

Political Budget Cycles and the Civil Service: Evidence from Highway Spending in US States*

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Abstract

We study political budget cycles in infrastructure spending that are conditional on bureaucratic organization. Bureaucrats can facilitate or hinder politicians' ability to engage in voter-friendly spending around elections. To test this idea, we use civil service reforms undertaken by US states in the second half of the 20th century to study political budget cycles in highway spending under civil service and patronage. We find that under patronage, highway spending is 12% higher in election years and 9% higher in the year before an election. By contrast, under civil service highway spending is essentially smooth over the electoral cycle. These findings provide a novel way through which civil service rules can stabilize government activity.

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1 Introduction

This paper brings together two large literatures: political budget cycles and bureaucracies. We show that bureaucratic organization can be an important factor in determining politicians' ability or incentives to create electoral cycles in government spending. In our context of study - highway spending by US state governments - we find significant budget cycles when state bureaucracy is organized based on political patronage, but spending becomes smooth under a civil service system. These findings indicate a novel way through which civil service rules can create "stability" in government.

Interest in political budget cycles, the timing of government spending decisions to the electoral cycle, has been long-standing. The recent literature emphasizes that cycles are context-conditional, depending on factors such as budgetary transparency, media freedom, electoral rules, or party polarization (Persson and Tabellini, 2003; Shi and Svensson, 2006; Alt and Lassen, 2006; Canes-Wrone and Park, 2012). We propose that bureaucratic organization is an important element of the institutional context that should also shape political budget cycles. Taking into account not just policy determination but also policy implementation may provide a useful complementary perspective on the institutions affecting the cycles.

Theoretically, the bureaucracy plays a mediating role between politicians and voters in at least two respects. First, bureaucrats often have considerable discretion in the implementation of policies. Therefore whether a policy initiative reaches and benefits a targeted group of voters depends in large part on the effort of bureaucrats. Second, voters are often forced to evaluate politicians and their policies with little direct information. When attempting to draw inferences regarding the quality of politicians and their policies, voters rely, at least to some extent, on information gained while interacting with bureaucrats. These mediating roles of the bureaucracy can facilitate or hinder politicians' ability to translate policies into votes. In particular, when a politician has less to gain from the timing of government spending, he will have less incentive to generate political cycles in the budget. Thus, an independent bureaucracy protected by civil service rules can limit politicians' incentive to create political budget cycles.¹

To test this idea, we study highway spending by US state governments in the second half of the 20th century. Spearheaded by the Interstate Highway program, highway construction and maintenance was one of the biggest areas of state government activity in this period. This is an area requiring technical expertise, but also one that can be highly politically

¹Besides lowering politicians' *incentive* to create cycles, protected bureaucrats can also have a direct impact on politicians' *ability* to do so, e.g., if they simply refuse to time spending decisions to the electoral cycle.

lucrative if a politician can make sure that a new road segment is built at the right place at the right time. To study highway spending with and without civil service, we take advantage of recently collected data on the timing of civil service reform in US state governments.

Throughout the 20th century US state governments changed their bureaucratic organization from political patronage to civil service (the “merit system”). Modeled after similar legislation at the federal level, these laws contained three key provisions. They (i) established merit-based recruitment through competitive civil service examination, (ii) prohibited requiring political services from employees and firing them for political reasons, and (iii) established a bipartisan Civil Service Commission or similar body to enforce the system. While the reforms were similar, they occurred at different times in different states and we use them as a natural experiment to study the impact of civil service on the budget cycles in highway expenditures. We study both the introduction of the statewide merit system and, using newly collected information, the introduction of merit systems specifically in states’ highway departments.

Our findings provide strong evidence for political budget cycles in highway spending that are conditional on bureaucratic organization. Under patronage, we find that highway spending is 9% higher in the year before an election and 12% higher in the election year as compared to the first year immediately following the election. By contrast we find no evidence of political budget cycles under civil service. Under civil service, highway spending is essentially smooth over the electoral cycle. These findings survive a variety of robustness checks, including different samples and estimation methods. We also provide evidence that the cycle under patronage is in fact driven by electoral considerations: it is not affected by politicians’ past experience in office, but disappears when future electoral considerations are shut down by term limits.

Our paper contributes to two literatures: political budget cycles and the bureaucracy. With respect to political budget cycles, we emphasize the importance of an institutional determinant that has not previously been studied.² Importantly, bureaucratic rules vary significantly across countries, our results therefore suggest a possible reason for some of the cross-country heterogeneity observed in the literature. Apart from this focus on a new source of institutional variation, our empirical setting has several advantages for identification relative to some of the earlier political budget cycle studies. First, unlike most of this literature, we study political budget cycles across jurisdictions within a country rather than across countries.³ This has the usual benefit of holding fixed a variety of confounding institutional

²See Drazen (2001), Eslava (2011), and de Haan and Klomp (2013) for surveys of the political budget cycle literature.

³Other studies offering within-country evidence include Akhmedov and Zhuravskaya (2004), Rose (2006), and Drazen and Eslava (2010).

and economic factors. In particular, it provides a context where election dates are exogenously fixed. Second, in our period of study some of the states changed their bureaucratic institutions, so we are not exclusively comparing budget cycles in the cross section. Third, we are studying heterogeneity in actual institutions rather than in an index of perceived institutions, avoiding concerns regarding what is being measured.⁴

With respect to bureaucracies, the broad question we address is: What is the impact of civil service rules? Several theoretical studies relate to this question at least indirectly by studying society’s incentives to delegate decisions to independent, expert bureaucrats (e.g., Maskin and Tirole, 2004; Alesina and Tabellini, 2007, 2008) or politicians’ incentives to do the same (Epstein and O’Halloran, 1999; Gailmard and Patty, 2007; Fox and Jordan, 2011; Ting, 2002, 2012). Ujhelyi (2014a) asks about the welfare effects of civil service rules when they affect the interaction of politicians and bureaucrats. The empirical literature on civil service rules is much smaller as the difficulty of obtaining comparable data on institutional reforms in a large number of jurisdictions often makes identification challenging. Early studies of the economic impact of the civil service include Rauch (1995), Rauch and Evans (2000), and Krause et al. (2006). To improve identification, the recent literature has focused on civil service reforms in US states, finding that it is more difficult for incumbent parties to remain in power under civil service than under patronage (Folke et al., 2011), and that politicians circumvent state bureaucracies that are under civil service by using intergovernmental transfers (Ujhelyi, 2014b).⁵ The present paper continues the agenda of seeking to understand the impact of civil service rules on politicians’ behavior by asking how the civil service shapes government spending over the electoral cycle. Our results suggest a possible channel for the adverse effect of civil service on incumbent politicians: under a civil service system, incumbents may lose some of their opportunities for creating political budget cycles. This finding generalizes the idea that a civil service system can ensure the “stability” and “continuity” of government activity in election times. While it is natural that, compared to patronage, civil service rules create stability within the bureaucracy, our findings show that this stability can extend to other areas of government as well - in particular, to the policy choices of election-minded politicians.

⁴Donchev and Ujhelyi (2014) discuss several issues related to measuring institutions, and de Haan and Klomp (2013) suggest that the political budget cycles literature has not paid enough attention to this question.

⁵See also Ornaghi (2016), who bypasses the lack of merit system information with an innovative intent-to-treat analysis to study impacts on police performance in US cities.

2 Hypothesis and context

2.1 Political budget cycles and the bureaucracy

In models of political budget cycles, election year spending differs from other years because politicians attempt to convey information to voters through their policy choices. This information could relate to their overall competence (Rogoff, 1990; Shi and Svensson, 2006) or to their preferences regarding the composition of government spending (Drazen and Eslava, 2010). Regardless of the nature of information conveyed, the budget cycle predicted by these models rests on two key assumptions: that politicians have direct control over policies, and that voters observe these policies. These assumptions allow voters to update their expectations about the incumbent's type based on the current policy. And it is because current policy affects voters' expectations that politicians have an incentive to create political budget cycles.

