from the data biases the results. This bias, however, is likely to be in the direction of attenuating results (i.e. bringing them closer to zero)

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

[35]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*).

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

[37]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.

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

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

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

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 5: 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 7: Socioeconomic characteristics of municipalities not reporting employment data in 2016
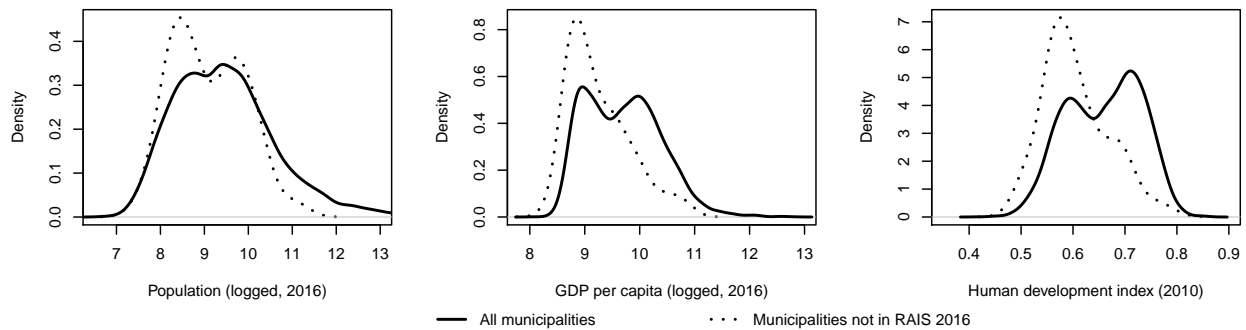
C   Distribution of municipal hiring by month

Figure 8: Distribution of hires, by month and type
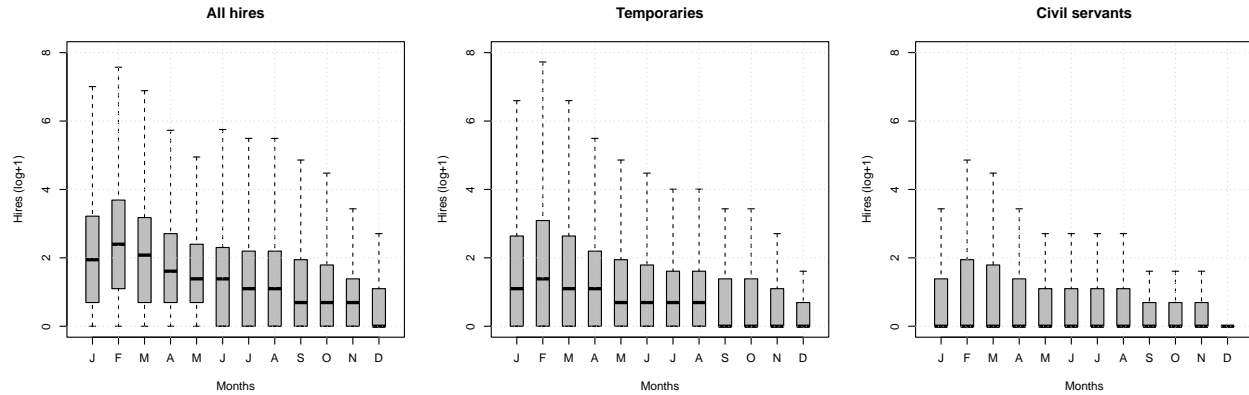
Horizontal lines correspond to the median. Boxes cover the interquartile range.

Figure 9: Distribution of resignations, by month and type
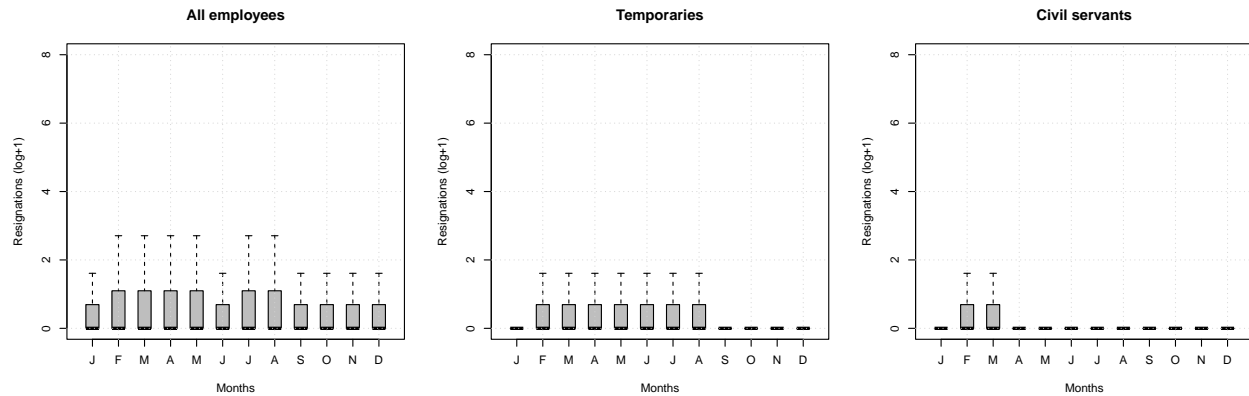
D Continuity of municipal population around the threshold triggering one additional legislator in 2004

Figure 10: McCrary density test for the continuity of municipal population count for 2003 around the threshold of 47,619 residents
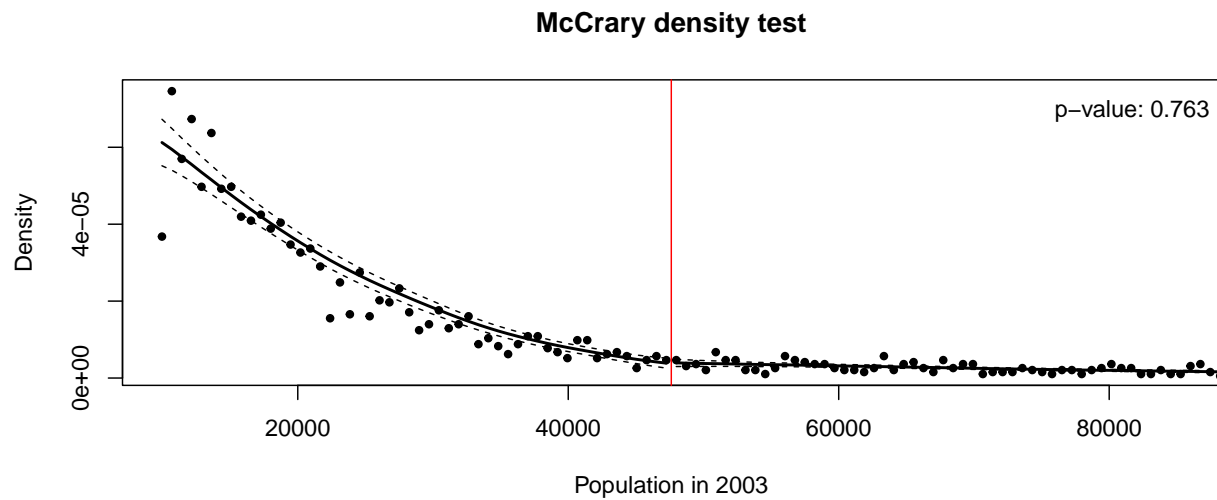
Figure 11: Cattaneo, Jansson and Ma density test for the continuity of municipal population count for 2003 around the threshold of 47,619 residents
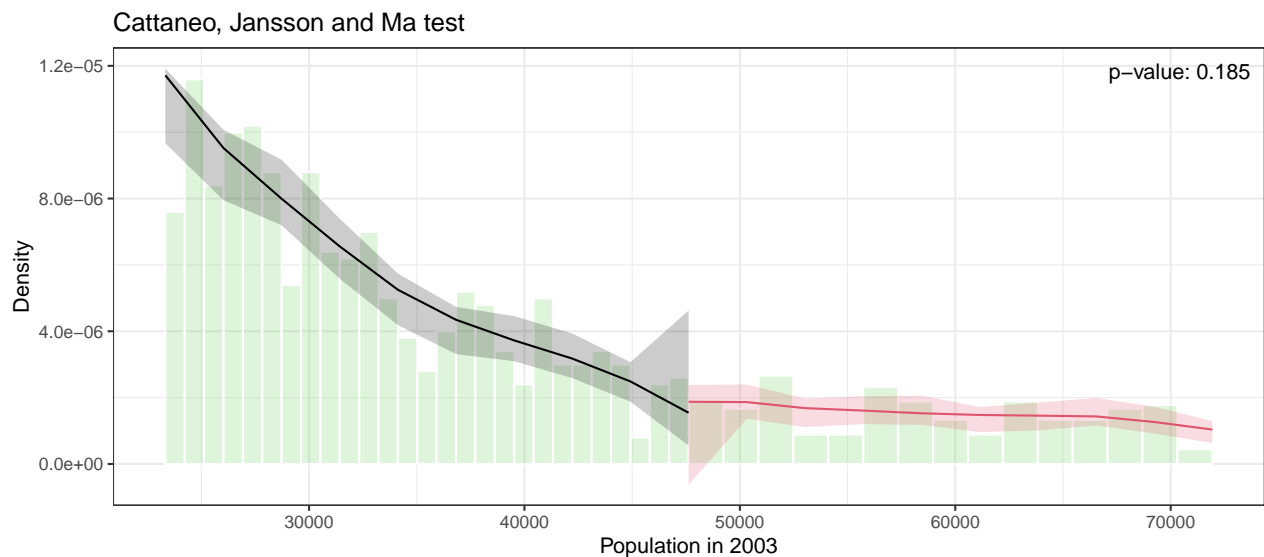
E Political cycles in hires, across groups of employees

Figure 12: Heterogeneity in cycles in total hires, by skill level and by policy sector
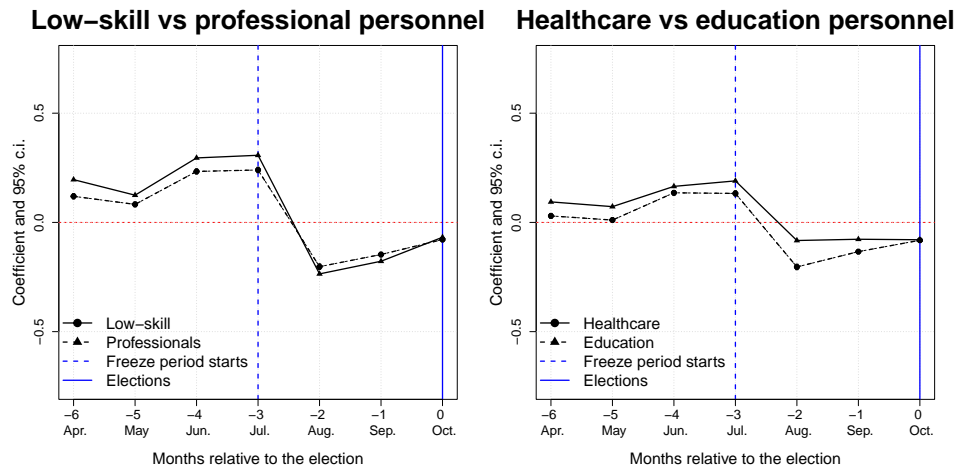
Points and their confidence intervals (c.i.) correspond to the $\hat{\beta}$ coefficients in Equation 1. Different lines correspond to different models run using panels built using data only for its corresponding subset of the municipal workforce.

