Fiscal policy and politicians' term length[*]

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Abstract

This paper investigates the causal effect of the term length of political executives on economic policy outcomes. To establish causality, we exploit the staggered adoption of four-year terms for governors across US states, using data for the period 1937–2008. We find that increasing governors' tenure in office from two years to four years reduced state expenditures and revenues by approximately 0.3–0.5 percentage points of GDP. The effect on state finances is primarily driven by a reduction of current spending and grants from the federal government, and it is concentrated in states where the incumbent governor expects fierce competition in the next election. Lastly, we discuss the implications of longer terms for macroeconomic stabilization, political budget cycles, and intergovernmental resource allocation.

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1 Introduction

Political term lengths vary significantly within and across countries. For example, the parliament of Australia remains in office for three years while in France terms usually last five years. Similarly, the President of Brazil is elected for a four-year term, while the President of Mexico serves for six years before a new election occurs. In the United States, the President is elected for a four-year term, while members of the House of Representatives and of the Senate are elected for a two-year and a six-year term, respectively. US state governors remain in power for four years in most states, while a two-year term applies in New Hampshire and Vermont. As documented in panel (a) of Figure 1, the length of the Head of Government terms globally varies between 3 and 7 years, although the majority of countries have chosen a term length of four or five years.

The choice of the length of term – or, in other words, of the statutory distance between two consecutive elections – affects both *who* is attracted to the political office and under *which incentives* the incumbent politician decides policy to secure reelection. However, the literature on the economic effects of politicians' term length is rather scarce. Empirical studies (Dal Bó and Rossi, 2011; Titiunik, 2016) focused their attention on how the term length of members of representative assemblies affect legislators' behavior.[1] The results document that legislators who are granted office for a longer term perform better across a variety of measures. Research on term lengths for political leaders (i.e., Head of Government) is limited to theoretical models. Schultz (2008) analyze a model in which shorter terms improve the incumbent politician's accountability but increase the incentive to distort policy to seek re-election.[2] Gersbach et al. (2020) study the term length that minimizes dynamic inefficiencies if the median voter shifts over time and policy changes are costly for citizens and politicians.[3]

We empirically study the effect of extending political executives' term length on the fiscal policy implemented by their governments. To start, we report in Panel (b) of Figure 1 the results from estimating a cross-country regression to correlate the statutory length of Head of Government terms and government spending (as a fraction of the country's GDP).[4] We disclose a strong and negative correlation. Countries in which the

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[1] Conconi et al. (2014) show that US senators elected for a six-year term are more likely to support trade liberalization policies than representatives elected for a two-year term. The earlier generation of empirical studies (Amacher and Boyes, 1978; Bernhard and Sala, 2006; Bernstein, 1991; Elling, 1982; Levitt, 1996; Thomas, 1985; Wood and Andersson, 1998; Wright and Berkman, 1986) focused on the effects of election proximity by utilizing that a third of the seats in the US Senate is up for election every two years.

[2] According to Schultz (2008), swing voters prefer shorter terms if uncertainty is high, parties are not very polarized, and politicians do not learn while in office. On the contrary, partisan voters always prefer longer terms.

[3] On the one hand, longer terms reduce the costs associated with policy changes; on the other hand, longer terms implies that policy is less aligned with the preferences of the median voter.

[4] Data on government spending refer to the last available data according to either the World Bank or the International Monetary Fund.
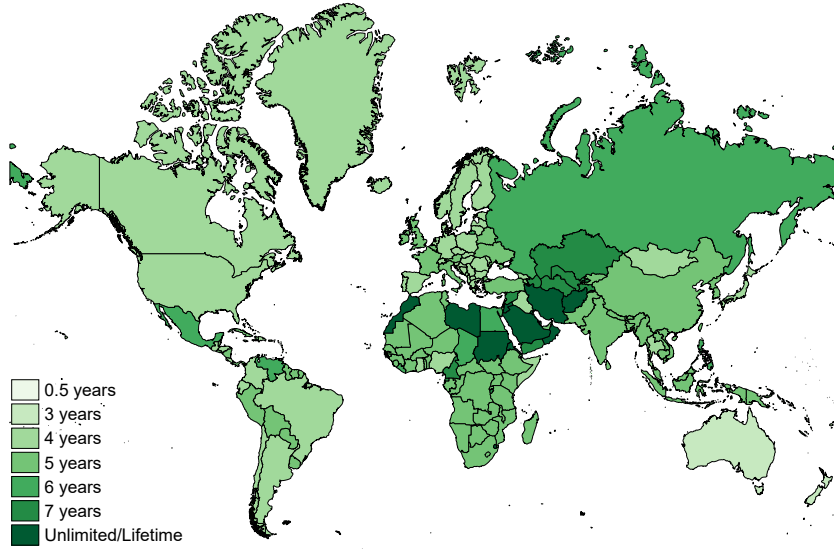
(a) Distribution of Head of Government term lengths
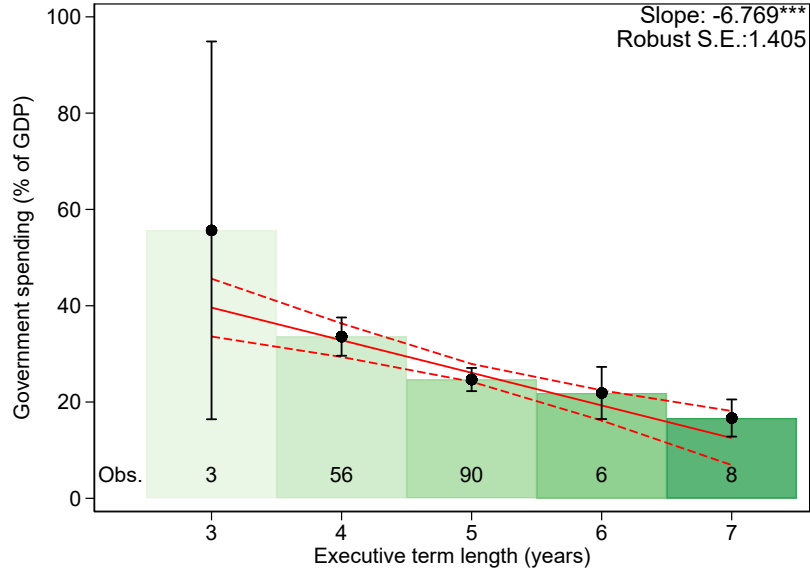
(b) Cross-country correlation with government spending

Figure 1: Head of Government term length across the globe

Notes: The unit of observation is a country. Head of Government term length is calculated based on presidential terms (presidential countries) or on lower house terms (parliamentary countries). See Appendix B for further information on the dataset utilized for this analysis. Panel (a) shows the global distribution of executive term lengths. Panel (b) reports the average government spendin f GDP for each group of countries and the binary correlation between government spending as a percentage of GDP and executive term length. San Marino (which is the only country in which the term for the two executive officers lasts 6 months) and countries in which executive term lengths are unlimited are excluded from panel (b). In panel (b), 95% confidence intervals are based on standard errors robust to heteroskedasticity.

head of Government is secured in power for more years tend to have smaller budgets than countries that hold general elections more frequently. The size of the correlation is

sizable, as a one-year increase in the term length is associated with a reduction of the spending-to-GDP ratio equal to 6.7 percentage points. Although the correlation survives controlling for a number of observable characteristics, cross-country analysis is not well-endowed to draw credible causal claims.[5]

To make such claims, we study the reforms that in the XX century extended the term length of US state governors from two to four years. We construct a panel of state expenditures and revenues that spans several decades (1937–2008) and estimate a staggered Difference-in-Differences model.[6] Twenty-one states extended their term length from two to four years during our observation window. Our main results establish that extending the gubernatorial term length reduced the size of the state total expenditures and revenues by 0.3–0.5 percentage points of GDP.[7] The estimated negative effects on expenditures and revenues are similar to each other, suggesting that the institutional reform did not significantly affect the propensity to run a deficit. We also find that the negative effect on spending is driven by a reduction in current spending. This latter result is consistent with the observation that incumbent governors subject to a four-year term face stronger incentives to prioritize far-sighted investments over short-lived policies.