When spending decisions are made by politicians but implemented by bureaucrats, these assumptions often no longer hold. While politicians can set the budget, they may not be able to fully control how it is spent because of bureaucratic discretion. Similarly, voters may be able to more directly observe the policies as implemented (by bureaucrats) than the policies as chosen (by the politician) because it is the former that they will personally experience.⁶ This opens the possibility that the bureaucracy will affect politicians' ability and incentives to create political budget cycles. For example, a state highway engineer may simply be unwilling to move up a project's timeline to ensure that a highway segment is completed in an election year. Or he may be reluctant to execute the project according to the politician's preferences. In such cases, the nature and timing of observed government spending may be less informative to voters regarding the politician (as they will partly reflect bureaucrats' actions). In turn, this will reduce the incentive of politicians to create political budget cycles.

Bureaucratic discretion is likely to be larger under civil service than under patronage. Based on this, we expect political budget cycles to be more pronounced under patronage, and weaker or non-existent under civil service.⁷

⁶See Ujhelyi (2014a) for a formal model emphasizing these roles of the bureaucracy.

⁷The prediction that election year spending may differ from other years is not unique to political budget cycle models that focus on voters. For example, Khemani (2004) discusses Indian spending cycles that result from politicians catering to special interests in exchange for campaign support. Because support is more valuable in election years, spending in election years is also higher. In such a model too, bureaucratic discretion is likely to attenuate the cycles: by limiting politicians' ability to cater to special interests, bureaucratic discretion may lower their incentives to create budget cycles in the first place.

2.2 Highway spending and civil service reform in US states

Testing the idea that political budget cycles are a function of bureaucratic institutions requires a policy context that is politically important and where bureaucrats can have meaningful impacts on implementation. Highway construction in the post-war US satisfies these criteria: throughout the 35 years of the Interstate Highway program (“the world’s largest public works project”⁸) road construction was politically salient, and the technical nature of the projects created an important role for bureaucrats.⁹

The Appendix contains a detailed description of the institutional context, some of which we summarize here briefly. Highway expenditures (which include both construction and maintenance of highways and related structures) were one of the main areas of state government activity for most of the 20th century. Even though the majority of funding comes from federal sources (the Highway Trust Fund created in 1956), the laws make it state governments’ responsibility to decide which projects are undertaken, where they are located, and who is hired to work on them. The system affords state governors in particular a large degree of autonomy both from Congress and from state legislatures. In addition, because local voters only bear a fraction of the costs, highway spending is a potentially valuable political tool. Governors can benefit politically from details of project implementation such as timing, location, or the hiring of specific workers and contractors. All these details are factors that bureaucrats such as state highway engineers have considerable control over. The assumption underlying our analysis is that under patronage, these bureaucrats’ choices will often reflect the political preferences of the incumbent administration, while under civil service, they are more likely to reflect technical or efficiency considerations. Two case studies in the Appendix illustrate.

To compare patronage and civil service, we use data on civil service reforms undertaken in US states during the 20th century. US states’ civil service (or “merit”) systems were modeled after similar legislation at the federal level (the 1883 Pendleton Act). The key provisions of these laws were to (i) establish merit-based recruitment through competitive civil service examination, (ii) prohibit requiring political services from employees, or retaliating against them for failing to provide such services, and (iii) establish a bipartisan Civil Service Commission to promulgate rules and enforce the merit system. As originally adopted, the merit systems in different states were fairly uniform, and once adopted, they remained in place during our period of study. Although all states except Texas eventually adopted a statewide merit system, adoption of the reforms was slow: 24 states did not have a statewide

⁸<https://www.fhwa.dot.gov/interstate/quotable.cfm>

⁹Previous studies found political budget cycles in this type of spending in, e.g., India (Khemani, 2004) and Colombia (Drazen and Eslava, 2010).

merit system before 1950 and 19 did not have a statewide merit system before 1960 (see Table 1).¹⁰

What were the causes of these reforms? The empirical evidence suggests that most politicians had little to gain from giving up patronage: following the reforms there was a reduction in the number of public employees who could potentially provide political support (Ujhelyi, 2014b) and incumbents forced to give up patronage had trouble getting reelected (Folke et al., 2011). Historians and public administration scholars generally describe the reform movement as bottom-up, fueled by the good government movement rooted in the Progressive Era. According to this view, the main driver of reform was pressure from various citizen groups and the voters themselves (National Research Council, 1952; Tolchin and Tolchin, 1971; Mosher, 1982; Ingraham, 1995). Consistent with this, in several instances the transition to a statewide merit system was initiated by a referendum and codified in the state's constitution. There are of course other possible explanations for reform (see Ruhil and Camoes, 2003), and our empirical analysis below attempts to control for some of these.¹¹

While many states did not establish a centralized statewide merit system until the second half of the 20th century, every state had specific departments with their own merit systems before then.¹² To check whether this was the case for highway departments, we collected new data on the timing of merit system adoptions in these departments. Specifically, for each state that did not have a statewide merit system in 1960, we checked whether its highway department adopted its own merit system before the statewide merit system was introduced. Understandably, there is less information available on the personnel practices of highway departments than on those of state governments as a whole. A highway department may have a statutory merit system, or it may have a de facto merit system sustained by a department leadership that is committed to technical expertise and independence from politics.¹³ To establish whether a department operated a merit system we relied on several sources, including contemporary news reports, government documents, and a 1952 study by the Highway Research Board of the National Research Council (NRC, 1952). Details are

¹⁰Information on statewide merit system reforms comes from Ujhelyi (2014b). That paper uses a wide range of primary sources to pin down the exact year of reform, making the data appropriate for studying outcomes - such as highway spending - that change every year. For historical descriptions of the reform process, see Mosher (1982) and Ingraham (1995).

¹¹Since 1996, a number of states have begun a new wave of reforms generally weakening civil service protections. These have included provisions making it easier to fire employees and increasing the flexibility in pay-setting procedures. Because these reforms are more heterogenous than the first wave, studying them directly is left for future research.

¹²One example are departments administering funds under the Social Security Act (such as public health and social welfare). In 1939-1940 federal legislation mandated the introduction of merit systems for these departments in all states as a condition for funding. No such requirement was adopted for any other department, including highway departments.

¹³See the example of Texas described in detail in the Appendix.

given in the Appendix.

In our period of study only 5 states had their highway departments introduce a merit system before the statewide merit system was established: Arizona, Idaho, Texas, South Carolina, and Washington (see Table 1). For all other states that did not have a statewide merit system by 1960, their highway department came under civil service when the statewide merit system was eventually established. In the main analysis we show that we get similar results using either the statewide or the department-specific civil service reforms. We explore potential differences in the impact of the two reforms in section 5.3.

Table 1: States in the sample with no statewide merit system before 1960

State	Statewide merit system introduced	Prior Highway Department merit system introduced	State in sample both before and after reform
West Virginia	1989		*
Mississippi	1977		*
Montana	1976		*
North Dakota	1975		*
South Dakota	1973		
Arkansas	1969		
South Carolina	1969	1950	*
Arizona	1968	1957	
Delaware	1968		*
Florida	1967		*
Idaho	1967	1951	*
Iowa	1967		
Pennsylvania	1963		*
Utah	1963		*
New Mexico	1961		
Washington	1961	1955	*
Kentucky	1960		
Texas	-	1940	

Notes: States in our sample not listed here introduced a statewide merit system before 1960 (Nebraska introduced the system in 1975 but is excluded from the sample). A missing year in the second column indicates that the state highway department first came under a merit system when the statewide civil service was established. Texas never had a statewide merit system. States marked in the third column contribute observations both before and after their statewide reform, other states' pre-reform observations are dropped when we restrict attention to 4-year gubernatorial terms. Dates of statewide merit system adoptions are from Ujhelyi (2014b). See the Appendix for the sources of the highway department-specific information.