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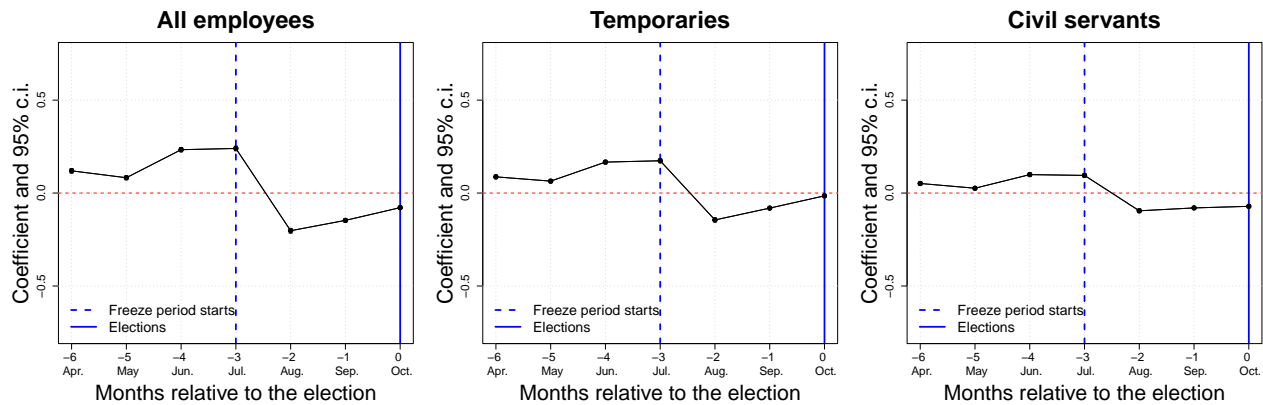
Figure 14: Political bureaucratic cycles in hires: Low-skill employees (CBO 4+)
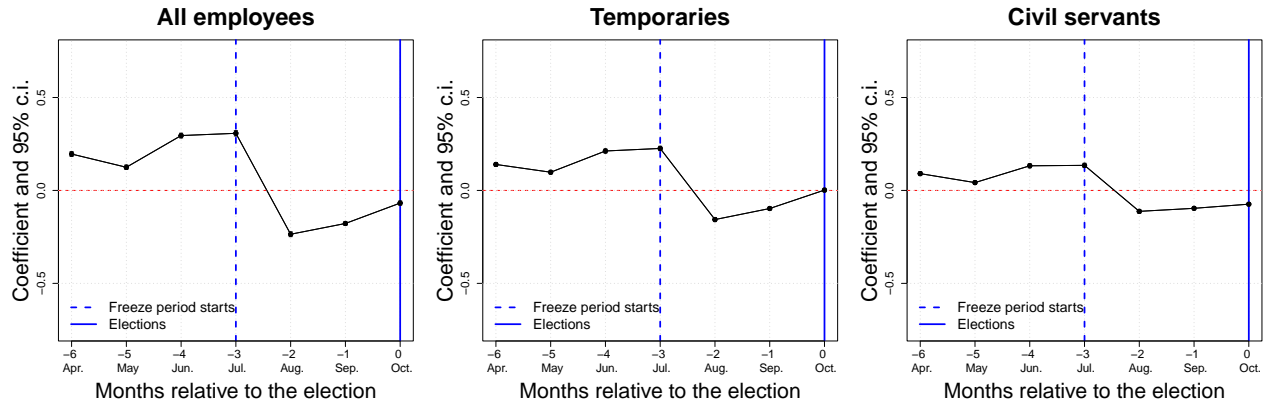
Points and their confidence intervals (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1.

Figure 15: Political bureaucratic cycles in hires: Healthcare professionals
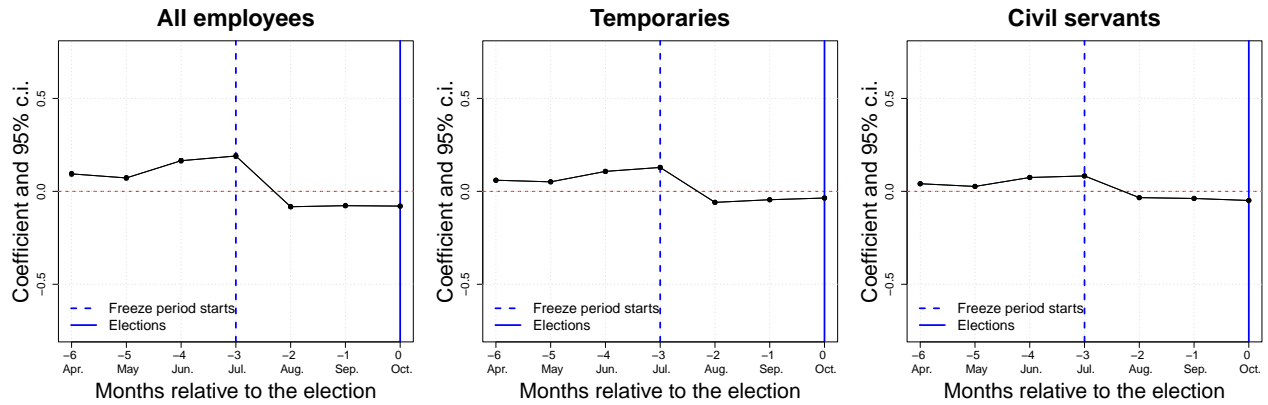
Figure 16: Political bureaucratic cycles in hires: Education professionals
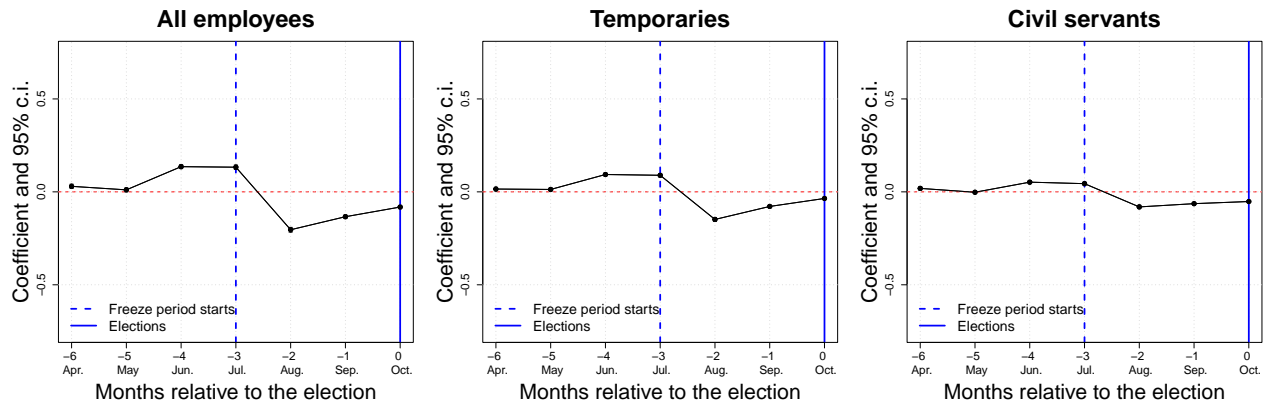
F Political cycles in resignations

Figure 17: Political bureaucratic cycles in placebo outcomes: Employee deaths and retirements
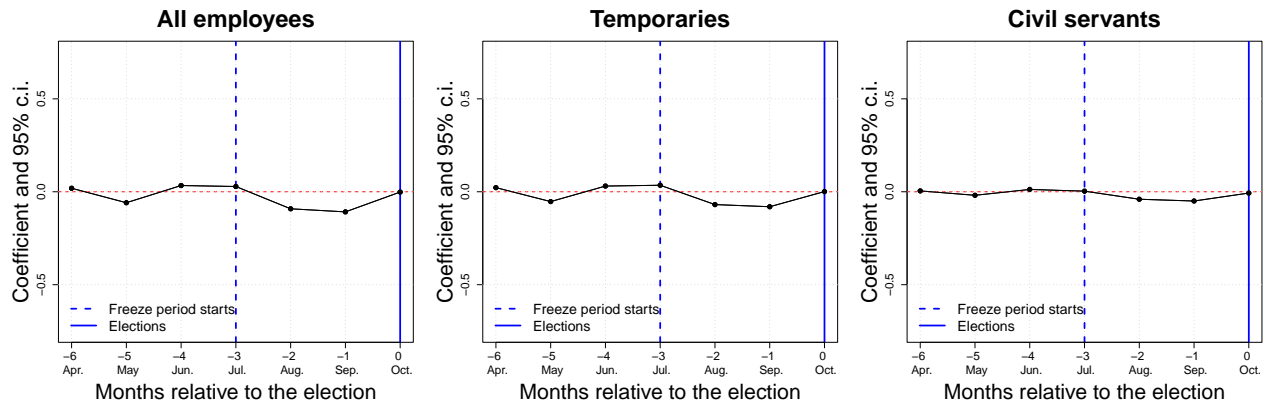
Points and their confidence intervals (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1.
Regression details are in Appendix G.