Why do longer term lengths affect the fiscal policy implemented by US governors? We test for a number of potential mechanisms. An hypothesis which is consistent with the earlier literature focused on members of legislative assemblies (e.g., Dal Bó and Rossi, 2011; Titiunik, 2016) is that longer terms increase the monetary value of holding the gubernatorial office and, for this reason, the effort exerted by incumbent governors to secure re-election. We find several pieces of empirical evidence in support of this hypothesis.

First, we show that our results are driven by governors that will be allowed to re-contest at the next election – either because term limit laws do not apply in their state or because the governor is not serving for their final term yet.[8]

Second, we document that our main results are significantly stronger in states whose voters were historically better informed about the performance of the incumbent government. More specifically, we divide the sample based on whether the proportion of

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[5]The panel (a) of Table A.1 in the Appendix reports the correlation coefficients obtained controlling for population, longitude, latitude, altitude, world region fixed effects, democratic regime dummy, parliamentary form of government dummy, and World Bank income class. Figure A.1 and the panel (b) of Table A.1 in the Appendix replicate the results presented in Figure 1 and in panel (a) of Table A.1 using government revenues as a percentage of GDP as the dependent variable. The conclusions are unaltered.

[6]To account for the potential bias of Two-Way Fixed Effects models in the context of a staggered treatment, we adopt the stacked-by-event approach (e.g., Cengiz et al., 2019).

[7]Due to data availability, we use the state personal income as a proxy for the state GDP.

[8]Lame-duck incumbents (i.e., incumbents in their final term) are expected not to respond to electoral incentives (e.g., Besley and Case, 1995; Sieg and Yoon, 2022) – or to respond to electoral incentives to a lesser extent (Harrington, 1992) – as they know they cannot be re-elected. In turn, (lack of) incentives induced by term limits affect fiscal policy (Alt et al., 2011; Besley and Case, 1995) and secondary policies (List and Sturm, 2006), increases corruption (Ferraz and Finan, 2011), and reduce the overall productivity of legislators (Fouirnaies and Hall, 2022). Sieg and Yoon (2022) show that the effect of term limits on the incumbent's performance is significantly more driven by its effect on incentives than by its effect on political selection.

households holding a radio in 1930 (i.e., just before the beginning of our data coverage) was above or below that of the median state. We find that the effect of extending gubernatorial term length from two to four years is stronger in states in which the radio was relatively more widespread in 1930. These results are consistent with the extensive literature on the interplay between political accountability, electoral incentives, and incumbent's performance (e.g., Snyder and Strömberg, 2010; Strömberg, 2004) according to which media exposure allows voters to hold incumbent politicians more accountable and, in turn, induces politicians to perform better.[9]

Third, we estimate that our main results are stronger in states that were experiencing tightly contested gubernatorial elections just before the reform than in states where governors were traditionally supported by a landslide majority of voters. Indeed, incumbents elected by a narrow margin of votes expect a fierce competition in the next election than incumbents that easily won their seat (Curto-Grau et al., 2018). Taken together, this set of results suggests that the length of political executives matters for fiscal policy outcomes especially if i) incumbent governors are at risk of losing their seat at the next election and ii) voters are sufficiently informed about their performance and can hold them accountable for the actions taken while in office.

Another potential channel connected to the increase in the value of holding office relates to the possibility that granting longer terms to governors increases the quality of individuals that compete for the position of governor. To our knowledge, this channel has been overseen by the existing theoretical and empirical literature on the term length. We manually collect detailed information on education and previous political experience of a large sample of gubernatorial candidates (approximately 85 percent of the universe of candidates running in gubernatorial elections during our period of observation).[10] Our empirical results disclose that four-year terms attract individuals with more promising background characteristics into gubernatorial races. More specifically, we find that candidates running after the reform are significantly more likely to hold a college degree than candidates running for a two-year term. We also find that the extension of term-length

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[9]Snyder and Strömberg (2010) estimate that voters living in areas of a US congressional district in which the press covers more the local representative's activity are more likely to recall and being able to rate the representative. In turn, representatives often covered by the local press perform better than representatives that are less covered. More specifically, about the diffusion of radio during the twenties of the XIX century, Strömberg (2004) finds that the diffusion of radio affected New Deal's spending in the United States while Adena et al. (2015) find that the diffusion of radio in Germany's Weimar Republic increased the support for the incumbent government and reduced the early support for the Nazi party.

[10]Education is commonly used in the literature as a proxy for the competence of political leaders (e.g., Besley and Reynal-Querol, 2011; Gagliarducci and Nannicini, 2013; Gamalerio and Trombetta, 2025) since highly educated political leaders can foster economic growth (Besley et al., 2011), ease the production of public goods (Martinez-Bravo, 2017), and improve the chances of fiscal stability (Meriläinen, 2022), although the existing empirical evidence is mixed (Carnes and Lupu, 2016 find that college-educated politicians do not perform better than leaders without college degree in Brazil or in the US Congress. It is also important to mention that education may reflect other aspects rather than competence such as, for example, the socio-economic background (Black et al., 2005).

increased the initial level of political experience of gubernatorial candidates.

In the second part of the paper, we discuss the implications of longer executive terms on the macroeconomic stabilization function of governments, electoral budget cycles, and the inter-governmental allocation of resources.

First, we investigate whether state governments are more likely to pursue a counter-cyclical fiscal policy under a two-year or a four-year term.[11] We find that states subject to a four-year gubernatorial term length tend to decrease the size of their budget when the economy is booming – i.e., when reducing the state budget is the right action to take if the state aims to pursue a counter-cyclical fiscal policy – more often than states exposed to a two-year term.

Second, we explore how political budget cycles are affected by the extension of the executive term length.[12] We document that states subject to a two-year term and states exposed to a four-year term implement analogous fiscal policy in the election year, while the estimated average difference between the two institutional regimes is concentrated in off-election years.[13]

Third, we evaluate the implications on the political distortions in the allocation of federal grants to state governments. We provide evidence that the reduction in the size of the state budget is concentrated in states where the incumbent US President either won or lost the last presidential election by a large margin of the votes (i.e., the *core support* states). In opposite, we do not find any evidence of a reduction of the state budget in states that the US president either won or lost by a small margin of votes (i.e., the swing states).[14] Taken together, our results suggest that longer term lengths cause an overall improvement of the state finances but may come with unintended consequences that should be taken into consideration: higher economic uncertainty due to larger difference between fiscal policy conducted during election years and off-election years, and political distortions in the inter-governmental allocation of resources.