3 Data

Our period of study is 1960-1995. The starting date reflects two considerations: data availability, and the establishment of the federal Highway Trust fund in 1956. The latter not only

gave a boost to highway construction throughout the US, it also lowered state governments' cost share to 10% on most projects. This likely made it easier for state politicians to use highway spending as a political vehicle and we expect state government behavior to differ before and after the 1956 act. The end date of the study period also reflects two considerations: a need to have as long a panel as possible to avoid a bias in fixed effects regressions with lagged dependent variables (see below), and making sure that we are comparing similar institutional reforms. As mentioned above, since 1996 several states have made steps to weaken their civil service protections, and including these reforms would be difficult due to the heterogeneity in their provisions.

Our main outcome of interest, per capita real highway expenditures by state governments, comes from the US Census Bureau's Census of Governments.¹⁴ We restrict attention to direct expenditures (expenditures made directly by the state government as opposed to transfers to local governments) which account for 85% of state government's highway expenditures in the average state.

Our main independent variables are the merit system indicators discussed above and indicators for the gubernatorial cycle in the state (election year / election year minus one / election year minus two, with the post-election year serving as the omitted category). The political budget cycle literature has traditionally focused on the chief executive rather than the legislature,¹⁵ and based on the institutional context this seems particularly warranted for US states' highway expenditures (see the Appendix). In our study period, two states held governor's elections every two years, while several states moved from a two to a four-year cycle. Because politicians' behavior in a two-year cycle is likely to be fundamentally different from their behavior in a four-year cycle, we restrict attention to four-year cycles. This gives us a total of 385 election cycles in 44 states.¹⁶ Of these, 11 states contribute observations both before and after civil service reform (see Table 1).

As control variables, we use characteristics common in the literature on institutions and policy outcomes (Besley and Case, 2003). In particular, we control for government resources such as the tax base, measured by state real per capita income and its squared value, as well as for demographic variables - population size and its squared value, and the fractions of state population that are school-aged (5-17) and elderly (over 65) - to capture the demand for government services. We control for the percentage of urban population, which is likely to affect highway construction and which also has been suggested as a potential correlate of

¹⁴Sources, definitions, and summary statistics of all variables appear in the Appendix.

¹⁵See Rose (2006) and Alt and Rose (2009) specifically for the context of US states.

¹⁶Excluded are New Hampshire and Vermont which have two-year cycles, and Rhode Island which switched to a four-year cycle in 1994. As is standard in the literature, we also exclude Alaska and Hawaii which are considered fiscal outliers, and Nebraska, which has a nonpartisan legislature.

civil service reform (Ruhil and Camoes, 2003). We also control for political characteristics that might be correlated with political cycles, the introduction of the merit system, and expenditures. We include a dummy for Republican control of both houses of the state legislature, a dummy for Democratic control of both houses, as well as an indicator for the governor’s party affiliation. We also include the Berry et al. (1998) measure of voter ideology, which creates an index of voter liberalism by using the ideology rating of congressional candidates and their vote shares. Finally, to separate election cycle effects from a governor’s experience in office, we include the number of years since the current administration was originally elected.

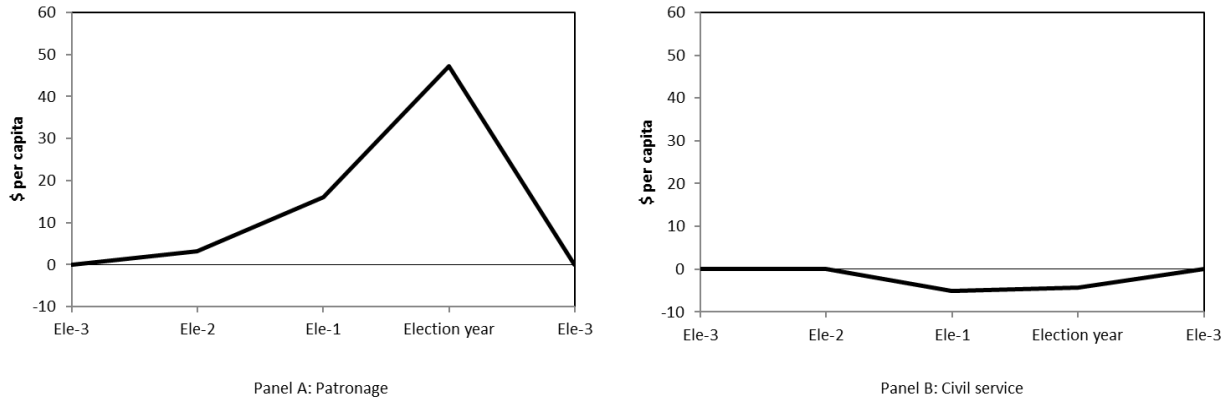
With an exercise like ours, the timing of the variables matters considerably. Governments report expenditures by fiscal year, which typically run from July 1 of the *previous* year to June 30 of the given year (e.g., fiscal year 1970 ran from July 1, 1969 to June 30, 1970). Elections are typically held in November, and we therefore match (for example) the 1970 election year with fiscal year 1970.¹⁷ Thus, we match election cycle indicators to expenditures based on the fiscal year. However, because the fiscal year 1970 budget was set in 1969 and spending from this budget began in 1969, other independent variables are lagged by 1 period. For example, a merit system adopted in calendar year 1969 is matched to expenditures in fiscal year 1970 as it is unlikely to affect expenditures made in fiscal year 1969. See the Appendix for more details.

Before presenting our econometric specification and results, we take a look at the raw data. Figure 1 plots average per capita highway spending for each year of the electoral cycle separately for periods under patronage and civil service. The post-election year is normalized to 0. The raw data clearly suggests that highway spending exhibits an electoral cycle under patronage but not under civil service.

As mentioned above, a useful feature of the data is that the institutional variation we exploit is not exclusively cross-sectional, since some of the states changed their bureaucratic institutions during the sample period. To highlight this “within” variation, Figure 2 restricts attention to the 11 states that switch from patronage to civil service. We also control for state fixed effects and some of the time variation common to all states (this is done by controlling for the presidential electoral cycle). The data again suggests an electoral cycle that is conditional on bureaucratic organization.

¹⁷This follows the standard way of matching in the literature which ensures that most spending matched to a given year occurred *before* an election was held in that year (e.g., Brender and Drazen, 2005). This seems particularly reasonable for highway projects. These take time to complete, and expenditures made a month or two before an election are unlikely to have any visible impact before the election.

Figure 1: Highway spending over the election cycle under civil service and patronage



Notes: The figure shows average highway expenditures in the raw data (in real 2009 dollars per capita) for each year of the election cycle under patronage (Panel A) and statewide civil service (Panel B). In each case the base category is the post-election year, normalized to 0. $N = 1387$.