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

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.019***	0.022***	0.005
	(0.004)	(0.003)	(0.003)
May	-0.059***	-0.053***	-0.019***
	(0.003)	(0.003)	(0.003)
June	0.034***	0.031***	0.013***
	(0.004)	(0.003)	(0.003)
July	0.028***	0.035***	0.003
	(0.004)	(0.003)	(0.003)
August	-0.092***	-0.069***	-0.040***
	(0.003)	(0.003)	(0.003)
September	-0.108***	-0.081***	-0.050***
	(0.004)	(0.003)	(0.003)
October	-0.002	0.001	-0.007**
	(0.004)	(0.003)	(0.003)
Observations	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568
R^2	0.716	0.683	0.680

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

G Political cycles in placebo outcomes

Figure 18: Political bureaucratic cycles in placebo outcomes: Employee deaths and retirements
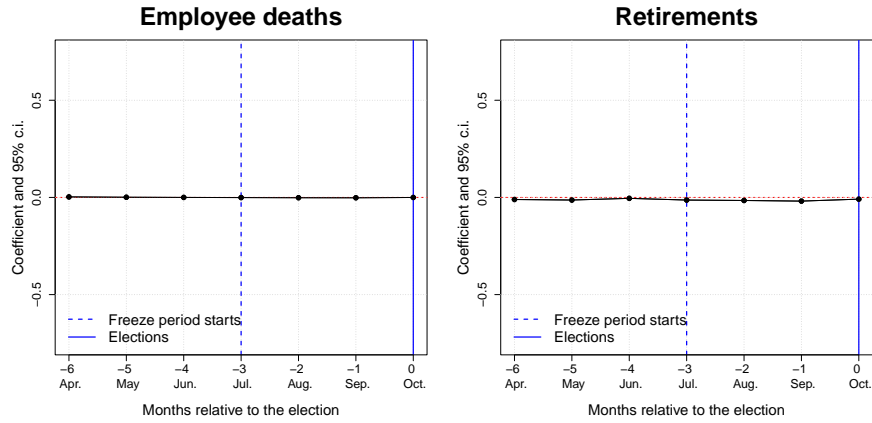
Points and their confidence intervals (c.i.) corresponds to the $\hat{\beta}$ coefficients in Equation 1. Regression details are in Appendix G.

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

	Deaths (1)	Retirements (2)
April	0.003	-0.011***
	(0.001)	(0.002)
May	0.0009	-0.013***
	(0.001)	(0.002)
June	-6.3×10^{-5}	-0.005*
	(0.001)	(0.002)
July	-0.0008	-0.013***
	(0.001)	(0.002)
August	-0.001	-0.016***
	(0.001)	(0.002)
September	-0.002	-0.019***
	(0.001)	(0.002)
October	-0.0003	-0.009***
	(0.001)	(0.002)
Observations	1,300,527	1,300,527
Municipalities	5,568	5,568
R^2	0.366	0.583

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

H   Political cycles in hires, by level of electoral competitiveness in the previous election

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

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.241***	0.189***	0.092***
	(0.008)	(0.007)	(0.007)
May	0.092***	0.066***	0.0007
	(0.007)	(0.006)	(0.006)
June	0.326***	0.235***	0.138***
	(0.007)	(0.006)	(0.007)
July	0.298***	0.216***	0.095***
	(0.008)	(0.007)	(0.007)
August	-0.362***	-0.270***	-0.223***
	(0.008)	(0.007)	(0.007)
September	-0.164***	-0.064***	-0.099***
	(0.006)	(0.005)	(0.005)
October	0.051***	0.126***	-0.035***
	(0.007)	(0.006)	(0.005)
More competitive $\times$ April	0.004	0.026	-0.016
	(0.015)	(0.013)	(0.015)
More competitive $\times$ May	0.020	0.006	0.015
	(0.013)	(0.012)	(0.013)
More competitive $\times$ June	0.036**	0.035**	0.029*
	(0.014)	(0.012)	(0.014)
More competitive $\times$ July	0.021	0.029*	0.027
	(0.016)	(0.014)	(0.015)
More competitive $\times$ August	0.003	-0.062***	0.033*
	(0.016)	(0.014)	(0.014)
More competitive $\times$ September	-0.041***	-0.062***	0.0009
	(0.012)	(0.011)	(0.011)
More competitive $\times$ October	-0.007	0.001	-9.8×10^{-5}
	(0.013)	(0.012)	(0.011)
Observations	1,241,728	1,241,728	1,241,728
Municipalities	5,568	5,568	5,568
R^2	0.608	0.618	0.502

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 parentheses. $^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

I Political cycles in hires, by legislature size as determined by a judicial ruling and a population threshold

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

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.131***	0.076***	0.053***
	(0.015)	(0.013)	(0.014)
May	0.042**	0.013	-0.005
	(0.014)	(0.011)	(0.012)
June	0.243***	0.136***	0.122***
	(0.014)	(0.011)	(0.012)
July	0.107***	0.058***	0.017
	(0.015)	(0.012)	(0.012)
August	-0.289***	-0.178***	-0.190***
	(0.013)	(0.011)	(0.011)
September	-0.103***	-0.055***	-0.068***
	(0.011)	(0.009)	(0.009)
October	0.033**	0.055***	-0.028**
	(0.011)	(0.010)	(0.009)
Larger legislature × April	0.057	0.066	0.066
	(0.046)	(0.043)	(0.051)
Larger legislature × May	0.047	0.037	0.064
	(0.045)	(0.040)	(0.047)
Larger legislature × June	0.174***	0.146***	0.142**
	(0.045)	(0.039)	(0.048)
Larger legislature × July	-0.025	-0.028	7.42×10^{-5}
	(0.053)	(0.046)	(0.054)
Larger legislature × August	0.011	-0.101*	0.050
	(0.049)	(0.044)	(0.048)
Larger legislature × September	-0.077	-0.100**	-0.014
	(0.044)	(0.038)	(0.043)
Larger legislature × October	-0.030	0.016	-0.063
	(0.040)	(0.036)	(0.036)
Observations	253,346	253,346	253,346
Municipalities	5,479	5,479	5,479
R^2	0.496	0.527	0.424

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 parentheses. *p<0.05; **p<0.01; ***p<0.001.

J Political bureaucratic cycles in hires, by exposure to the 2000 Fiscal Responsibility Law

Table 10: Political bureaucratic cycles in hires by timing relative to the 2000 Fiscal Responsibility Law (LRF) and by contract type

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.172***	0.101***	0.095***
	(0.013)	(0.010)	(0.011)
May	0.120***	0.059***	0.065***
	(0.012)	(0.009)	(0.010)
June	0.172***	0.102***	0.082***
	(0.013)	(0.010)	(0.010)
July	0.227***	0.134***	0.117***
	(0.014)	(0.011)	(0.011)
August	-0.068***	-0.045***	-0.040***
	(0.012)	(0.009)	(0.009)
September	-0.041***	-0.018*	-0.024**
	(0.010)	(0.008)	(0.008)
October	0.043***	0.038***	0.012
	(0.010)	(0.008)	(0.008)
Post-LRF × April	0.076***	0.098***	0.017
	(0.014)	(0.011)	(0.012)
Post-LRF × May	0.047***	0.076***	-0.004
	(0.014)	(0.010)	(0.011)
Post-LRF × June	0.229***	0.201***	0.105***
	(0.014)	(0.011)	(0.011)
Post-LRF × July	0.095***	0.126***	0.020
	(0.015)	(0.012)	(0.012)
Post-LRF × August	-0.171***	-0.112***	-0.092***
	(0.013)	(0.010)	(0.011)
Post-LRF × September	-0.160***	-0.093***	-0.087***
	(0.011)	(0.009)	(0.009)
Post-LRF × October	-0.035**	0.037***	-0.068***
	(0.011)	(0.009)	(0.009)
Observations	1,564,165	1,564,165	1,564,165
Municipalities	5,568	5,568	5,568
R^2	0.687	0.713	0.611

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

K Political bureaucratic cycles in hires, by exposure to a federal anti-corruption audit

Table 11: Political bureaucratic cycles in hires by anti-corruption audit and by contract type

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.231***	0.194***	0.082***
	(0.006)	(0.006)	(0.006)
May	0.111***	0.083***	0.011*
	(0.006)	(0.005)	(0.005)
June	0.366***	0.269***	0.155***
	(0.006)	(0.005)	(0.006)
July	0.220***	0.163***	0.059***
	(0.007)	(0.006)	(0.006)
August	-0.316***	-0.237***	-0.193***
	(0.007)	(0.006)	(0.006)
September	-0.135***	-0.044***	-0.082***
	(0.005)	(0.004)	(0.005)
October	0.076***	0.141***	-0.018***
	(0.006)	(0.005)	(0.005)
Audited × April	-0.017	-0.013	-0.007
	(0.021)	(0.018)	(0.020)
Audited × May	0.007	-0.0004	0.026
	(0.020)	(0.018)	(0.019)
Audited × June	-0.056**	-0.036*	-0.020
	(0.020)	(0.018)	(0.019)
Audited × July	0.081***	0.060**	0.067**
	(0.023)	(0.021)	(0.021)
Audited × August	-0.078**	-0.083***	-0.027
	(0.024)	(0.022)	(0.020)
Audited × September	-0.086***	-0.064***	-0.028
	(0.018)	(0.016)	(0.015)
Audited × October	-0.031	0.002	-0.027
	(0.019)	(0.017)	(0.014)
Observations	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568
R^2	0.608	0.616	0.500