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[11] Persson and Tabellini (2003) document that presidential democracies are associated to a more responsible fiscal policy than parliamentary democracies because governments in the two regimes are equally likely to increase government spending during recessions, but presidential democracies are more likely to reduce spending during economic booms.

[12] A large literature originated from Rogoff (1990); Rogoff and Sibert (1988) has documented that governments tend to increase government spending and reduce taxes just before the election and, conversely, to reduce spending and increase taxes just after the election.

[13] When comparing the first year of a two-year term with the first year (or the first and the second year) of a four-year term, we estimate a negative effect on spending equal to 0.6 percentage points of GDP. Similar results are obtained when comparing the first year of two-year term with the first, the second, and the third year of a four-year term (negative effect on spending equal to 0.5 percentage points of GDP).

[14] Standard political economy models such as Dixit and Londregan (1996) and Lindbeck and Weibull (1987) predict that politicians competing in a nation-wide election will find it optimal to allocate extra resources to *swing voters* or, similarly, to target swing districts. See, e.g., Dahlberg and Johansson (2002) for empirical evidence in support of this hypothesis. Strömberg (2008) extends the theory to the US President's electoral college and finds empirical support for the hypothesis that presidential candidates target states in which the local presidential race is expected to be tight and ii) are likely to be decisive in order to assign the presidency.

Our study contributes to the literature on the economic effects of political institutions. A large strand of literature documented effects of the form of government (Persson and Tabellini, 2003), voting systems (Bordignon et al., 2016; Gagliarducci et al., 2011; Persson et al., 2007; Persson and Tabellini, 2003), and the presence of term-limits for officers (Besley and Case, 1995) on economic policy choices. Our work documents the causal effect of securing longer terms to executive officers on the size of the public sector, its macroeconomic stabilization function, and inter-governmental relations, all in a unified setting which allows us to exploit quasi-experimental variation. Compared to earlier empirical studies on politicians' term length (Dal Bó and Rossi, 2011; Titiunik, 2016), we make three main contributions. First, we focus on the head of the executive power rather than on members of a legislative assembly. Second, we provide evidence of both an effect on the performance of policymakers and of an effect on the self-selection of individuals into the gubernatorial election. Third, we analyze the implications of the institutional reforms for the government to conduct macroeconomic stabilization policy, for political budget cycles in the states' public finances, and on intergovernmental relations between the states and the federal government.

More broadly, our paper also relates with the existing literature on the mandate of firm directors' (e.g., Fahlenbrach et al., 2017; Nguyen and Nielsen, 2010) and corporate boards (e.g., Boone et al., 2007) as well as on the literature on the effects of job insecurity (see, e.g., Bjuggren, 2018, for a recent survey of the literature). In this regard, our empirical results suggest that increasing job security or directors' mandate may affect firm outcomes via both an improvement in the pool of individuals who are interested in the position and a better performance of current workers or managers.

2 Institutional Background

2.1 US State Governors

The governor is the head of the executive power in US states. Governors are directly elected by the general population for a term in office whose length is determined by each state Constitution independently. State Constitutions also rule the maximum number of terms that a governor can remain in power before being prevented from running again for the same position.

US governors have a direct impact on the state finances as they are responsible for drafting the state budget that the state Congress will eventually discuss and approve. Also, the governor must sign the budget approved by the state Congress to ensure it enters into force. In some states, the governors' budgetary powers are amplified by the line-item veto, which gives them the power to reject specific items in the budget.[15] The

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[15]According to the National Conference of State Legislatures, line-item veto power is permitted in 44

state budget mainly includes expenditures on public goods such as public health care, public education, social programs, transportation and local infrastructure. Spending is financed through taxation, user charges, and revenues from state-owned-enterprises, as well as intergovernmental grants from the federal government.[16]

2.2   The Extension of Gubernatorial Term Lengths

Proponents of US state constitutions argued that short gubernatorial terms were more democratic, as they prevented incumbents from entrenching themselves in office and constructing political machines to perpetuate their power (Sabato, 1983). As documented in Figure A.2 in the Appendix – the figure offers a graphical overview of the different term lengths for US governors from 1778 to 2008 – 10 of the original 13 state constitutions prescribed annual gubernatorial elections. In the 19th century, state governments became increasingly active, investing heavily in banks, canals, and other infrastructure projects (Wallis, 2000). Administrating the states' new commitments became increasingly burdensome, and calls were made to expand the gubernatorial terms in order to improve the governors' capacities to deal with more complex administrative tasks. As a result, the pre-existing states gradually extended the term length from one to two years, while the new states that joined the union generally specified four-year terms (Sabato, 1983).

State governments continued to expand in the first half of the 20th century, especially after the enactment of the Social Security Act in 1935. The still frequent use of two-year terms came under scrutiny of commissions aiming to improve the state governments' cost-efficiency. Extending the term length to four years was one of the "strikingly similar" recommendations that came out of the so-called "Little Hoover Commissions" (The Council of State Governments, 1952).

An important reason for why half of the states still prescribed two-year terms less than a century ago is that extending the term length for the governor requires amending the state constitution. Indeed, neither the governor nor the state congress' majority could make such decision readily. Each state has its own rules for exactly how this procedure is done, but common for all states is that a constitutional amendment is proposed by the legislature, by a state constitutional convention or a ballot initiative, and must be approved by voters in a state-wide election.[17] Inspecting the universe of popular votes about constitutional amendments, we disclose that 50 percent of the successful

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of the 50 states.

[16]All states with the except of Vermont are subject to either a Constitutional or a statutory balanced-budget requirements of variable severity. In some States, the drafted and/or the approved budget shall be balanced while other states also require that deficits cannot carry over into the next year. Nebraska requires that a two-year budget is balanced, while surpluses or deficits are admitted in the first year.

[17]A ballot initiative is a petition signed by a minimum number of voters. A state constitutional convention is a committee whose members are constitutional experts such as legal scholars, former judges and politicians. Some states allow a simple majority vote to ratify a proposed amendment, while other states require a qualified two-thirds majority.

attempts to extend the term length from two to four year were preceded – oftentimes by several years – by at least one attempt that failed because voters did not approve the proposed constitutional amendment. Today, all 50 states except two (New Hampshire and Vermont) have adopted four-year terms.[18] Table A.2 in the Appendix reports, for each state, the date of the extension of gubernatorial term lengths as well as all failed attempts.

The general procedure for extending the term length is the following. First, amendments to the Constitution are voted in concurrence with general elections, which are usually held in November. If the amendment is approved, the governor that is elected on the same date enters office in January of the following year and serves for a last two-year term. Four-year terms become effective at the end of that governor's term (i.e., the first governor that remains in power for four years is usually elected two years after the approval of the constitutional amendment and enters office in January of the subsequent year).