4 Specification

Our main specification follows the standard approach to estimating (conditional) political budget cycles in the literature. To test for the possibility that the cycles differ under civil service and patronage, we use the *Merit* variable and its interaction with the electoral cycle. Specifically, we estimate

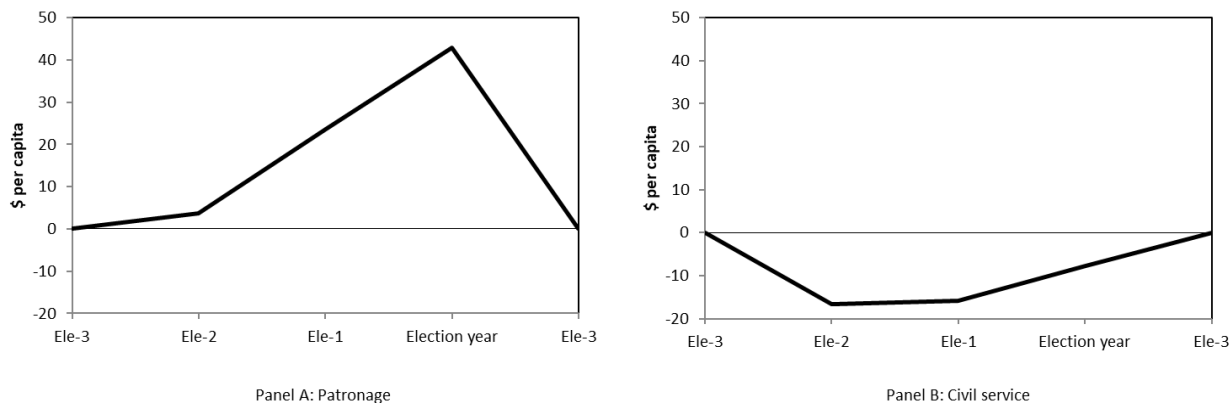
$$y_{st} = \sum_{\tau=-2}^0 (\alpha_{\tau} Ele_{st}^{\tau} + \beta_{\tau} Ele_{st}^{\tau} \times Merit_{st}) + \gamma Merit_{st} \quad (1)$$

$$+ \delta y_{s,t-1} + \mathbf{X}'_{st} \boldsymbol{\rho} + \boldsymbol{\lambda}_s + \boldsymbol{\mu}_t + \varepsilon_{st},$$

where y_{st} is per capita highway expenditures in state s in year t . The indicators Ele_{st}^{τ} capture the election cycle, with Ele_{st}^{τ} taking a value of one τ years from the next election (Ele_{st}^{-3} , or the post-election year, is the omitted category). The variable $Merit_{st}$ takes the value of one if a statewide merit system is in place in year t . Control variables include lagged highway expenditures $y_{s,t-1}$, the various time-varying state characteristics \mathbf{X}'_{st} described above, and state and year fixed effects ($\boldsymbol{\lambda}_s, \boldsymbol{\mu}_t$). Because our panel is relatively long, we estimate (1) using a standard fixed effect estimator. As discussed in Section 5.2 below, using Arellano-Bond type GMM estimators yields similar results.

The coefficients of interest in Equation (1) are the α 's and β 's. The coefficients α_{τ} capture the presence of political budget cycles under patronage ($Merit_{st} = 0$), while the coefficients β_{τ} measure the difference in this cycle under civil service. For example, $\alpha_0 > 0$

Figure 2: Highway spending over the election cycle under civil service and patronage, within variation only



Notes: The figure restricts attention to the states that switch from patronage to civil service during the sample period. It shows demeaned highway expenditures (subtracting state-specific averages and presidential election cycle specific averages) for each year of the election cycle under patronage (Panel A) and statewide civil service (Panel B). In each case the base category is the post-election year, normalized to 0. $N = 379$.

would indicate the presence of an election year budget cycle under patronage and $\beta_0 < 0$ would indicate that this is dampened by the merit system. Because highway projects take time to complete, we may expect the budget cycle to appear before the election year (with $\alpha_{-1} > 0$ and $\beta_{-1} < 0$).

While a specification like (1) is the standard way of analyzing conditional political budget cycles in the literature, these specifications do not control for differences *in the electoral cycle* (as opposed to the level of spending) across states, over time, or as a function of the time-varying characteristics \mathbf{X} . To allow for such differences, we modify (1) to include interactions of the electoral cycle with all fixed effects and time-varying controls. A difficulty arises here due to the fact that in some (odd-numbered) years, very few states hold elections in the sample. For example, in 1961, 2 states held elections, 10 were one year after an election, 3 two years after an election, and 17 three years after. In 1962, the latter 17 held elections, while 2 states were one year after an election, etc. Once we control for the interaction of the electoral cycle with state fixed effects, we find that the resulting multicollinearity makes the estimates unstable.¹⁸ To overcome this problem, in the interacted specification

¹⁸To see why years when few states hold elections create multicollinearity problems, consider an extreme case where all states other than state 1 hold elections in the same years. Letting T^* denote these election years, $\sum_{t \in T^*} \mathbf{I}(year = t)$ is then equal to Ele^0 for all states other than 1. Suppose that, e.g., $Ele_{1t}^{-2} = 1$ for all $t \in T^*$. Then Ele^0 can be expressed as $\sum_{t \in T^*} \mathbf{I}(year = t) - \mathbf{I}(state = 1) \times Ele^{-2} + \mathbf{I}(state = 1) \times Ele^0$. Thus, a specification that includes interactions between state fixed effects and the election cycle indicators,

we combine years one+two and three+four of the electoral cycle: we only use one election cycle indicator, Ele_{st} , which takes the value of 1 in the election year *and* the pre-election year, and 0 otherwise. Thus, we estimate

$$y_{st} = \bar{\alpha}Ele_{st} + \bar{\beta}Ele_{st} \times Merit_{st} + \bar{\gamma}Merit_{st} + \bar{\delta}y_{s,t-1} \quad (2)$$

$$+ \mathbf{X}'_{st}\bar{\rho} + \bar{\lambda}_s + \bar{\mu}_t + Ele_{st} \times \mathbf{X}'_{st}\tilde{\rho} + Ele_{st} \times \tilde{\lambda}_s + Ele_{st} \times \tilde{\mu}_t + \varepsilon_{st}.$$

Our empirical setting offers several advantages for identification relative to the literature. First, unlike some of the institutions studied previously, bureaucratic organization changed over time within states during our period of study. Thus, we are identifying the α and β parameters both by comparing the budget cycle across civil service and patronage states, and by comparing changes in the budget cycle when a state switches from patronage to civil service (this is what makes it possible to estimate specification (2)). Second, we avoid the serious identification concerns that arise if election dates are endogenous. Elections in our setting are always held on the same date, fixed exogenously. Third, the *Merit* variable is a measure of actual institutions, as opposed to a perception measure.

Our exercise also has a number of limitations. First, naturally, civil service reforms in US states did not arise as a result of a randomized experiment. While we can rule out a number of well-specified challenges to a causal interpretation of our results, we will not be able to *prove* that such an interpretation is valid. Second, we do not have individual data to directly test the assumption that bureaucrats behave differently under patronage and civil service. In this sense, our empirical exercise is a joint test of this underlying assumption and its implications. Third, we do not have project-level data that would allow us to study the type, location, or timing of specific projects. For example, we cannot tell if increasing highway spending in an election year caters directly to voters, or to special interests who provide campaign support. Overcoming these limitations is an important avenue for future research.

5 Results

5.1 Main result

Table 2 reports the results of estimating specifications in which the dependent variable is real per capita direct expenditures on highways (in 2009 dollars). The first column is a benchmark specification that tests for the presence of a cycle in the average state, without as well as year fixed effects, would suffer from perfect multicollinearity.

differentiating between civil service and patronage. The results do not show any evidence of a political budget cycle: highway spending in any year of the political cycle is statistically indistinguishable from the first year.