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

L   Alternative specifications

Figure 19: Political bureaucratic cycles in hires, by contract type
Poisson regression (dependent variable and its lag unlogged)
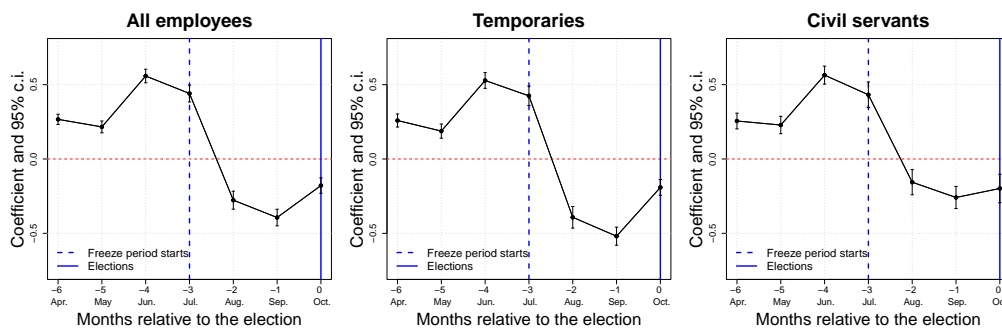
Figure 20: Political bureaucratic cycles in hires, by contract type
Expanded specification with 12-month window around elections
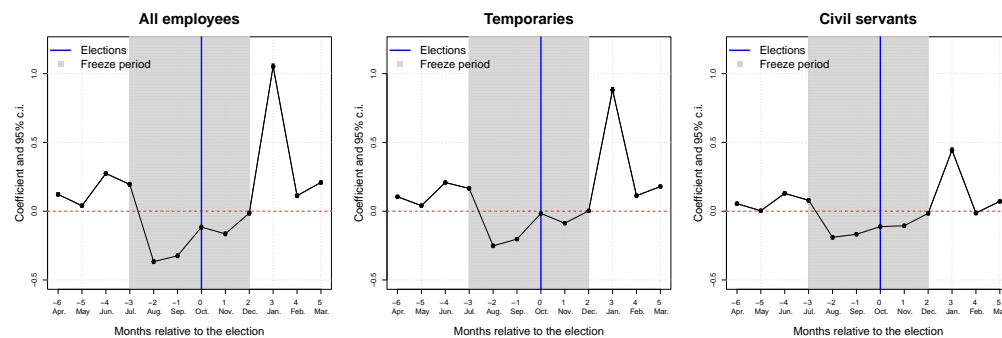
Figure 21: Political bureaucratic cycles in hires, by contract type
Month by month specification (each $\hat{\beta}$ coefficient estimated in a separate regression)
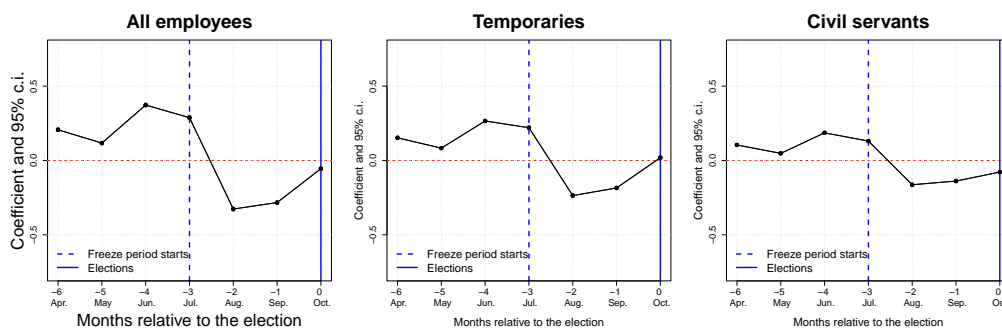
Figure 22: Political bureaucratic cycles in hires, by contract type
Log, without adding 1
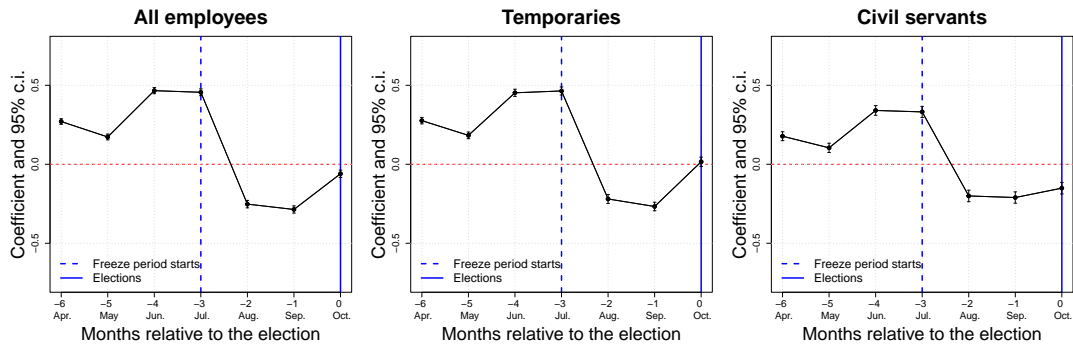
Figure 23: Political bureaucratic cycles in hires, by contract type
Inverse hyperbolic sine transformation
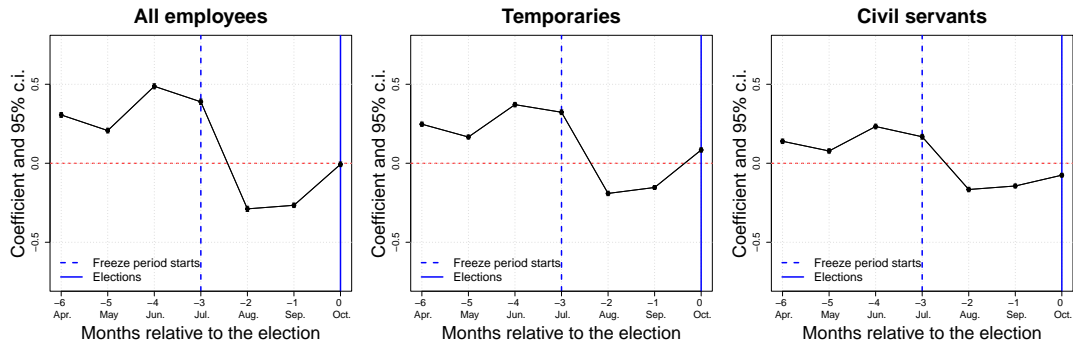
Figure 24: Political bureaucratic cycles in hires, by contract type
Binary measure of whether there are any hires
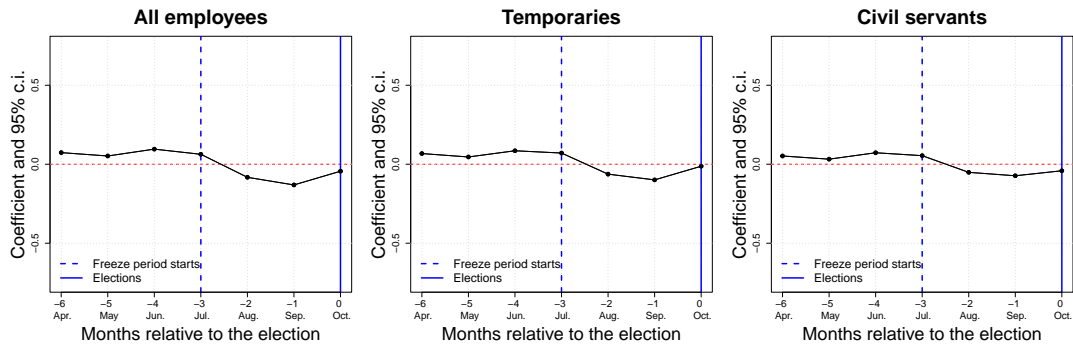
Figure 25: Political bureaucratic cycles in hires, by contract type
Omitting the lag of the dependent variable
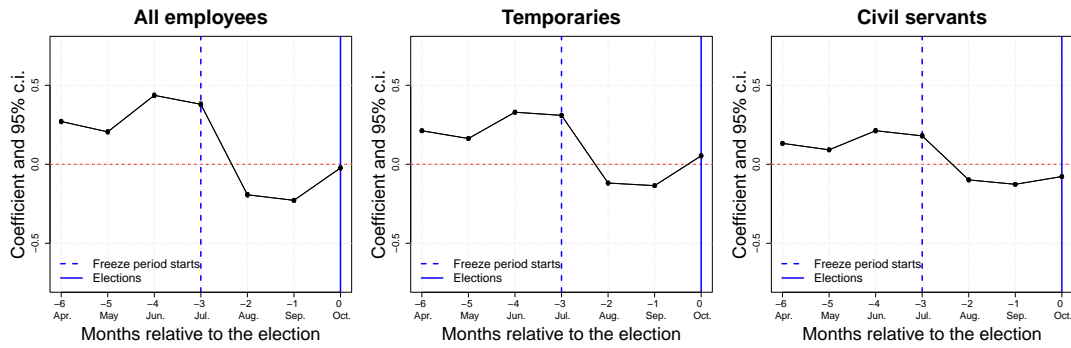
Figure 26: Political bureaucratic cycles in hires, by contract type
Municipality and year fixed effects instead of municipality-year fixed effects
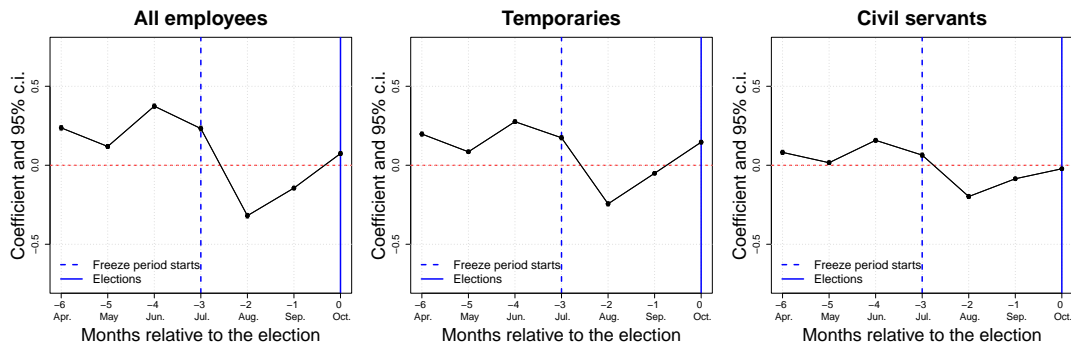
Figure 27: Political bureaucratic cycles in hires, by contract type
Omitting years with state and federal elections
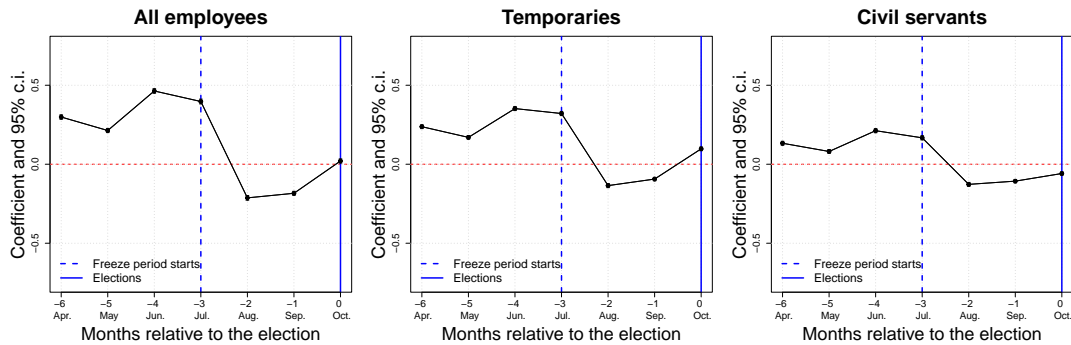
Figure 28: Political bureaucratic cycles in hires, by contract type
Balanced panel
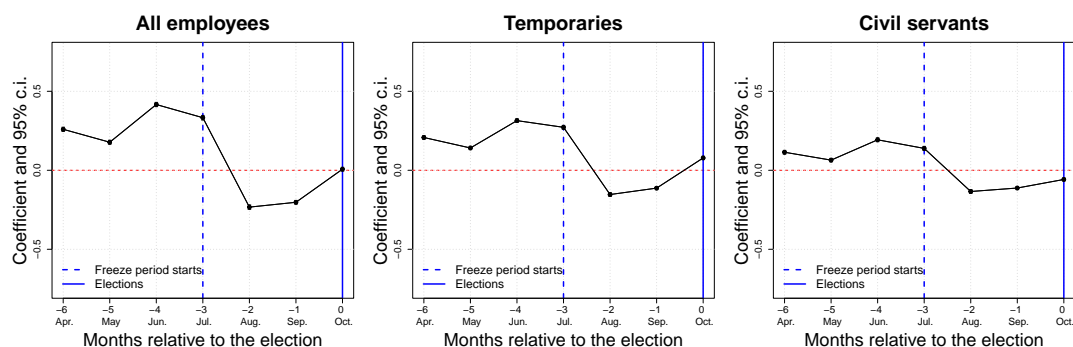
Figure 29: Political bureaucratic cycles in hires, by contract type
Standard errors clustered by municipality and by month
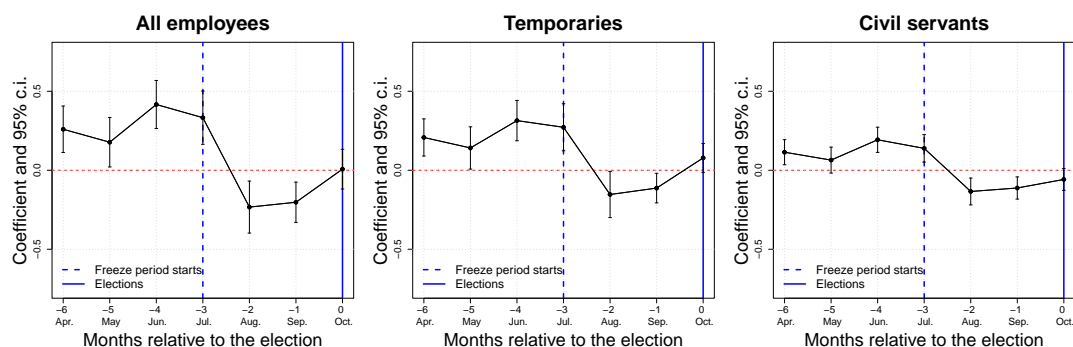
Figure 30: Political bureaucratic cycles in hires, by contract type
Standard errors clustered by municipality and by year