3   Data and Descriptive Statistics

We construct a novel panel of US states that spans the years 1937–2008 and combines information on state public finances, individual characteristics of incumbent governors and defeated gubernatorial candidates, successful and failed attempts to amend the term length for governors as well as economic, political, and institutional background characteristics of each state. Our main sample includes 23 states that used to rely on a two-year term length in 1937, of which 21 later adopted four-year terms.[19]

Data on state government finances are collected from different sources all published by the Bureau of the Census of the US Department of Commerce. Although some states approve yearly budgets while others approve (or used to approve) multi-year budgets, US Census Bureau data are useful as they aggregate ex-post expenditures and revenues by fiscal year for each US state. The definition of fiscal year varies by state: in most states fiscal years end on June 30th, although other dates such as March 31st or October 31st are used by some. We harmonize the balance sheet data so that in our panel, year t corresponds to the fiscal year ending in t. To ensure that the outcome variables utilized in the analysis are consistently measured over several decades, we limit our attention to total spending and revenues of the state as well as macro-aggregations such as the current

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[18]Constitutions are amended relatively frequently, and voters are usually asked to decide on several constitutional amendments on the same election day. Table C.1 in the Appendix reports all the other amendments that passed in the same year as the term length was extended. We cannot identify any pattern suggesting that term length amendments were consistently bundled with other specific constitutional reforms.

[19]Our main sample excludes all states that adopted four-year terms before 1937 (i.e., "always treated" states) plus New Jersey, that extended governors' term length from 3 to 4 years in 1947. As discussed in Section 5, including New Jersey does not affect our results.

spending (net of interest payment), the capital spending, the state tax revenues, and the amount of grants received from the federal government.[20]

We complement state finances with other data sources. First, we construct a manually collected, novel database covering information on the educational attainments and previous political experience of gubernatorial candidates running for office between 1938 and 2008 for a total of 595 elections and 1059 gubernatorial candidates. This data covers 85 percent of all gubernatorial candidates and the universe of elected governors. Second, we use data on approved and failed constitutional amendments to extend the term length from Ballotpedia. Third, we collect ancillary data on economic, demographic, political and institutional characteristics from various sources (see Appendix B for information about all the data sources).

It is customary to rescale government spending and revenues by the GDP of the economy. However, state-level GDP data are only available since 1997. We proxy for the nominal GDP using data from the Bureau of Economic Analysis on state personal income (state personal income is used a proxy for GDP also by, e.g., Barro and Sala-i Martin, 1992).[21] In Table A.3 in the Appendix, we present basic summary statistics for the main variables used in the empirical analysis. In our sample, current expenditures are predominant in the state budget, as they account for approximately 85% of total spending, whereas capital expenditures contribute to circa 15%. State total spending and total revenues amount on average to 9.4% of GDP, with state tax revenues comprising 5.4% and state grants revenues 2.5%. The remaining amount comprises other charges and miscellaneous revenues. The relative magnitude of state tax revenues compared to state grants revenues suggests that states primarily rely on their own taxation capacity rather than on federal transfers, although grants account for approximately 25 percent of the budget.

4   Empirical Strategy

Intuition and general setting. The main objective of this paper is to test empirically whether and how longer gubernatorial term lengths affect public finances. We compare the evolution of public finances over time among states that extended the term length in earlier years (the *early treated* states) versus states that extended the term length in later years (the *not-yet treated* states) and with those that still are subject, to date, to two-year terms (the *never treated* states). The key identifying assumption of our staggered Difference-in-Differences approach is the standard parallel-trends assumption:

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[20]In our analysis, total spending (resp., revenue) refers to the total spending (resp., revenue) of the general state government – i.e., the total amounts net of the balance of the insurance trust and of public state enterprises.

[21]Throughout the paper, we refer to state finance variables as a share of GDP. It is important to notice that, formally, the denominator is the state personal income.
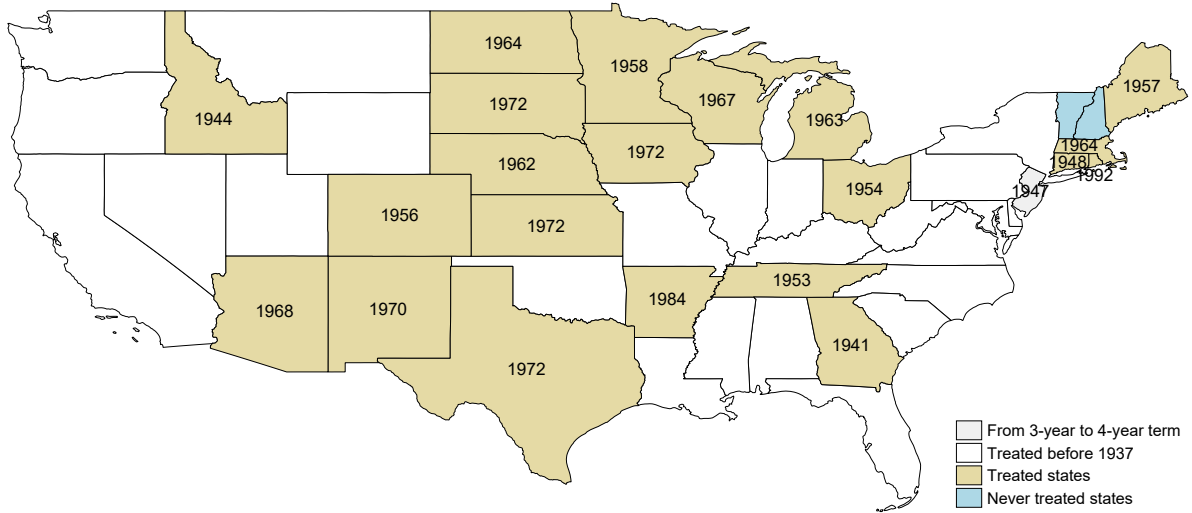
Figure 2: Spatial distribution of the staggered adoption of four-year term for US governors across states

Notes: The figure shows the spatial distribution of the approval of constitutional amendments that extended the gubernatorial term length from two to four years during the period 1937–2008. States that adopted a four-year term during the period are reported in light brown. For those states, the year reported in the map is the year of the constitutional amendment's approval (i.e., year $s - 1$). States that, to date, have not adopted yet a four-year term are reported in light blue. States that adopted a four-year gubernatorial term length before 1937 are reported in white. New Jersey, that extended the gubernatorial term length from three years to four years in 1947 is reported in gray.

fiscal policy in states that adopt four-year terms on different years would have followed parallel trends had the reform not occurred. Although we will provide formal evidence about the validity of the assumption, it is worth mentioning that the procedure to amend the state constitution reassures us about its soundness. Moreover, we will formally exploit the failed attempts to extend the term length from two to four years as an ideal *placebo* analysis.