Table 2: Political cycles in highway spending and the merit system

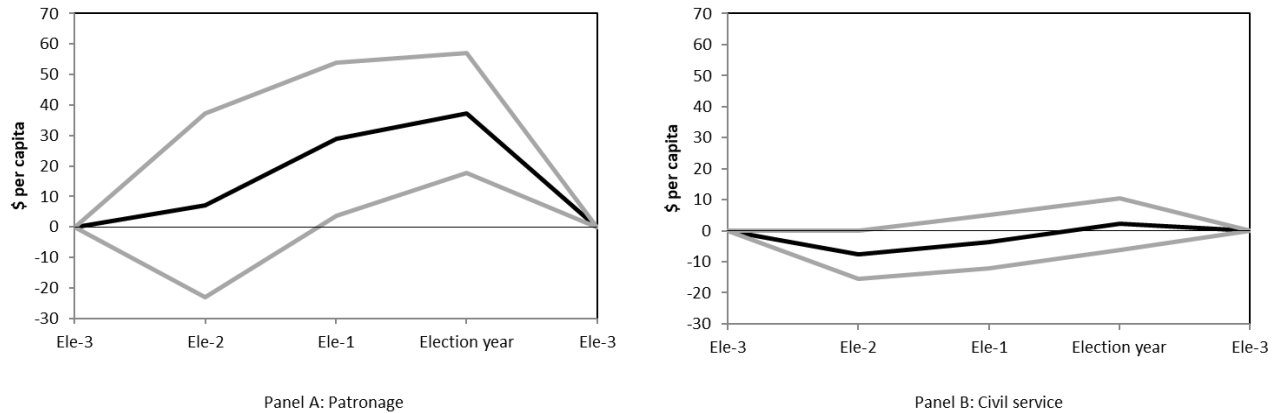
	(1)	(2)	(3)
Ele ⁰	6.17 (4.15)	37.33** (9.72)	39.15** (13.26)
Ele ⁻¹	0.50 (4.28)	28.83* (12.43)	38.39* (15.90)
Ele ⁻²	-5.89 (4.05)	7.14 (14.95)	13.27 (19.09)
Ele ⁰ × Merit		-35.10** (9.65)	-36.27* (13.60)
Ele ⁻¹ × Merit		-32.44* (12.69)	-42.11* (16.25)
Ele ⁻² × Merit		-14.84 (14.58)	-21.31 (18.78)
Merit		17.20 (8.81)	18.54 (11.16)
Merit system:		Statewide	Department-specific
R ²	0.68	0.68	0.68
N	1387	1387	1387

Notes: The dependent variable is real per capita highway expenditures. Regressions control for state and year fixed effects, lagged highway expenditures, log state population and its square, real per capita income and its square, the fraction of population aged 5-17 and the fraction aged 65 and over, urbanization, Dem. control, Rep. control, the governor's party, citizen ideology, and the governor's experience. Robust standard errors clustered by state in parentheses. ** $p < 0.01$, * $p < 0.05$.

In the second column, we interact the election cycle indicators with *Merit* to allow for the possibility that political budget cycles differ under patronage and civil service (Eqn. (1)). Once this institutional heterogeneity is accounted for, our estimate of the political budget cycle under patronage becomes large and statistically significant. This estimated political budget cycle is illustrated on panel A of Figure 3. The point estimates indicate that under patronage per capita highway spending in election years is \$37.33 higher than in the year after the election (the excluded category). In addition, highway spending in the year before an election is also larger, by \$28.83 per capita. Finding electoral effects on highway spending in the pre-election year is not surprising given that highway construction projects can take a long time to complete: having a project completed in an election year may require expenditures in the previous year. Compared to average spending in a post-

election year, these figures represent increases of 9% (pre-election year) and 12% (election year), respectively.

Figure 3: Political cycles in highway spending under civil service and patronage



Notes: The figure shows the political budget cycles in highway expenditures (in real 2009 dollars per capita) implied by the estimates in Table 2, column (2) under patronage (Panel A) and civil service (Panel B). In each case the base category is the post-election year, normalized to 0. The 95 percent confidence interval is shown by the grey lines.

Column (2) of Table 2 also shows that the estimated budget cycle is only present under patronage and disappears under civil service. For election years and pre-election years, the interactions of the political cycle indicators with *Merit* are statistically significant, and, compared to the estimates under patronage, have similar magnitudes but the opposite signs. Under civil service, election year spending is only \$2.21 higher than in the post-election year ($= 37.33 - 35.12$), and pre-election year spending is \$3.62 lower ($= 28.83 - 32.45$), neither of which is statistically different from 0 at conventional levels. This is illustrated on panel B of Figure 3. Introduction of the civil service appears to dampen the political budget cycle in highway expenditures.

In column (3), we use the highway department-specific merit system rather than the statewide merit system as our civil service indicator. As can be seen, the results are very similar, with a somewhat larger estimated cycle under patronage. Note that this is what we would expect if the Merit variable in column (3) is a less noisy indicator of the relevant bureaucratic organization for highway expenditures. We explore the implications of having *only* a department-specific merit system without statewide civil service in section 5.3 below.

Table 3 presents estimates of Eqn. (2). Because the election cycle indicator here is binary (taking a value of 1 for the pre-election year and the election year), column (1) first

shows that our findings above hold using this indicator as well.¹⁹ Column (2) adds all the interactions with fixed effects and controls. Here, the reported coefficient on the *Ele* indicator is the estimated marginal effect when the value of each interacted control and fixed effect is fixed at its sample mean. We find that the patterns from column (1) are reinforced, with the coefficients on both the election cycle dummy and its interaction with *Merit* increasing in size. Columns (3) and (4) repeat these regressions using the department-specific merit system variable and show similar findings. In all cases we find a significant political budget cycle under patronage, and this is smoothed out under civil service.

Table 3: Fully interacted specifications

	(1)	(2)	(3)	(4)
Ele	29.25** (8.18)	42.54** (12.03)	31.89** (11.25)	51.16** (13.36)
Ele × Merit	-26.22** (8.25)	-37.16* (13.92)	-28.49* (11.62)	-45.12** (14.92)
Merit	9.91 (7.94)	17.79* (6.90)	8.01 (8.28)	17.45* (7.38)
Merit system:	Statewide	Statewide	Department-specific	Department-specific
Interactions with Ele	No	Yes	No	Yes
R ²	0.68	0.93	0.68	0.93
N	1,387	1,387	1,387	1,387

Notes: The dependent variable is real per capita highway expenditures. Ele = 1 in the pre-election or the election year and 0 otherwise. Columns (2) and (4) include interactions of Ele with all control variables \mathbf{X} and state and year fixed effects. In these regressions reported coefficients on Ele are the estimated marginal effects of this indicator when the value of each interacted variable is fixed at its sample mean. All regressions control for state and year fixed effects, lagged highway expenditures, log state population and its square, real per capita income and its square, the fraction of population aged 5-17 and the fraction aged 65 and over, urbanization, Dem. control, Rep. control, the governor's party, citizen ideology, and the governor's experience. Robust standard errors clustered by state in parentheses. ** $p < 0.01$, * $p < 0.05$.

In terms of magnitude, is the political budget cycle uncovered under patronage large or small? While an extra \$30-40 per capita per year would not be much if it was purely a monetary transfer, here this represents investment in infrastructure that likely has large positive externalities over many years. Voters' valuation for such an investment is likely to be much larger than \$40. In the absence of applicable welfare measures, a more useful benchmark may be the 9-12% increase relative to the average post-election year spending. This is comparable to previous estimates in the literature: Drazen and Eslava (2010) find a 7% election year effect for urban infrastructure in Colombia, and Khemani (2004) a 9%

¹⁹Compared to the benchmark specification in Eqn. (1), this specification imposes the restrictions that $\alpha_0 = \alpha_{-1}$, $\beta_0 = \beta_{-1}$, $\alpha_{-2} = 0$, and $\beta_{-2} = \gamma$. We checked that none of these restrictions can be rejected at conventional levels of significance, although the hypothesis that all of them are jointly satisfied is rejected. We interpret this as supporting our use of the four-valued election cycle indicator in the rest of the paper, while indicating that the results from the specification in Table 3 are still informative.

effect for capital investment in India.

5.2 Robustness

In this section we present several robustness checks on our main result. We focus on the specification in equation (1) since this is the approach most commonly used in the literature. We discuss our findings in the text and provide detailed estimates in the Appendix.