A-19
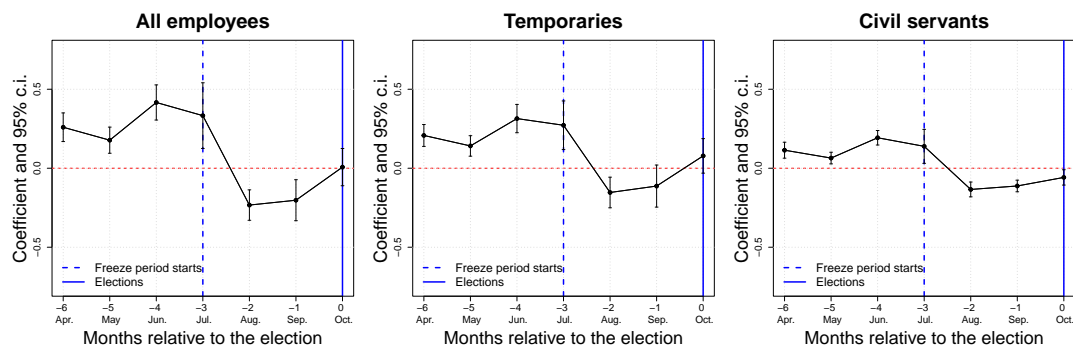
M   Complete regression tables

Following APSR policy, this appendix reports full regression tables for all the results reported in the main body and appendices of the paper. I omit the fixed effects, which are "partialled out" by the estimation procedure, but include estimates for all control variables as well as their interactions with the month fixed effects (which are denoted by "as.factor(month_num)" followed by the month number, where January is assigned 1 and December is assigned 12).

Table 12: Full regression table for the results presented in Figure 2

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.248***	0.198***	0.112***
	(0.006)	(0.005)	(0.005)
May	0.167***	0.135***	0.060***
	(0.006)	(0.005)	(0.005)
June	0.401***	0.302***	0.187***
	(0.006)	(0.005)	(0.006)
July	0.323***	0.261***	0.136***
	(0.007)	(0.006)	(0.006)
August	-0.239***	-0.157***	-0.133***
	(0.006)	(0.005)	(0.005)
September	-0.201***	-0.111***	-0.111***
	(0.005)	(0.005)	(0.005)
October	0.007	0.075***	-0.055***
	(0.005)	(0.005)	(0.004)
Lagged DV	0.117***	0.137***	0.175***
	(0.002)	(0.002)	(0.002)
Observations	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568
R^2	0.684	0.711	0.612

*p<0.05; **p<0.01; ***p<0.001.

Table 13: Full regression table for the results presented in Figure 3

	Total (1)	Temporaries (2)	Civil servants (3)
More competitive	0.023	0.104***	-0.020
	(0.015)	(0.016)	(0.013)
April	0.241***	0.189***	0.092***
	(0.008)	(0.007)	(0.007)
May	0.092***	0.066***	0.0007
	(0.007)	(0.006)	(0.006)
June	0.326***	0.235***	0.138***
	(0.007)	(0.006)	(0.007)
July	0.298***	0.216***	0.095***
	(0.008)	(0.007)	(0.007)
August	-0.362***	-0.270***	-0.223***
	(0.008)	(0.007)	(0.007)
September	-0.164***	-0.064***	-0.099***
	(0.006)	(0.005)	(0.005)
October	0.051***	0.126***	-0.035***
	(0.007)	(0.006)	(0.005)
Lagged DV	0.361***	0.482***	0.495***
	(0.003)	(0.003)	(0.003)
More competitive × April	0.004	0.026	-0.016
	(0.015)	(0.013)	(0.015)
More competitive × May	0.020	0.006	0.015
	(0.013)	(0.012)	(0.013)
More competitive × June	0.036**	0.035**	0.029*
	(0.014)	(0.012)	(0.014)
More competitive × July	0.021	0.029*	0.027
	(0.016)	(0.014)	(0.015)
More competitive × August	0.003	-0.062***	0.033*
	(0.016)	(0.014)	(0.014)
More competitive × September	-0.041***	-0.062***	0.0009
	(0.012)	(0.011)	(0.011)
More competitive × October	-0.007	0.001	-9.8×10^{-5}
	(0.013)	(0.012)	(0.011)
More competitive × as.factor(month_num)2	0.022	-0.006	0.009
	(0.020)	(0.020)	(0.015)
More competitive × as.factor(month_num)3	-0.017	-0.079***	0.004
	(0.016)	(0.015)	(0.013)
More competitive × as.factor(month_num)4	0.021	-0.070***	0.051***
	(0.015)	(0.015)	(0.013)
More competitive × as.factor(month_num)5	0.004	-0.080***	0.021
	(0.015)	(0.015)	(0.013)
More competitive × as.factor(month_num)6	-0.032*	-0.110***	0.003
	(0.016)	(0.015)	(0.013)
More competitive × as.factor(month_num)7	-0.018	-0.106***	0.021
	(0.015)	(0.015)	(0.013)
More competitive × as.factor(month_num)8	0.035*	-0.044**	0.031*
	(0.015)	(0.015)	(0.012)
More competitive × as.factor(month_num)9	-0.029	-0.115***	-0.001
	(0.016)	(0.016)	(0.013)
More competitive × as.factor(month_num)10	-0.030	-0.121***	0.009
	(0.016)	(0.016)	(0.013)
More competitive × as.factor(month_num)11	-0.061***	-0.150***	-0.004
	(0.016)	(0.016)	(0.013)
More competitive × as.factor(month_num)12	-0.166***	-0.244***	-0.054***
	(0.018)	(0.019)	(0.015)
Observations	1,241,728	1,241,728	1,241,728
Municipalities	5,568	5,568	5,568
R^2	0.608	0.618	0.502

*p<0.05; **p<0.01; ***p<0.001.