Visualization of the cross-sectional and the temporal variation. Figures 2 and 3 display the spatial and the temporal variation that we exploit. In Figure 2, we present a map of the mainland US that identifies the states included in our sample and the year in which each state adopted a four-year term. The figure reports in light brown the states that extended the term length during our data coverage. For each state, we specify the year in which voters approved the constitutional amendment. Also, we report in light blue the two states that never decided to extend the governors' term from two to four years. All states that adopted a four-year term upon being granted statehood or before 1937 are reported in white.[22] These are the *always treated* states, which are excluded from our sample. In Figure 3, we plot the cumulative number of contiguous US states that were subject to either a two-year or a four-year term in every year of our observation

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[22]Alaska and Hawaii, not reported on the map, were granted statehood after the beginning of our observation time window. Both states adopted a four-year term length continuously since.
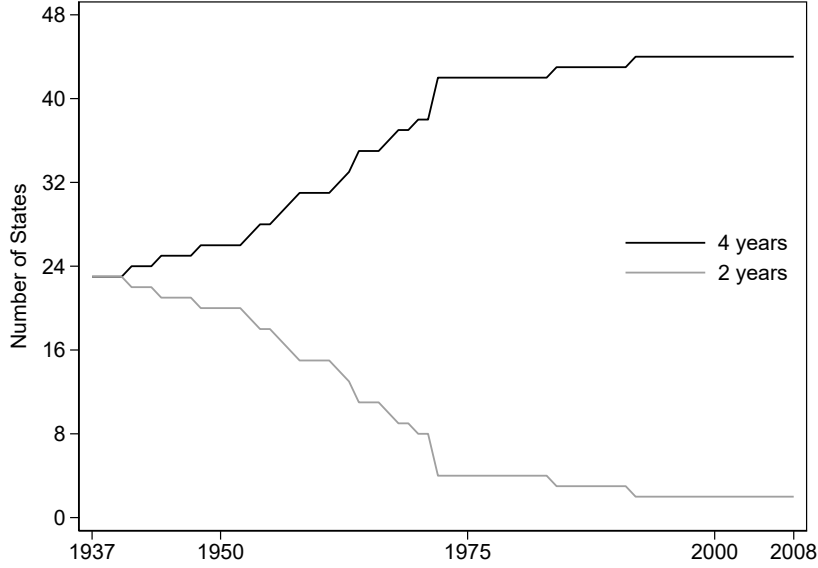
Figure 3: Evolution of two-year and four-year term for US governors across states

Notes: The figure shows the evolution of the total number of states subject to either a four-year gubernatorial term length (in black) or a two-year gubernatorial term length (in gray).

window. Figure 3 documents that the term length extensions that we exploit occurred between 1941 (Georgia) and 1992 (Rhode Island).

Timing of the treatment.   As described in Section 2, states usually vote on constitutional ballots on the same day as the gubernatorial election, which implies that the first term after the ballot lasts two years independently from the result. For this reason, one may suspect that the *post treatment* period should start when the first four-year term begins (i.e., January of the third year after the approval of the amendment). We argue, instead, that also the last two-year term is a part of the *post treatment* period. The reason is that the incumbent governor, although serving for a two-year term, may realize policy with the objective of securing re-election for a four-year term.[23] The other mechanisms triggered by the reform will enter into force upon the beginning of the first four-year term. Figure 4 summarizes our logic. In the figure, we denote the year of the constitutional amendment as $s - 1$ (i.e., the final year before the treatment). In turn, s is the first post-treatment year while the first four-year term begins at time $s + 2$.

Stacked-by-event specification.   Recent research has formalized the limitations of estimating staggered Difference-in-Differences models using a two-way fixed effects (TWFE) specification (e.g., Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille,

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[23]This argument sounds natural when thinking about theoretical models of political accountability, in which the incumbent policymaker realizes policy with the objective to secure re-election (hence, some ego-rents from holding in office). The ego-rent that enters the incumbent's objective function is determined by the next term's length.

Figure 4: Timeline of term length extensions
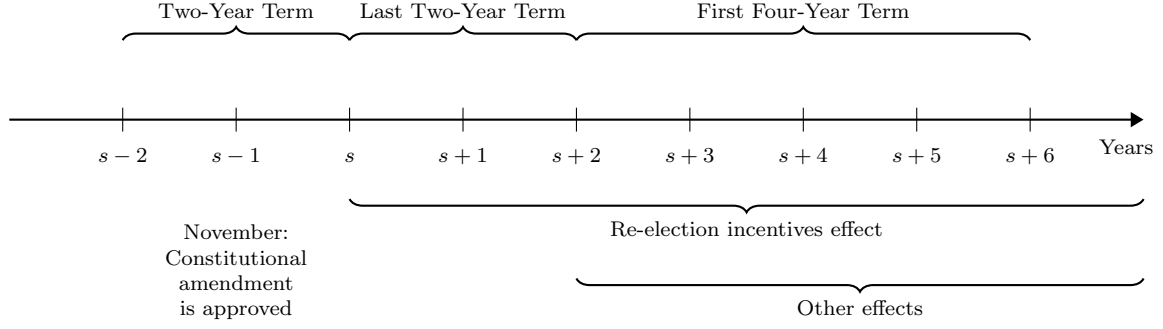
2020; Goodman-Bacon, 2021). Specifically, a key limitation of TWFE models is that units (in this case, states) treated in earlier periods are incorrectly used as a part of the control group for states that will extend the term length later. We address this concern by adopting a stacked-by-event design (e.g., Cengiz et al., 2019). The stacked-by-event design, also known as Difference-in-Differences on stacked data, requires manually constructing a dataset (denoted stack) for each treatment cohort s. In stack s, the treatment group comprises the states that receive treatment at time s. The control group includes only the states that will extend the term length at a date $s' > s$ or that will never extend the term length at time $s'' < s$ are not in the reconstructed data for cohort s. Each constructed dataset covers a fixed time window τ of years before and after the reform (i.e., from the year $s - \tau$ to the year $s + \tau - 1$).[24] The choice of τ is arbitrary but its pros- and the cons- are evident. On the one hand, increasing τ allows to estimate long-run effects and to evaluate the validity of the parallel trends assumption during a longer pre-treatment time window. On the other hand, reducing τ reassures that states that were similar before a reform remain comparable also after its implementation. Our main estimates are based on a window $\tau = 10$ years but we document in the Appendix that the results do not change when focusing on other windows (6 years; 14 years; 18 years).[25]

For example, consider the case of Rhode Island, which approved the constitutional amendment in November 1992, and a window of $\tau = 10$ years. We construct the stack for the $s = 1993$ cohort by i) removing from the sample all always treated states and all states that adopted a four-year term before 1993 and ii) limiting the time window to observations within a 10-year time window around 1992. That is, our stack $s = 1993$ covers three states (Rhode Island, New Hampshire, and Vermont), each observed between 1983 and 2002. The stack features a well-defined treatment group (Rhode Island), a well-

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[24]If a state approves the reform at a time s'' such that $s < s' \leq s + \tau$, such state enters stack s as a part of the control group for a window from the year $s - tau$ to the year $s'' - 1$.

[25]Windows of 6 years, 10 years, 14 years, and 18 years ensure that the sample always include full terms. For instance, a window of 6 years implies that three two-year terms prior to the constitutional amendment, one two-year and one four-year term after the amendment, and six two-year terms for the never-treated states in the sample.

defined control group (New Hampshire and Vermont), as well as ten pre-treatment periods (from year 1983 to 1992) and ten post-treatment periods (from 1993 to 2002) for both groups of states.