5.2.1 Different samples and estimation methods

Because we restrict attention to 4-year gubernatorial terms, the set of states in the sample changes over time. In particular, some states switch from 2 to 4 year terms during our sample period, these states therefore only enter the sample in later years. Does the changing set of states affect our results? We ran regressions on a balanced panel of states, using only the 32 states that had 4-year terms throughout the sample period. Our results are very similar to those obtained earlier, indicating that the changing set of states does not affect our findings.

Our main estimates covered the period 1960-1995. There are two reasons to wonder whether the estimates are robust to considering a shorter period. First, the nature of highway spending changed over time: while the focus in the earlier period was on construction, later projects were increasingly for maintenance. Dilger (1989) suggests that 1983 was a turning point in this respect (see also Knight, 2002). Second, the approach to civil service reform changed over time. After an emphasis on merit system protections and bureaucratic independence during the first wave of civil service reforms, the second wave of reforms emphasized accountability to managers and bureaucratic responsiveness. This led to a weakening of civil service protections. While at the state level the second wave of reforms did not start until a 1996 reform in Georgia, policy changes at the federal level came earlier, with the 1978 Civil Service Reform Act. Thus, it may be that the operation of state bureaucracies in the latter part of the sample (and in particular the 1989 introduction of the merit system in West Virginia) is less comparable than in earlier years. As a robustness check, we repeated our regressions shortening the sample period by a third, to 1960-1983. The findings for this period are very similar to those obtained earlier.

As is well-known, standard fixed effects estimates of (1) give biased results when the number of periods is small due to the presence of the lagged dependent variable. Because most papers in the political budget cycle literature study short panels of 10-20 periods, they often use variants of the difference GMM estimation methods proposed by Holtz-Eakin et al. (1988) and Arellano and Bond (1991) to overcome this bias (e.g., Shi and Svensson, 2006; Drazen and Eslava, 2010). At the same time, using the Arellano-Bond type methods in long

panels is not without costs, as the large number of potential instruments creates difficulties for identification and model selection (see Roodman (2009a) for a detailed discussion). Our panel, which contains 35 years for most states, is closer to a length for which the standard fixed effects estimates is typically viewed as appropriate. For example, Judson and Owen (1999) recommend using the standard fixed effects specification for panels longer than 30 periods. In the political budget cycles literature, Persson and Tabellini (2003) and Brender and Drazen (2005) use standard fixed effects estimation in panels of length 38 and 41, respectively. To check the robustness of our findings, we performed various versions of difference GMM estimation. Our findings appear robust to the use of these different estimation methods.

We also checked the robustness of our inference from the results above using two alternative methods. First, we computed two-way clustered standard errors by state and year. In contrast to the standard errors computed above, this also allows for correlation in unobserved shocks to highway spending across states for a given year. This may be the case, for example, if a highway project initiated in one state makes highway projects more likely in neighboring states as well. We find that two-way clustering tends to decrease the standard errors of interest, and our inference above remains valid. Second, we address finite sample concerns by conducting randomization inference. To do this, we considered the “treatment” of assigning the merit system to specific states in specific years. In our sample, some states are assigned the treatment before our period of study, some during, and one state (Texas) remains untreated throughout. To conduct randomization inference, we randomly drew 500 artificial treatment assignments that retained the number of states in each of these three groups. We estimated our regression on each of these samples and collected the corresponding t-statistics for our main variables of interest, the interactions of *Merit* with the electoral cycle indicators. Treating each of these tests independently, the randomization inference p-values can be computed as the share of “artificial” t-statistics that are higher in absolute value than the “true” t-statistic obtained from the actual sample. We again find that our inference above remains valid.

5.2.2 Controlling for political strength

The political strength of the administration and the competitiveness of elections is a potential confounder that may affect both the likelihood of civil service reform and the executive’s ability and incentives for creating budget cycles. For example, if a stronger administration found it easier to create political budget cycles *and* civil service reform was more likely under a weaker administration, then the disappearance of the budget cycle observed above could be due to a decline in the political strength of the administration, rather than to civil service

reform.²⁰ While the specifications above already include several political controls, we now present five exercises to further control for this and other potential confounders.

First, we include the winning margin of the incumbent governor from the previous election as a further control for the incumbent’s political strength and the competitiveness of elections. Including this variable either on its own or interacted with the election cycle indicators does not affect our results. Second, we follow Folke et al. (2011) and drop administrations elected with relatively wide margins. The idea is that administrations elected with smaller margins may be more comparable to each other on unobservables (for example, they may face more similar competitive environments). While the estimates become imprecise as the sample gets small, restricting attention to winning margins below 20, 10 or 5 percentage points causes little change in the pattern of our results.²¹ Third, we exclude states where the same party held the governor’s seat for a long period of time around civil service reform. In these cases any changes in political strength may not be adequately captured by our control variables, we therefore checked whether excluding these states affected our results. In particular, we excluded states where civil service reform occurred during our sample period and the same party held the governor’s seat in the 5 years preceding the reform as well as the 5 years following the reform. We find that our findings remain robust. Fourth, also following Folke et al. (2011), we drop years just before or just after civil service reform. These are the periods where any confounder correlated with civil service reform may be especially relevant. Leaving out the period 2, 4, or 6 years before and after reform does not change our findings.

5.3 Heterogeneity and mechanisms

5.3.1 Department-specific vs. statewide merit system

Is the statewide or the department-specific merit system more important in attenuating the political cycle in highway spending? One view is that, because highway expenditures go through highway departments, what is relevant is whether this department is under a merit system at the time when spending occurs. The highway department’s merit system is the most likely to represent a constraint for politicians’ ability to influence where and how projects are undertaken, who is hired to work on them, etc. Another view is that a merit system specific to a particular department is qualitatively different from a statewide civil service system. For example, enforcement of the department-specific merit system may

²⁰See Folke et al. (2011) and Ting et al. (2012) for discussions of the relationship between competitiveness and civil service reform.

²¹The pre-election year coefficient drops in magnitude for margins below 10 but is large again (though imprecisely estimated) for margins below 5. The coefficients on election year and its interaction with merit remain large throughout.

not be as vigorous when state government as a whole is still under patronage (National Research Council, 1952). Or, a department-specific merit system may not create the same constraints for politicians as a civil service system covering most bureaucrats does. For example, highway construction projects can involve other departments besides highways (e.g., agriculture/forestry, health, etc.). If some of these bureaucrats enjoy civil service protections while others do not, a politician could still be able to influence, e.g., the location of the project by exerting pressure on some of the decision-makers.

We can explore this question because we have periods in our data with department-specific merit system but no statewide civil service (see Table 1). However, since we only have a limited number of these periods (38 state-year observations in four states²²), these results should be taken merely as suggestive.

Table 4 presents the results of estimating equation (1) with both the statewide and the department-specific merit variables (and their respective interactions with the election cycle). Figure 4 shows the budget cycles implied by these estimates.

In the figure, Panel A is for patronage, panel B for a department-specific merit system only, and panel C for statewide civil service. Panels A and C are similar to Figure 3 and show the dampening of the budget cycle under a statewide merit system compared to patronage. Interestingly, the pattern in Panel B is somewhere between the two, with no pre-election year increase in spending, but still a spike in spending in election years. This may suggest that a department-specific merit system without a statewide merit system dampens the political budget cycle somewhat, but still leaves opportunities for increased spending, especially closer to the election.²³ These interpretations are subject to the caveat above regarding the small number of observations in our data that are used to identify the patterns in Panel B.

5.3.2 First-term vs. re-elected governors

Governors' ability to make policy could change with experience. On the one hand, the governor's mandate may be stronger after an election than later in the term, making policy actions more likely early on. On the other hand, some policies may require time to develop and implement, making them more likely as the governor gains more experience. For exam-

²²South Carolina, Idaho, Washington, and Texas. Although a fifth state, Arizona, had a department-specific merit system before statewide civil service was introduced, its governors were serving 2-year terms and is therefore excluded from the sample for that period.