Table 14: Full regression table for the results presented in Figure 4

	Total (1)	Temporaries (2)	Civil servants (3)
Larger legislature	0.569***	0.525***	0.293***
	(0.053)	(0.053)	(0.050)
April	0.131***	0.076***	0.053***
	(0.015)	(0.013)	(0.014)
May	0.042**	0.013	-0.005
	(0.014)	(0.011)	(0.012)
June	0.243***	0.136***	0.122***
	(0.014)	(0.011)	(0.012)
July	0.107***	0.058***	0.017
	(0.015)	(0.012)	(0.012)
August	-0.289***	-0.178***	-0.190***
	(0.013)	(0.011)	(0.011)
September	-0.103***	-0.055***	-0.068***
	(0.011)	(0.009)	(0.009)
October	0.033**	0.055***	-0.028**
	(0.011)	(0.010)	(0.009)
Population recentered	1.17×10^{-5}***	6.34×10^{-6}***	5.27×10^{-6}***
	(5.16×10^{-7})	(3.94×10^{-7})	(3.6×10^{-7})
Lagged DV	0.538***	0.642***	0.571***
	(0.004)	(0.004)	(0.005)
Larger legislature × April	0.057	0.066	0.066
	(0.046)	(0.043)	(0.051)
Larger legislature × May	0.047	0.037	0.064
	(0.045)	(0.040)	(0.047)
Larger legislature × June	0.174***	0.146***	0.142**
	(0.045)	(0.039)	(0.048)
Larger legislature × July	-0.025	-0.028	7.42×10^{-5}
	(0.053)	(0.046)	(0.054)
Larger legislature × August	0.011	-0.101*	0.050
	(0.049)	(0.044)	(0.048)
Larger legislature × September	-0.077	-0.100**	-0.014
	(0.044)	(0.038)	(0.043)
Larger legislature × October	-0.030	0.016	-0.063
	(0.040)	(0.036)	(0.036)
Larger legislature × as.factor(month_num)2	-0.082	-0.130*	0.047
	(0.062)	(0.056)	(0.058)
Larger legislature × as.factor(month_num)3	-0.207***	-0.278***	-0.097*
	(0.048)	(0.046)	(0.046)
Larger legislature × as.factor(month_num)4	-0.219***	-0.336***	-0.113*
	(0.048)	(0.046)	(0.046)
Larger legislature × as.factor(month_num)5	-0.249***	-0.329***	-0.151***
	(0.046)	(0.045)	(0.044)
Larger legislature × as.factor(month_num)6	-0.343***	-0.410***	-0.184***
	(0.047)	(0.046)	(0.044)
Larger legislature × as.factor(month_num)7	-0.272***	-0.356***	-0.146**
	(0.047)	(0.044)	(0.046)
Larger legislature × as.factor(month_num)8	-0.343***	-0.388***	-0.175***
	(0.048)	(0.046)	(0.045)
Larger legislature × as.factor(month_num)9	-0.375***	-0.424***	-0.216***
	(0.048)	(0.047)	(0.045)
Larger legislature × as.factor(month_num)10	-0.429***	-0.436***	-0.244***
	(0.048)	(0.046)	(0.045)
Larger legislature × as.factor(month_num)11	-0.473***	-0.488***	-0.277***
	(0.047)	(0.047)	(0.044)
Larger legislature × as.factor(month_num)12	-0.808***	-0.752***	-0.444***
	(0.055)	(0.057)	(0.051)
Larger legislature × Population recentered	-1.14×10^{-5}***	-6.18×10^{-6}***	-5.02×10^{-6}***
	(5.19×10^{-7})	(3.95×10^{-7})	(3.63×10^{-7})
Observations	253,346	253,346	253,346
Municipalities	5,479	5,479	5,479
R^2	0.496	0.527	0.424

*p<0.05; **p<0.01; ***p<0.001.

Table 15: Full regression table for the results presented in Figure 5

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.172***	0.101***	0.095***
	(0.013)	(0.010)	(0.011)
May	0.120***	0.059***	0.065***
	(0.012)	(0.009)	(0.010)
June	0.172***	0.102***	0.082***
	(0.013)	(0.010)	(0.010)
July	0.227***	0.134***	0.117***
	(0.014)	(0.011)	(0.011)
August	-0.068***	-0.045***	-0.040***
	(0.012)	(0.009)	(0.009)
September	-0.041***	-0.018*	-0.024**
	(0.010)	(0.008)	(0.008)
October	0.043***	0.038***	0.012
	(0.010)	(0.008)	(0.008)
Lagged DV	0.118***	0.139***	0.172***
	(0.001)	(0.002)	(0.002)
Post-LRF × April	0.076***	0.098***	0.017
	(0.014)	(0.011)	(0.012)
Post-LRF × May	0.047***	0.076***	-0.004
	(0.014)	(0.010)	(0.011)
Post-LRF × June	0.229***	0.201***	0.105***
	(0.014)	(0.011)	(0.011)
Post-LRF × July	0.095***	0.126***	0.020
	(0.015)	(0.012)	(0.012)
Post-LRF × August	-0.171***	-0.112***	-0.092***
	(0.013)	(0.010)	(0.011)
Post-LRF × September	-0.160***	-0.093***	-0.087***
	(0.011)	(0.009)	(0.009)
Post-LRF × October	-0.035**	0.037***	-0.068***
	(0.011)	(0.009)	(0.009)
Post-LRF × as.factor(month_num)2	-0.029*	-0.044***	-0.003
	(0.012)	(0.011)	(0.009)
Post-LRF × as.factor(month_num)3	-0.298***	-0.275***	-0.120***
	(0.011)	(0.010)	(0.009)
Post-LRF × as.factor(month_num)4	-0.380***	-0.380***	-0.125***
	(0.011)	(0.010)	(0.009)
Post-LRF × as.factor(month_num)5	-0.462***	-0.451***	-0.153***
	(0.011)	(0.010)	(0.009)
Post-LRF × as.factor(month_num)6	-0.517***	-0.510***	-0.167***
	(0.011)	(0.010)	(0.009)
Post-LRF × as.factor(month_num)7	-0.521***	-0.512***	-0.168***
	(0.012)	(0.010)	(0.009)
Post-LRF × as.factor(month_num)8	-0.407***	-0.420***	-0.119***
	(0.012)	(0.010)	(0.009)
Post-LRF × as.factor(month_num)9	-0.521***	-0.526***	-0.169***
	(0.012)	(0.010)	(0.009)
Post-LRF × as.factor(month_num)10	-0.563***	-0.569***	-0.175***
	(0.012)	(0.011)	(0.009)
Post-LRF × as.factor(month_num)11	-0.642***	-0.633***	-0.210***
	(0.012)	(0.011)	(0.009)
Post-LRF × as.factor(month_num)12	-0.782***	-0.758***	-0.260***
	(0.013)	(0.012)	(0.010)
Observations	1,564,165	1,564,165	1,564,165
Municipalities	5,568	5,568	5,568
R^2	0.687	0.713	0.611

*p<0.05; **p<0.01; ***p<0.001.

Table 16: Full regression table for the results presented in Figure 6

	Total (1)	Temporaries (2)	Civil servants (3)
Audited	0.185***	0.208***	0.027
	(0.028)	(0.029)	(0.022)
April	0.231***	0.194***	0.082***
	(0.006)	(0.006)	(0.006)
May	0.111***	0.083***	0.011*
	(0.006)	(0.005)	(0.005)
June	0.366***	0.269***	0.155***
	(0.006)	(0.005)	(0.006)
July	0.220***	0.163***	0.059***
	(0.007)	(0.006)	(0.006)
August	-0.316***	-0.237***	-0.193***
	(0.007)	(0.006)	(0.006)
September	-0.135***	-0.044***	-0.082***
	(0.005)	(0.004)	(0.005)
October	0.076***	0.141***	-0.018***
	(0.006)	(0.005)	(0.005)
Lagged DV	0.368***	0.489***	0.498***
	(0.003)	(0.003)	(0.003)
Audited × April	-0.017	-0.013	-0.007
	(0.021)	(0.018)	(0.020)
Audited × May	0.007	-0.0004	0.026
	(0.020)	(0.018)	(0.019)
Audited × June	-0.056**	-0.036*	-0.020
	(0.020)	(0.018)	(0.019)
Audited × July	0.081***	0.060**	0.067**
	(0.023)	(0.021)	(0.021)
Audited × August	-0.078**	-0.083***	-0.027
	(0.024)	(0.022)	(0.020)
Audited × September	-0.086***	-0.064***	-0.028
	(0.018)	(0.016)	(0.015)
Audited × October	-0.031	0.002	-0.027
	(0.019)	(0.017)	(0.014)
Audited × as.factor(month_num)2	-0.160***	-0.176***	-0.047
	(0.033)	(0.032)	(0.025)
Audited × as.factor(month_num)3	-0.128***	-0.150***	-0.045*
	(0.027)	(0.027)	(0.023)
Audited × as.factor(month_num)4	-0.174***	-0.206***	-0.029
	(0.026)	(0.027)	(0.021)
Audited × as.factor(month_num)5	-0.167***	-0.207***	-0.016
	(0.026)	(0.027)	(0.021)
Audited × as.factor(month_num)6	-0.180***	-0.217***	-0.021
	(0.027)	(0.027)	(0.022)
Audited × as.factor(month_num)7	-0.157***	-0.193***	-0.024
	(0.027)	(0.027)	(0.021)
Audited × as.factor(month_num)8	-0.166***	-0.203***	-0.013
	(0.026)	(0.026)	(0.020)
Audited × as.factor(month_num)9	-0.174***	-0.224***	-0.009
	(0.027)	(0.028)	(0.022)
Audited × as.factor(month_num)10	-0.194***	-0.234***	-0.024
	(0.028)	(0.028)	(0.022)
Audited × as.factor(month_num)11	-0.217***	-0.249***	-0.028
	(0.029)	(0.029)	(0.023)
Audited × as.factor(month_num)12	-0.232***	-0.279***	-0.021
	(0.031)	(0.033)	(0.025)
Observations	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568
R²	0.608	0.616	0.500

*p<0.05; **p<0.01; ***p<0.001.