Lastly, we append together – as the name stacked-by-event suggests – all the cohort-specific stacks and estimate the following regression model:

$$y_{i,t,s} = \beta \times \text{Post Reform}_{i,t,s} + \eta_{i,s} + \delta_{t,s} + \varepsilon_{i,t,s} \quad \forall t \in [s - \tau; s + \tau - 1], \tag{1}$$

where $y_{i,t,s}$ is the dependent variable of interest (e.g., state total expenditures as a share of the state GDP) of state i in year t and cohort stack s.[26] Lastly, $\eta_{i,s}$ and $\delta_{t,s}$ are state-stack fixed effects and year-stack fixed effects, respectively. We cluster the standard errors at the state-presidential term level.[27] To ensure that the main results of the paper do not rely on our choice of adopting a stacked-by-event methodology, in the Appendix we replicate the main results using the estimator proposed by Callaway and Sant'Anna (2021).

Dynamic specification. As briefly discussed earlier, the credibility of our estimates relies on the parallel trends' assumption. To provide formal evidence about its validity in the context of our analysis, we also estimate a dynamic version of equation (1) on the stacked dataset:

$$y_{i,t,s} = \eta_{i,s} + \delta_{t,s} + \sum_{k \in [-10,9], k \neq -1} \alpha_k \mathbb{1}(\text{Years to reform}_{i,t,s} = k) + \varepsilon_{i,t,s}. \tag{2}$$

Estimating negligible and statistically insignificant coefficients $\alpha_{-10}; \alpha_{-2}$ would imply that states that are treated at different point in time were following parallel trends prior to the reform; such evidence would be strongly suggestive that the parallel trends assumption is not violated. Equation (2) is also helpful to visualize whether the effects of the reform on fiscal policy outcomes are visible during the last two-year term or, instead, materialize starting from the first four-year term.

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[26]Table A.4 in the Appendix presents the descriptive statistics as calculated in the stacked dataset.

[27]It is frequent practice to cluster the standard errors at the treatment level (Abadie et al., 2023). Thus, in the context of this analysis, the customary choice would be to cluster the standard errors at the gubernatorial term level. However, since the treatment is an extension of the term length, following the standard approach would force us to construct clusters of mechanically fewer observations in the pre-treatment period than in the post-treatment period. Instead, we cluster the standard errors at the state-presidential term level (i.e., four-year terms per each state) to ensure that the sample size of each cluster does not vary with the treatment. More conservative clustering strategies (e.g., state level) might result in severely inflated standard errors (Abadie et al., 2023) and do not appear reasonable when utilizing yearly data that span several decades.

5 Results

5.1 Size of the state government budget

Table 1 presents our main empirical results. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP. The estimates reported in column (1) of Panel (a) show that adopting a four-year term causes a reduction equal to 0.5 percentage points. Taking into consideration that the average spending level in the stacked dataset before the reform was 7.4 percent of GDP, our estimates imply that doubling the mandate of US state governors reduces state spending by 7 percent. In Panel (b), the dependent variable is the state total revenues as a share of the state GDP. We estimate an effect that is similar in magnitude to the one estimated in Panel (a), suggesting that the extension of gubernatorial term length did not affect state deficit significantly.[28]

In columns (2)–(5), we add controls for a variety of demographic, economic, and institutional characteristics. Column (2), which is our preferred specification, includes controls for pre-determined state characteristics interacted with year dummies. The coefficients estimated in column (2) are slightly smaller in magnitude than those presented in column (1) but the conclusions are unaltered. In column (3), we include time-varying controls for other state institutional reforms. Specifically, we control for dummies equal to 1 after the adoption of balance-budget requirements, civil service reforms, state budget stabilization funds, gubernatorial veto power, individual income tax, corporate income tax, general sale tax, and tax and expenditure limits, respectively. We also control for indicators equal to 1 after the introduction of a one- or two-term limit for incumbent governors, respectively. Very reassuringly regarding the validity of our empirical analysis, the estimated effects of the term length extension remain statistically significant and of similar magnitude.

In column (4), we control for time-varying demographic characteristics such as the state population and the share of kids and elderly individuals. The most conservative estimates, presented in column (5), in which we simultaneously include all the controls, confirm that longer gubernatorial terms cause a reduction in the size of the public sector. The estimated coefficient for the revenues loses its statistical significance at conventional levels but remains negative and similar to the estimates reported in the previous columns.[29]

Overall, the results presented in Table 1 show that governors secured in office for a longer period run smaller budgets than governors that are up for election every second

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[28]It is perhaps not surprising that we estimate comparable effect sizes on spending and revenues since the vast majority of states is subject to relatively strict balanced-budget requirements.

[29]Our preferred specification is the one presented in column (2) because the specifications in column (3), (4), and (5) includes controls for post-treatment variables that, for this reason, may be *bad controls*.
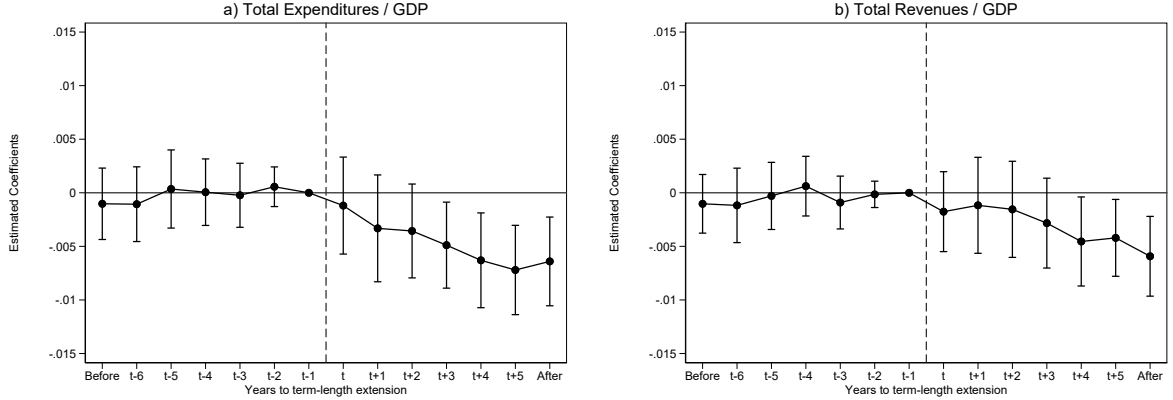
Table 1: Effect of adopting four-year term on state total expenditures and revenues

	(1)	(2)	(3)	(4)	(5)
Panel A. Dep. var.: Total expenditures / GDP					
Post Reform	–0.005***	–0.004***	–0.005**	–0.004**	–0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	4,620	4,620	4,620	4,620	4,620
R^2	0.919	0.964	0.922	0.923	0.969
Mean at baseline	0.0740	0.0740	0.0740	0.0740	0.0740
Panel B. Dep. var.: Total revenues / GDP					
Post Reform	–0.004***	–0.003**	–0.004**	–0.003**	–0.002
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Observations	4,620	4,620	4,620	4,620	4,620
R^2	0.931	0.972	0.936	0.934	0.977
Mean at baseline	0.0740	0.0740	0.0740	0.0740	0.0740
State-Stack FE	✓	✓	✓	✓	✓
Year-Stack FE	✓	✓	✓	✓	✓
Pre-determined covs × Year FE		✓			✓
Institutional covs			✓		✓
Demographic covs				✓	✓

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state total revenues as a share of the state GDP. The estimated equation is (1). In column (2), the specification includes the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). In column (3), the specification includes dummies for periods after the approval of a set of institutional reforms: the adoption of balance-budget requirements, the adoption of the civil service reforms, the adoption of state budget stabilization funds, the adoption of the gubernatorial veto power, the adoption of the individual income tax, corporate income tax and general sale tax, the adoption of tax and expenditure limits. The specification includes also dummies for periods after the introduction of one-term limit policy, and for periods after the introduction of two-term limits policy. In column (4), the specification includes controls for the state population and the share of kids and elderly individuals (time-varying). In column (5), the specification includes all controls utilized in columns (1)–(4). Standard errors robust to clustering at the state-presidential term level are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

year. This evidence is remarkably consistent with the cross-country correlation exercise performed in Figure 1. Drawing normative conclusions from these estimates alone is difficult, as they could either reflect that states were running on an excessively large budget prior to the reform or that excessive budget cuts took place after the amendment. However, the literature oftentimes considers similar results as an indication of improved government performance (e.g., Besley and Case, 1995).