²³It is possible that politically motivated spending in the pre-election year takes a different form than election year spending: for example, the former could be weighed towards construction projects, which take time to yield results, while the latter could be more maintenance work, which can yield electoral benefits faster. Figure 4 may suggest that, on its own, a department-specific merit system may constrain the former type of spending more than the latter. Exploring this further would be an interesting topic for future research.

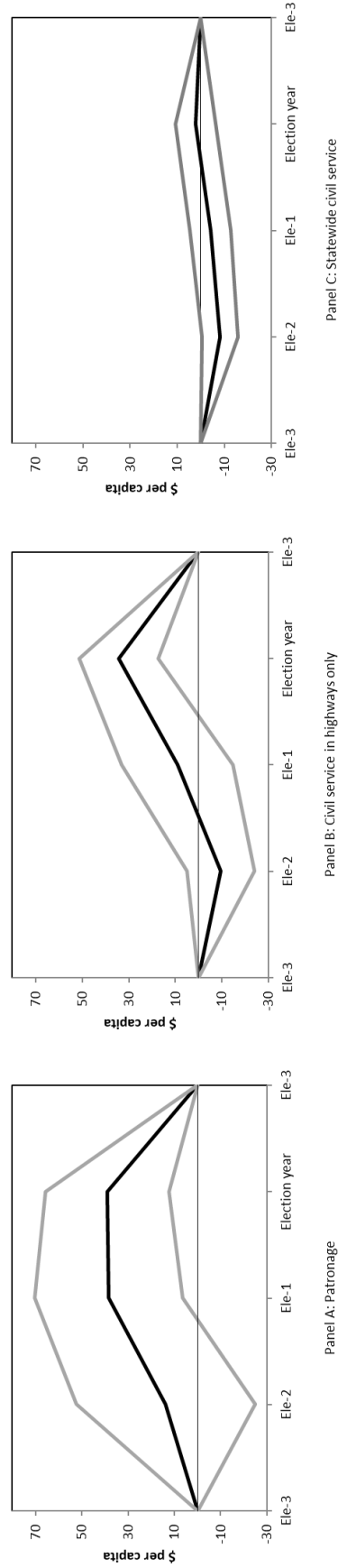
ple, because highway projects take time to develop, they may be more likely in the second half of a term, i.e., in election years and pre-election years. To interpret our results above, it is important to know whether political budget cycles are driven by politicians' incentives to engage in voter-friendly policies, or simply by changes in their experience. To address this question, in the Appendix we present results controlling for a more refined measure of governor experience (beyond the linear measure included in the above regressions). Specifically, we compare the budget cycle under patronage between first-term governors and governors who already served (either in the immediately preceding term or at some point in the past). If the budget cycle in highway spending uncovered above was driven by experience, we would expect it to be less pronounced under re-elected governors (since these governors are already experienced at the start of their current term). We obtain similar results across the two groups, suggesting that the cycle is unlikely to be due to changing governor experience and the time it takes to make policy.

Table 4: Political cycles under statewide or department-specific merit systems

Ele ⁰	39.09** (13.23)
Ele ⁻¹	38.38* (15.84)
Ele ⁻²	13.86 (19.14)
Ele ⁰ × Merit	-32.11** (7.42)
Ele ⁻¹ × Merit	-13.13 (11.37)
Ele ⁻² × Merit	1.30 (7.30)
Merit	17.40 (9.63)
Ele ⁰ × HwyMerit	-4.83 (15.49)
Ele ⁻¹ × HwyMerit	-29.32 (19.61)
Ele ⁻² × HwyMerit	-23.28 (19.93)
HwyMerit	2.00 (14.03)
R ²	0.69
N	1387

Notes: The dependent variable is real per capita highway expenditures. HwyMerit = 1 if a department-specific merit system is in place. The regression controls for state and year fixed effects, lagged highway expenditures, log state population and its square, real per capita income and its square, the fraction of population aged 5-17 and the fraction aged 65 and over, urbanization, Dem. control, Rep. control, the governor's party, citizen ideology, and the governor's experience. Robust standard errors clustered by state in parentheses. ** $p < 0.01$, * $p < 0.05$.

Figure 4: Political cycles in highway spending under statewide or highway department-specific civil service, or patronage



Notes: The figure shows the political budget cycles in highway expenditures (in real 2009 dollars per capita) implied by estimates of Equation (1) that include both the statewide and the department-specific merit variables, and their respective interactions with the electoral cycle indicators. In each panel the base category is the post-election year, normalized to 0. The 95 percent confidence interval is shown by the grey lines.

5.3.3 Term-limited governors

Most states impose limits on reelecting incumbent governors. When a sitting governor is legally prevented from running again, this may lower his incentives for election-motivated spending (Besley and Case, 1995; List and Sturm, 2006).²⁴ For our analysis, this implies that (i) any budget cycles uncovered can be expected to be weaker under term-limited governors, and (ii) the impact of civil service rules on the cycles should also be weaker under term-limited governors. In other words, if the empirical findings above indeed reflect politically motivated spending by the executive, the results should be stronger for governors who can run in the next election.

To test this, we use information on term limit regulations from Besley and Case (1995) and the updates of List and Sturm (2006). We create an indicator equal to 1 if the current governor cannot run in the next election and 0 otherwise.²⁵ In column (1) of Table 5, we drop the merit system variables but interact the election cycle indicators with the new term-limited indicator. We present coefficient estimates separately for term-limited governors and governors eligible to run again. These confirm that, without controlling for bureaucratic organization, there is no evidence for a political budget cycle in either case. In column (2), we include the merit system variables and their interactions with the electoral cycle, and we further interact all of these variables with the term-limited indicator. For governors eligible to run again, the results echo our earlier findings: we find evidence of political budget cycles under patronage, and these disappear under civil service. For governors who cannot run again, the findings are very different: if anything, spending is higher in the *post-election* year (the omitted category), and all coefficients are statistically insignificant. Column (3) confirms these patterns for the department-specific merit system variable.

These results support the interpretation of the above findings as an electorally-motivated spending cycle that is dampened by the civil service. As expected, when the electoral motivations are shut down (or at least attenuated) by term limits, the pattern disappears.

²⁴We say ‘may’ because spending decisions could still affect the electoral prospect of his party’s next candidate, or his own if running for possibly a different office in the future.

²⁵Note that this variable is different from the re-elected governor indicator introduced in Section 5.3.2 because (i) some states had a 1-term limit (so a first-term governor may be term-limited), and (ii) most states impose limits on consecutive terms only (so a re-elected governor, who served at some point in the past, may not be term limited). The Appendix shows specifications including both variables.

Table 5: The role of term limits

	(1)	(2)	(3)
Governors eligible to run again			
Ele ⁰	7.46 (6.06)	55.15** (13.75)	67.57** (17.57)
Ele ⁻¹	2.61 (5.82)	43.29* (18.41)	66.50** (18.78)
Ele ⁻²	-3.72 (5.39)	12.80 (20.03)	26.23 (24.97)
Ele ⁰ × Merit		-53.27** (13.44)	-65.13** (17.28)
Ele ⁻¹ × Merit		-46.14* (18.83)	-70.05** (18.19)
Ele ⁻² × Merit		-18.30 (19.68)	-32.27 (24.34)
Merit		21.97 (11.67)	29.45* (13.90)
Governors ineligible to run			
Ele ⁰	1.29 (5.02)	-6.15 (9.09)	-9.27 (8.97)
Ele ⁻¹	-5.83 (5.83)	-8.04 (13.43)	-15.50 (12.79)
Ele ⁻²	-10.52* (4.91)	-8.22 (10.24)	-14.91 (10.07)
Ele ⁰ × Merit		7.70 (10.97)	10.42 (11.26)
Ele ⁻¹ × Merit		1.50 (13.58)	8.61 (13.44)
Ele ⁻² × Merit		-2.72 (10.91)	4.12 (10.90)
Merit		3.06 (7.66)	-3.37 (6.70)
Merit system:		Statewide	Department-specific
R ²	0.93	0.93	0.93
N	1387	1387	1387

Notes: The dependent variable is real per capita highway expenditures. Regressions control for state and year fixed effects, lagged highway expenditures, log state population and its square, real per capita income and its square, the fraction of population aged 5-17 and the fraction aged 65 and over, urbanization, Dem. control, Rep. control, the governor's party, citizen ideology, and the governor's experience. Robust standard errors clustered by state in parentheses. ** $p < 0.01$, * $p < 0.05$.