Table 17: Full regression table for the results presented in Table 4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant	0.478***			0.036***			0.561***		
	(0.003)			(0.003)			(0.005)		
Hiring boom	0.025***	0.026***	0.027***	0.007**	0.007**	0.006*	0.021***	0.020***	0.023***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)	(0.005)	(0.005)	(0.005)
PT incumbent			0.051***			0.006			0.005
			(0.008)			(0.006)			(0.014)
PSDB incumbent			-0.015			0.023**			0.070***
			(0.011)			(0.006)			(0.015)
MDB incumbent			-0.013			0.004			0.007
			(0.013)			(0.007)			(0.014)
Fixed effects		✓	✓		✓	✓		✓	✓
Covariates			✓			✓			✓
Standard-Errors	IID	uf		IID	uf		IID	uf	
Observations	21,498	21,498	21,447	9,996	9,996	9,994	9,996	9,996	9,994
R^2	0.003	0.023	0.025	0.0009	0.029	0.032	0.002	0.031	0.036

Regressions 1-3 have as dependent variable an indicator for whether the incumbent ran. Models 4-6 have as dependent variable the incumbemt's elec toral margin. Models 7-9 have as dependent variable an indicator for whether the incumbent won. $^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

Table 18: Full regression table for the results presented in Figure 13

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.120***	0.087***	0.051***
	(0.006)	(0.005)	(0.005)
May	0.083***	0.064***	0.026***
	(0.006)	(0.005)	(0.005)
June	0.233***	0.167***	0.099***
	(0.006)	(0.005)	(0.005)
July	0.240***	0.174***	0.095***
	(0.006)	(0.005)	(0.005)
August	-0.203***	-0.145***	-0.095***
	(0.006)	(0.005)	(0.005)
September	-0.147***	-0.081***	-0.080***
	(0.005)	(0.004)	(0.004)
October	-0.079***	-0.015***	-0.071***
	(0.005)	(0.004)	(0.004)
Lagged DV	0.090***	0.117***	0.147***
	(0.002)	(0.002)	(0.002)
Observations	1,092,580	1,092,580	1,092,580
Municipalities	5,566	5,566	5,566
R^2	0.629	0.640	0.563

*p<0.05; **p<0.01; ***p<0.001.

Table 19: Full regression table for the results presented in Figure 14

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.196***	0.140***	0.090***
	(0.006)	(0.005)	(0.005)
May	0.125***	0.098***	0.042***
	(0.006)	(0.005)	(0.005)
June	0.295***	0.212***	0.133***
	(0.007)	(0.005)	(0.005)
July	0.307***	0.226***	0.135***
	(0.007)	(0.006)	(0.006)
August	-0.236***	-0.157***	-0.113***
	(0.007)	(0.006)	(0.005)
September	-0.178***	-0.097***	-0.096***
	(0.005)	(0.005)	(0.004)
October	-0.068***	0.002	-0.074***
	(0.006)	(0.005)	(0.004)
Lagged DV	0.131***	0.145***	0.154***
	(0.002)	(0.002)	(0.002)
Observations	1,106,825	1,106,825	1,106,825
Municipalities	5,568	5,568	5,568
R^2	0.614	0.648	0.541

*p<0.05; **p<0.01; ***p<0.001.

Table 20: Full regression table for the results presented in Figure 15

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.094***	0.060***	0.041***
	(0.005)	(0.004)	(0.004)
May	0.072***	0.051***	0.026***
	(0.005)	(0.004)	(0.004)
June	0.165***	0.107***	0.075***
	(0.006)	(0.004)	(0.004)
July	0.190***	0.129***	0.083***
	(0.006)	(0.005)	(0.004)
August	-0.083***	-0.059***	-0.034***
	(0.005)	(0.004)	(0.004)
September	-0.077***	-0.045***	-0.038***
	(0.004)	(0.004)	(0.003)
October	-0.079***	-0.036***	-0.049***
	(0.004)	(0.003)	(0.003)
Lagged DV	0.063***	0.059***	0.097***
	(0.002)	(0.002)	(0.003)
Observations	1,067,501	1,067,501	1,067,501
Municipalities	5,564	5,564	5,564
R^2	0.524	0.552	0.456

$^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

Table 21: Full regression table for the results presented in Figure 16

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.030***	0.015**	0.019***
	(0.006)	(0.005)	(0.005)
May	0.011*	0.013**	-0.002
	(0.005)	(0.004)	(0.004)
June	0.136***	0.093***	0.052***
	(0.005)	(0.004)	(0.004)
July	0.133***	0.089***	0.044***
	(0.006)	(0.005)	(0.004)
August	-0.204***	-0.148***	-0.081***
	(0.006)	(0.005)	(0.005)
September	-0.134***	-0.079***	-0.064***
	(0.005)	(0.004)	(0.004)
October	-0.082***	-0.035***	-0.052***
	(0.004)	(0.004)	(0.003)
Lagged DV	0.077***	0.113***	0.115***
	(0.002)	(0.003)	(0.003)
Observations	1,074,996	1,074,996	1,074,996
Municipalities	5,559	5,559	5,559
R^2	0.545	0.546	0.471

*p<0.05; **p<0.01; ***p<0.001.

Table 22: Full regression table for the results presented in Figure 17

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.019***	0.022***	0.005
	(0.004)	(0.003)	(0.003)
May	-0.059***	-0.053***	-0.019***
	(0.003)	(0.003)	(0.003)
June	0.034***	0.031***	0.013***
	(0.004)	(0.003)	(0.003)
July	0.028***	0.035***	0.003
	(0.004)	(0.003)	(0.003)
August	-0.092***	-0.069***	-0.040***
	(0.003)	(0.003)	(0.003)
September	-0.108***	-0.081***	-0.050***
	(0.004)	(0.003)	(0.003)
October	-0.002	0.001	-0.007**
	(0.004)	(0.003)	(0.003)
Lagged DV	0.046***	0.049***	0.030***
	(0.002)	(0.002)	(0.002)
Observations	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568
R^2	0.716	0.683	0.680

*p<0.05; **p<0.01; ***p<0.001.

Table 23: Full regression table for the results presented in Figure 18

| | Deaths | Retirements |
	(1)	(2)
April	0.003	-0.011***
	(0.001)	(0.002)
May	0.0009	-0.013***
	(0.001)	(0.002)
June	-6.3×10^{-5}	-0.005*
	(0.001)	(0.002)
July	-0.0008	-0.013***
	(0.001)	(0.002)
August	-0.001	-0.016***
	(0.001)	(0.002)
September	-0.002	-0.019***
	(0.001)	(0.002)
October	-0.0003	-0.009***
	(0.001)	(0.002)
Lagged DV	-0.075***	-0.031***
	(0.002)	(0.002)
Observations	1,300,527	1,300,527
Municipalities	5,568	5,568
R^2	0.366	0.583

*p<0.05; **p<0.01; ***p<0.001.

Table 24: Full regression table for the results presented in Figure 19

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.267***	0.259***	0.255***
	(0.018)	(0.022)	(0.027)
May	0.216***	0.188***	0.228***
	(0.020)	(0.024)	(0.030)
June	0.558***	0.528***	0.564***
	(0.023)	(0.027)	(0.031)
July	0.441***	0.425***	0.432***
	(0.029)	(0.034)	(0.044)
August	-0.277***	-0.392***	-0.156***
	(0.031)	(0.037)	(0.043)
September	-0.394***	-0.519***	-0.259***
	(0.028)	(0.031)	(0.038)
October	-0.179***	-0.191***	-0.198***
	(0.026)	(0.027)	(0.049)
Lagged DV	-0.0002***	-0.0003***	-5.24×10^{-6}
	(5.67×10^{-5})	(4.75×10^{-5})	(0.0001)
Observations	1,230,805	1,050,314	968,562
Municipalities	5,568	5,544	5,486

*p<0.05; **p<0.01; ***p<0.001.

Table 25: Full regression table for the results presented in Figure 20

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.122***	0.105***	0.055***
	(0.007)	(0.006)	(0.006)
May	0.040***	0.040***	0.003
	(0.006)	(0.005)	(0.006)
June	0.274***	0.208***	0.130***
	(0.007)	(0.006)	(0.006)
July	0.195***	0.165***	0.079***
	(0.007)	(0.006)	(0.007)
August	-0.367***	-0.252***	-0.190***
	(0.007)	(0.006)	(0.006)
September	-0.324***	-0.203***	-0.168***
	(0.007)	(0.006)	(0.006)
October	-0.116***	-0.016**	-0.112***
	(0.007)	(0.006)	(0.006)
November	-0.165***	-0.087***	-0.106***
	(0.007)	(0.006)	(0.006)
December	-0.014	0.003	-0.016*
	(0.007)	(0.006)	(0.006)
January	1.05***	0.881***	0.444***
	(0.009)	(0.009)	(0.008)
February	0.112***	0.113***	-0.013*
	(0.007)	(0.007)	(0.006)
March	0.209***	0.179***	0.071***
	(0.006)	(0.006)	(0.006)
Lagged DV	0.125***	0.145***	0.177***
	(0.002)	(0.002)	(0.002)
Observations	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568
R^2	0.691	0.716	0.615

*p<0.05; **p<0.01; ***p<0.001.