Figure 5: Dynamic specifications

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state total revenues as a share of the state GDP. The estimated equation is (2). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.

Figure 5 presents the results obtained estimating equation (2). For both expenditures and revenues, coefficients estimated prior to the reform are statistically insignificant, negligible in magnitude, and do not follow a clear trend. After the reform, instead, the coefficients are consistently negative and tend to increase in magnitude over time. The evolution of the treatment effect over time does not come as a surprise: the reform increases the length of each future term; that is, the effects tend to reinforce themselves year after year. Moreover, the results presented in Figure 5 suggest that the effects materialize immediately upon the approval of the constitutional amendment – i.e., during the last two-year term. However, we lack sufficient statistical power to draw strong conclusions about this latter point.

5.2   Composition of the state budget

In Table 2, we estimate the effect of the reform on the composition of the state budget.[30] As discussed earlier, our data do not allow us to reconstruct a sufficiently long time series of program-by-program state expenditures or to identify separate tax revenue items. Nevertheless, our data allow us to compare, on the one hand, the effect of longer terms on current (net of interest payment) vs. capital spending and, on the other hand, the effect on tax revenues vs. grants from the federal government.

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[30]The results from estimating dynamic specifications as in equation (2) are reported in Figure A.3 in the Appendix.

Table 2: Effect of adopting four-year term on state expenditures and revenues categories

	(1)	(2)	(3)	(4)
Dep var.:	Current exp. / GDP	Capital exp. / GDP	Tax rev. / GDP	Grant rev. / GDP
Post Reform	–0.003***	–0.001	–0.000	–0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	4,620	4,620	4,620	4,620
R^2	0.971	0.916	0.955	0.965
Mean at baseline	0.0570	0.0160	0.0460	0.0190
State-Stack FE	✓	✓	✓	✓
Year-Stack FE	✓	✓	✓	✓
Pre-determined covs × Year FE	✓	✓	✓	✓

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Column (1), the dependent variable is the state current expenditures as a share of the state GDP; in Column (2), the dependent variable is the state capital expenditures as a share of the state GDP; in Column (3), the dependent variable is the state tax revenues as a share of the state GDP; in Column (4), the dependent variable is the amount of federal grants received by the state government as a share of the state GDP. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). Standard errors robust to clustering at the state-presidential term level are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

On the spending side, we document that the reform significantly reduces the state current spending – see Column (1) – while we cannot estimate a statistically significant reduction in capital spending – see Column (2). The combined evidence presented in Columns (1) and (2) of Table 2 allows us to be less parsimonious about drawing normative conclusions. If capital spending is more productive than current spending, then a relative increase in capital spending reflects that the state government is pursuing a more effective fiscal policy when gubernatorial terms are longer.[31] This result is in line with the intended aim of term length extension reforms. According to the proponents, longer terms would have ensured a more efficient state administration (Sabato, 1983). Moreover, we argue that longer terms should reinforce the incumbent governor's incentives towards allocating resources to investments: far-sighted policies such as new infrastructures require time and short gubernatorial terms discourage their production if voters do not reward the incumbent before they are constructed. In this regard, the results presented in Table 2 show that longer terms induce the governor to prioritize investments and other far-oriented policies over short-lived policies – more adequately proxied by current spending.

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[31] For example, Auerbach and Gorodnichenko (2012) estimate a 1.2 fiscal multiplier on current spending and a fiscal multiplier above 2 for investments.

On the revenues side, in Column (4) we estimate a sizable reduction in the amount of federal grants (approximately 10 percent compared to the baseline mean) while we do not estimate any average effect of the reform on tax revenues – see Column (3).[32] These results are perhaps surprising as one might expect the state government to cut taxes if current spending goes down and a balance-budget requirement is in place – as in most of our sample.[33] The negative impact of the reform on federal transfers sheds the light on the possibility that the reform may have affected also the incentives faced by the federal government. We investigate this channel in Section 7.

6   Mechanisms

The results reported and commented in Section 5 document that increasing the term length from two to four years reduces the size of the state budget. The effect is driven by a reduction of current spending and paired with a reduction of federal grants. In this section, we investigate the mechanisms underlying the main effects.

In principle, one may think of two types of mechanisms. On the one hand, the results may depend on the effect of longer terms on the cost-efficiency of the state government which is, in fact, the intended consequence that those advocating for longer terms were expecting (The Council of State Governments, 1952). Longer terms ensure that policymakers are less often distracted by political campaigns, that transitions of powers are less frequent, and that the top bureaucracy, which tends to be politically connected to the incumbent government, is replaced less frequently. If these *organizational* aspects of state government were the primary drivers, one would expect the effects of the term length extension to be homogeneous across states.

On the other hand, the term length may affect policymaking via the incentives to seek re-election and the characteristics of individuals who become governors (i.e., the combination of the self-selection of individuals into the gubernatorial race and the voters' selection among the candidates). First, extending the term length increases the monetary value of holding the gubernatorial office: if the annual salary remains constant, winning an election secures a stream of revenue that lasts four years instead of two. A higher value of holding office strengthen the incumbent's incentives to pursue re-election (Dal Bó and Rossi, 2011; Titiunik, 2016). Also, a higher value of holding office should attract individuals with a higher reservation wage (e.g., high-skill individuals), who could even-

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[32]Although tax revenues and federal grants are (by far) the largest component of state revenues, their sum does not coincide with the total revenues. We also estimate the effect of the term length extension on the other charges and miscellaneous revenues, without disclosing any statistically significant effect. The results are available on request.

[33]This result suggests that US state governments, despite the presence of balance-budget requirements, may be subject to the *soft budget constraint syndrome* (Kornai, 1986; Kornai et al., 2003) as the extra spending under a two-term rule was financed via federal grants rather than own sources.

tually perform better if elected. Second, longer terms imply that the incumbent may induce the incumbent to prioritize far-sighted policies whose effects would otherwise not materialize early enough to help gain votes in the next election.

We presented evidence in support of the latter channel in Section 5. Here we provide further evidence that longer term lengths affect policymaking not only via the *organizational* channel. More precisely, we show that (i) the effect of the reform on the state fiscal policy depends on the presence and the strength of contingent electoral incentives faced by the incumbent governor and (ii) that longer terms attract individuals with better observable quality traits than those attracted by shorter terms as gubernatorial candidates (and, in turn, as governors).