5.3.4 The role of the legislature

In line with the political budget cycle literature, we have focused on the electoral cycle of the chief executive (the governor). As described above, in the context of US highway finance state legislatures face several constraints in affecting spending decisions. To the extent possible, we now check whether the limited role of legislatures in this context is also reflected in the data.

First, we ask whether political budget cycles may be linked to legislative elections. To do this we must deal with the difficulty posed by simultaneous elections: election years for governors are typically also election years for at least some legislators, which makes it challenging to identify the two electoral effects separately. To make some progress, we use the fact that in most cases, legislative elections happen biannually, while all governors in our sample serve 4-year terms. This means that in addition to Ele^0 , the year Ele^{-2} is typically also a legislative election year. As our results in Table 2 show, however, there is an increase in highway spending in Ele^0 , the gubernatorial election year, but not in Ele^{-2} , when only legislative elections are held. To probe this further, Table 6 column (1) excludes the 5 states where the lower house is also elected for 4-year terms (Alabama, Louisiana, Maryland, Mississippi, and North Dakota). The results confirm that highway spending follows the 4-year gubernatorial cycle, not the 2-year legislative cycle. This provides further support for our focus on the role of the executive rather than the legislature in creating spending cycles.

While the legislature may not play a direct part in highway spending cycles, it is possible that it has a mediating role by affecting governors' ability to generate cycles. For example, cycles may be less likely under divided government when one party controls the legislature while the other holds the governor's seat, or under a more professional legislature. We test for the former possibility in column (2) of Table 6 by including interactions between the election cycle variables and an indicator for divided government. The new coefficients are small and not statistically significant, while our earlier findings on civil services vs patronage remain robust in this specification. To study legislature professionalism, column (3) includes an index created by King (2000) based on Squire (1992). This captures the degree of professionalism based on legislators' compensation, the legislature's days in session, and its expenditures for services and operations per legislator.²⁶ As above, the new coefficients are not significant while our earlier findings remain robust. Overall, a civil service system appears to have a bigger role than divided government or professional legislatures in dampening the political cycle in highway spending.

²⁶In our sample, the range of the index is [0.048, 0.9], with a mean of 0.241 and a std. deviation of 0.138.

Table 6: The role of state legislatures

	2-year house elections only (1)	Divided government (2)	Legislature professionalism (3)
Ele ⁰	43.35** (10.23)	37.63** (9.54)	34.47** (9.89)
Ele ⁻¹	25.94* (11.30)	28.08* (12.61)	24.78 (13.18)
Ele ⁻²	15.45 (16.89)	5.67 (14.69)	7.24 (15.22)
Ele ⁰ × Merit	-43.73** (9.72)	-35.10** (9.63)	-36.25** (10.08)
Ele ⁻¹ × Merit	-30.46* (11.38)	-32.51* (12.62)	-34.23* (13.20)
Ele ⁻² × Merit	-22.84 (16.13)	-14.92 (14.46)	-14.97 (14.82)
Merit	18.75 (10.15)	17.34 (8.79)	18.03 (8.98)
Ele ⁰ × Divided		-0.89 (7.51)	
Ele ⁻¹ × Divided		2.95 (6.78)	
Ele ⁻² × Divided		5.50 (7.60)	
Divided		0.57 (5.43)	
Ele ⁰ × Prof			17.44 (17.68)
Ele ⁻¹ × Prof			25.69 (25.11)
Ele ⁻² × Prof			0.08 (22.47)
Prof			-11.27 (66.04)
R ²	0.70	0.69	0.69
N	1,216	1,387	1,387

Notes: The dependent variable is real per capita highway expenditures. Column (1) restricts attention to states with 2-year terms for house representatives. Divided is 1 if the legislature and the governor's seat are controlled by different parties. Prof is an index of the legislature's professionalism. All regressions control for state and year fixed effects, lagged highway expenditures, log state population and its square, real per capita income and its square, the fraction of population aged 5-17 and the fraction aged 65 and over, urbanization, Dem. control, Rep. control, the governor's party, citizen ideology, and the governor's experience. Robust standard errors clustered by state in parentheses. ** $p < 0.01$, * $p < 0.05$.

5.3.5 Toll vs. regular highways

At the most general level, political budget cycles refer to any changes in fiscal categories correlated with the electoral cycle. Budget cycles can arise from politicians' and voters' focus on expenditures or revenues (or both). For example, if cycles reflect politicians' desire to please voters and voters are "fiscal conservatives" (Peltzman, 1992), politicians' incentive may be to increase revenues and lower deficits before elections.

Our results above provide evidence of a focus on a particular type of expenditure in the context of US state politics. However, this interpretation may need to be qualified due to the presence of toll highways. While toll and non-toll highways are similar in many respects (including their potential to serve as a vehicle for patronage), there is one crucial difference: toll highways create revenue for state governments. If the political budget cycle uncovered above was driven by spending on toll highways, this could indicate that incumbent politicians are in fact motivated by revenues rather than expenditures.

Because the Census of Governments reports spending on toll and non-toll highways separately, we can check for this by estimating separate regressions for the two categories. The results, reported in Table 7, clearly indicate that the budget cycles under patronage arise in non-toll highway expenditures. The evidence regarding toll highways is at best inconclusive: there are no statistically significant cycles under either patronage or civil service, but the standard errors are large. Toll highways and an associated desire to increase revenues does not drive our results above.

6 Conclusion

Bureaucratic institutions matter for policy and the behavior of politicians. In this paper we found that civil service protections can stabilize government activity over time by dampening the political budget cycle. In particular, we found significant budget cycles in the highway expenditures of US state governments under patronage but no cycles under civil service.

These findings may suggest a possible explanation for some of the cross-country differences observed in previous studies: political budget cycles may be more prevalent in political systems characterized by patronage but less likely to occur under civil service. While the potential of civil service to stabilize the bureaucracy has long been recognized, our results suggest that this institution may also have a "multiplier" effect by stabilizing the policies chosen by election-minded politicians.

Table 7: Political cycles in toll vs. non-toll highway spending and the merit system

Dep. Var.:	Toll highways (1)	Non-toll highways (2)
Ele ⁰	6.60 (7.60)	30.56** (7.76)
Ele ⁻¹	6.14 (5.29)	22.19 (11.23)
Ele ⁻²	2.52 (2.22)	4.09 (14.20)
Ele ⁰ × Merit	-4.70 (7.69)	-30.33** (8.09)
Ele ⁻¹ × Merit	-5.64 (5.16)	-26.38* (12.03)
Ele ⁻² × Merit	-2.08 (2.13)	-12.34 (13.97)
Merit	3.85 (2.69)	12.99 (7.45)
R ²	0.39	0.68
N	1387	1387

Notes: The dependent variable is real per capita highway expenditures on toll and non-toll highways, respectively. Regressions control for state and year fixed effects, the lagged value of the dependent variable, log state population and its square, real per capita income and its square, the fraction of population aged 5-17 and the fraction aged 65 and over, urbanization, Dem. control, Rep. control, the governor's party, citizen ideology, and the governor's experience. Robust standard errors clustered by state in parentheses. ** $p < 0.01$, * $p < 0.05$.

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