Table 26: Full regression table for the results presented in the left-hand panel of Figure 21

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
April	0.206*** (0.006)						
Lagged DV	0.120*** (0.002)	0.120*** (0.002)	0.120*** (0.002)	0.118*** (0.002)	0.123*** (0.002)	0.119*** (0.002)	0.120*** (0.002)
May		0.117*** (0.005)					
June			0.373*** (0.006)				
July				0.288*** (0.006)			
August					-0.327*** (0.006)		
September						-0.283*** (0.005)	
October							-0.055*** (0.005)
Observations	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568	5,568	5,568	5,568	5,568
R^2	0.681	0.680	0.681	0.681	0.681	0.681	0.680

$^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

Table 27: Full regression table for the results presented in the central panel of Figure 21

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
April	0.152***						
	(0.005)						
Lagged DV	0.139***	0.139***	0.139***	0.138***	0.140***	0.138***	0.139***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
May		0.083***					
		(0.004)					
June			0.266***				
			(0.005)				
July				0.220***			
				(0.005)			
August					-0.236***		
					(0.005)		
September						-0.185***	
						(0.004)	
October							0.019***
							(0.004)
Observations	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568	5,568	5,568	5,568	5,568
R^2	0.709	0.709	0.709	0.709	0.709	0.709	0.709

*$p<0.05$; **$p<0.01$; ***$p<0.001$.

Table 28: Full regression table for the results presented in the right-hand panel of Figure 21

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
April	0.104***						
	(0.005)						
Lagged DV	0.177***	0.177***	0.177***	0.176***	0.178***	0.176***	0.177***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
May		0.048***					
		(0.005)					
June			0.186***				
			(0.005)				
July				0.131***			
				(0.005)			
August					-0.163***		
					(0.005)		
September						-0.139***	
						(0.004)	
October							-0.078***
							(0.004)
Observations	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527	1,300,527
Municipalities	5,568	5,568	5,568	5,568	5,568	5,568	5,568
R^2	0.611	0.611	0.611	0.611	0.611	0.611	0.611

*$p<0.05$; **$p<0.01$; ***$p<0.001$.

Table 29: Full regression table for the results presented in Figure 22

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.271***	0.276***	0.178***
	(0.009)	(0.010)	(0.014)
May	0.173***	0.183***	0.104***
	(0.009)	(0.011)	(0.015)
June	0.466***	0.453***	0.341***
	(0.010)	(0.012)	(0.015)
July	0.456***	0.464***	0.332***
	(0.011)	(0.013)	(0.018)
August	-0.252***	-0.220***	-0.200***
	(0.012)	(0.014)	(0.019)
September	-0.286***	-0.267***	-0.210***
	(0.012)	(0.014)	(0.018)
October	-0.060***	0.016	-0.151***
	(0.012)	(0.015)	(0.019)
Observations	633,328	461,655	291,645
Municipalities	4,386	4,337	4,265
R^2	0.645	0.657	0.589

*p$<$0.05; **p$<$0.01; ***p$<$0.001.

Table 30: Full regression table for the results presented in Figure 23

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.306***	0.247***	0.139***
	(0.008)	(0.007)	(0.007)
May	0.207***	0.166***	0.078***
	(0.008)	(0.006)	(0.007)
June	0.487***	0.371***	0.233***
	(0.008)	(0.007)	(0.008)
July	0.389***	0.323***	0.168***
	(0.009)	(0.008)	(0.008)
August	-0.289***	-0.191***	-0.166***
	(0.008)	(0.007)	(0.007)
September	-0.266***	-0.153***	-0.144***
	(0.007)	(0.006)	(0.006)
October	-0.007	0.084***	-0.075***
	(0.008)	(0.007)	(0.006)
Observations	1,050,965	1,050,965	1,050,965
Municipalities	4,389	4,389	4,389
R^2	0.687	0.718	0.622

*p<0.05; **p<0.01; ***p<0.001.

Table 31: Full regression table for the results presented in Figure 24

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.073***	0.068***	0.052***
	(0.003)	(0.003)	(0.003)
May	0.052***	0.046***	0.032***
	(0.003)	(0.003)	(0.003)
June	0.096***	0.085***	0.073***
	(0.003)	(0.003)	(0.003)
July	0.063***	0.071***	0.054***
	(0.003)	(0.003)	(0.003)
August	-0.083***	-0.063***	-0.051***
	(0.003)	(0.003)	(0.003)
September	-0.131***	-0.099***	-0.073***
	(0.003)	(0.003)	(0.003)
October	-0.044***	-0.013***	-0.041***
	(0.003)	(0.003)	(0.003)
Observations	1,050,965	1,050,965	1,050,965
Municipalities	4,389	4,389	4,389
R^2	0.498	0.595	0.530

*p$<$0.05; **p$<$0.01; ***p$<$0.001.

Table 32: Full regression table for the results presented in Figure 25

	Total (1)	Temporaries (2)	Civil servants (3)
April	0.271***	0.212***	0.133***
	(0.007)	(0.006)	(0.006)
May	0.205***	0.163***	0.091***
	(0.007)	(0.006)	(0.006)
June	0.436***	0.330***	0.213***
	(0.007)	(0.006)	(0.007)
July	0.380***	0.310***	0.180***
	(0.007)	(0.006)	(0.007)
August	-0.193***	-0.119***	-0.099***
	(0.007)	(0.006)	(0.006)
September	-0.228***	-0.135***	-0.127***
	(0.006)	(0.005)	(0.006)
October	-0.023***	0.053***	-0.078***
	(0.006)	(0.006)	(0.005)
Observations	1,051,380	1,051,380	1,051,380
Municipalities	4,389	4,389	4,389
R^2	0.683	0.710	0.606

$^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

Table 33: Full regression table for the results presented in Figure 26

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.237***	0.198***	0.082***
	(0.007)	(0.006)	(0.007)
May	0.119***	0.085***	0.016**
	(0.006)	(0.005)	(0.006)
June	0.374***	0.276***	0.158***
	(0.007)	(0.006)	(0.006)
July	0.232***	0.174***	0.064***
	(0.007)	(0.006)	(0.006)
August	-0.319***	-0.244***	-0.198***
	(0.007)	(0.006)	(0.006)
September	-0.144***	-0.051***	-0.086***
	(0.006)	(0.005)	(0.005)
October	0.074***	0.146***	-0.022***
	(0.006)	(0.006)	(0.005)
Lagged DV	0.368***	0.491***	0.504***
	(0.003)	(0.004)	(0.003)
Observations	1,050,965	1,050,965	1,050,965
Municipalities	4,389	4,389	4,389
R^2	0.612	0.620	0.508

$^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

Table 34: Full regression table for the results presented in Figure 27

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.299***	0.238***	0.133***
	(0.007)	(0.006)	(0.006)
May	0.214***	0.170***	0.081***
	(0.007)	(0.006)	(0.006)
June	0.464***	0.352***	0.212***
	(0.007)	(0.006)	(0.007)
July	0.397***	0.321***	0.167***
	(0.008)	(0.007)	(0.007)
August	-0.213***	-0.135***	-0.127***
	(0.008)	(0.007)	(0.006)
September	-0.184***	-0.094***	-0.107***
	(0.006)	(0.006)	(0.005)
October	0.021**	0.097***	-0.059***
	(0.007)	(0.006)	(0.005)
Lagged DV	0.116***	0.136***	0.177***
	(0.002)	(0.002)	(0.002)
Observations	788,007	788,007	788,007
Municipalities	4,389	4,389	4,389
R^2	0.692	0.717	0.621

*p<0.05; **p<0.01; ***p<0.001.

Table 35: Full regression table for the results presented in Figure 28

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.073***	0.068***	0.052***
	(0.003)	(0.003)	(0.003)
May	0.052***	0.046***	0.032***
	(0.003)	(0.003)	(0.003)
June	0.096***	0.085***	0.073***
	(0.003)	(0.003)	(0.003)
July	0.063***	0.071***	0.054***
	(0.003)	(0.003)	(0.003)
August	-0.083***	-0.063***	-0.051***
	(0.003)	(0.003)	(0.003)
September	-0.131***	-0.099***	-0.073***
	(0.003)	(0.003)	(0.003)
October	-0.044***	-0.013***	-0.041***
	(0.003)	(0.003)	(0.003)
Observations	1,050,965	1,050,965	1,050,965
Municipalities	4,389	4,389	4,389
R^2	0.498	0.595	0.530

*p<0.05; **p<0.01; ***p<0.001.

Table 36: Full regression table for the results presented in Figure 29

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.259***	0.208***	0.114***
	(0.007)	(0.006)	(0.006)
May	0.178***	0.141***	0.064***
	(0.006)	(0.005)	(0.006)
June	0.416***	0.314***	0.193***
	(0.007)	(0.006)	(0.006)
July	0.333***	0.272***	0.139***
	(0.007)	(0.006)	(0.006)
August	-0.233***	-0.154***	-0.134***
	(0.007)	(0.006)	(0.006)
September	-0.203***	-0.113***	-0.112***
	(0.006)	(0.005)	(0.005)
October	0.007	0.078***	-0.058***
	(0.006)	(0.006)	(0.005)
Lagged DV	0.115***	0.134***	0.179***
	(0.002)	(0.002)	(0.002)
Observations	1,050,965	1,050,965	1,050,965
Municipalities	4,389	4,389	4,389
R^2	0.687	0.715	0.619

*p<0.05; **p<0.01; ***p<0.001.

Table 37: Full regression table for the results presented in Figure 30

	Total	Temporaries	Civil servants
	(1)	(2)	(3)
April	0.259***	0.208***	0.114***
	(0.007)	(0.006)	(0.006)
May	0.178***	0.141***	0.064***
	(0.006)	(0.005)	(0.006)
June	0.416***	0.314***	0.193***
	(0.007)	(0.006)	(0.006)
July	0.333***	0.272***	0.139***
	(0.007)	(0.006)	(0.006)
August	-0.233***	-0.154***	-0.134***
	(0.007)	(0.006)	(0.006)
September	-0.203***	-0.113***	-0.112***
	(0.006)	(0.005)	(0.005)
October	0.007	0.078***	-0.058***
	(0.006)	(0.006)	(0.005)
Lagged DV	0.115***	0.134***	0.179***
	(0.002)	(0.002)	(0.002)
Observations	1,050,965	1,050,965	1,050,965
Municipalities	4,389	4,389	4,389
R^2	0.687	0.715	0.619

*p<0.05; **p<0.01; ***p<0.001.