6.1   Incentives

In Figure 6, we document that the reduction in the size of the state budget is larger if the governor have stronger re-election incentives.[34] In Figure 6, coefficients reported in green denote observations characterized by strong electoral incentives, while coefficients reported in red refer to less stringent (or absent) incentives.

We consider three measures of electoral incentives. First, we divide the sample based on whether the share of families using the radio in 1930 in the state was above or below the median share in our sample. At the beginning of our observation window, the radio was one of the most prominent sources of local information. We expect that voters in states with more radios were better informed about the performance of the incumbent governor and, in turn, could more easily remove unfit incumbents at the time of the election.[35] Consistent with this hypothesis, we estimate that the effect of longer gubernatorial terms on both the state expenditures and the state revenues is stronger in states where the radio historically covered a large fraction of the population.

Second, we divide the sample based on whether the state law allows the incumbent governor to run again at the next election (either because the state does not have any term-limit policy or because the governor has not reached the limit yet). We find that the average effect of the term length extension is driven by governors that can compete in the next election.[36] Third, we divide the sample based on whether the average share of votes obtained by governors elected before the reform (within the window τ) is above or below the median value across all states in our sample. The intuition is that the incumbent governor likely expects fierce competition to occur in the next election if the state has a recent historical record of tight gubernatorial races. Again, we provide

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[34]In Figure A.4 in the Appendix, we replicate the analysis for the other outcomes.

[35]This argument builds on the extensive literature that documents a strong causal effect of the availability of information through mass media on the performance of the incumbent politician (see, e.g., Strömberg, 2004, on the impact of the radio).

[36]Notice that we cannot produce estimates for the sub-sample of governors exposed to term limits due to multi-collinearity.

Figure 6: Incentives

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state total revenues as a share of the state GDP. Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1, while coefficients and confidence intervals reported in green and red report, respectively, the effects estimated in sub-samples defined on the vertical axis: green coefficients and confidence intervals refer to values that satisfy the condition, relative to the median observation, while red coefficients and confidence intervals refer to values that do not satisfy the condition, relative to the median observation. The *Contestable Gubernatorial Elections* indicator is created by averaging the share of votes across all elections occurred within a window of 10 years before the reform in the treated state(s). The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level. Notice that we cannot produce estimates for the sub-sample of governors exposed to term limits due to multi-collinearity.

evidence that electoral incentives matter: the effect of extending the gubernatorial term length is stronger in electorally contestable states.

6.2   Selection

Studying whether longer terms increase the quality of gubernatorial candidates requires collecting information on individual characteristics of winning and losing candidates for several elections across multiple states and years. While readily existing databases offer satisfactory coverage of elected governors and/or of recent years, the coverage of background characteristics of defeated candidates is scarce. As briefly discussed in Section 3, we construct a novel database with information on education and previous political experience of gubernatorial candidates running for office between 1938 and 2008 for a

total of 595 elections.[37] Overall, we retrieve information for all elected governors and for 464 defeated gubernatorial candidates (which represent 71 percent of losing candidates). Table A.5 in the Appendix reports the descriptive statistics of the relevant variables both at the individual candidate level and upon transforming the data into our state-year-stack panel.

In Table 3, we estimate equation (1) on two individual quality traits of gubernatorial candidates that predict effective economic performance: an indicator for having acquired college education and an indicator for having acquired some political experience in the State administration prior to running for the position of governor. The results presented in Table 3 confirm the hypothesis that longer terms attract individuals with stronger (observable) background: the reform increases both the average education level of gubernatorial candidates and their average previous experience in the State administration. The results reported in Columns (1)–(3) – where we focus on elected governors – and the results in Columns (4)–(6) – where the dependent variables measure the average values across all candidates – are remarkably similar. This evidence suggests that the effect of the reform on the self-selection of individuals into candidacy is homogeneous for front-runners and other candidates.[38]

7   Implications

Our main results are driven by the incentives that longer terms offer to incumbent governors and to the impact on the term-length on the observable quality traits of individuals who decide to pursue candidacy. Longer terms increase the monetary value of holding the gubernatorial office and a common worry in the literature is that this may lead to more opportunistic behavior by the incumbent. Although an higher value of holding office leads to an overall improvement in public finances, it is possible that such reinforced incentives come with some unintended side effects. In this section, we hypothesize and provide formal empirical evidence that longer term lengths improve fiscal policy along one important dimension such as the macroeconomic stabilization function. At the same time, longer terms tend to exacerbate two primary political distortions: political budget cycles and strategic allocations of inter-governmental resources.

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[37]In order to obtain information that are comparable between elected governors and defeated candidates, we manually searched through the Wikipedia pages of all gubernatorial candidates (provided that the page exist). Candidates that do not have a Wikipedia page are excluded from our sample. We complemented data from the candidate Wikipedia page with additional data from complementary sources such as the National Governors Association, state encyclopedias, and Ballotpedia for 112 candidates whose Wikipedia page did not provide sufficient information.

[38]This evidence shows that the negative voter selection, briefly discussed at the beginning of this Section, is unlikely at play. We do not find any empirical evidence that the reform induces voters to choose the less qualified candidate.

Table 3: Effect of adopting four-year term on governors and gubernatorial candidates' individual characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	Attended College	Completed College	Experience in State Admin.	Attended College	Completed College	Experience in State Admin.
	Elected governor			All gubernatorial candidates		
Post Reform	0.095*	0.169**	0.186***	0.088**	0.183***	0.099*
	(0.051)	(0.067)	(0.062)	(0.042)	(0.060)	(0.056)
Observations	3,960	3,960	3,960	3,960	3,960	3,960
R^2	0.543	0.655	0.577	0.604	0.625	0.567
Mean at baseline	0.930	0.838	0.167	0.908	0.811	0.188
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Pre-determined covs × Year FE	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Column (1), the dependent variable is an indicator that takes the value 1 if the elected governor attended some college education; in Column (2), the dependent variable is an indicator that takes the value 1 if the elected governor holds a college degree; in Column (3), the dependent variable is an indicator equal to 1 if the elected governor has some previous experience as a member of the state administration/cabinet. In Column (4), the dependent variable is the share of gubernatorial candidates that attended some college education; in Column (5), the dependent variable is the share of gubernatorial candidates that hold a college degree; in Column (6), the dependent variable is the share of gubernatorial candidates who have some previous experience as a member of the state administration/cabinet. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). Standard errors robust to clustering at the state-presidential term level are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

7.1 Implications for macroeconomic stabilization

A common approach to evaluate fiscal policy across institutional frameworks is to measure how institutions are capable of dealing with economic crises by increasing government intervention in the economy when necessary and, conversely, to limit its scope during periods of economic growth (e.g., Persson and Tabellini, 2003).

In Figure 7, we divide our sample based on the state GDP growth in year t as compared to the median value (calculated across all states in our sample for the same year t) and estimate equation (1) in each sub-sample. Although the difference between the effect sizes estimated in the two sub-samples is not statistically significant, the reduction in the size of government spending caused by the extension of the gubernatorial term length
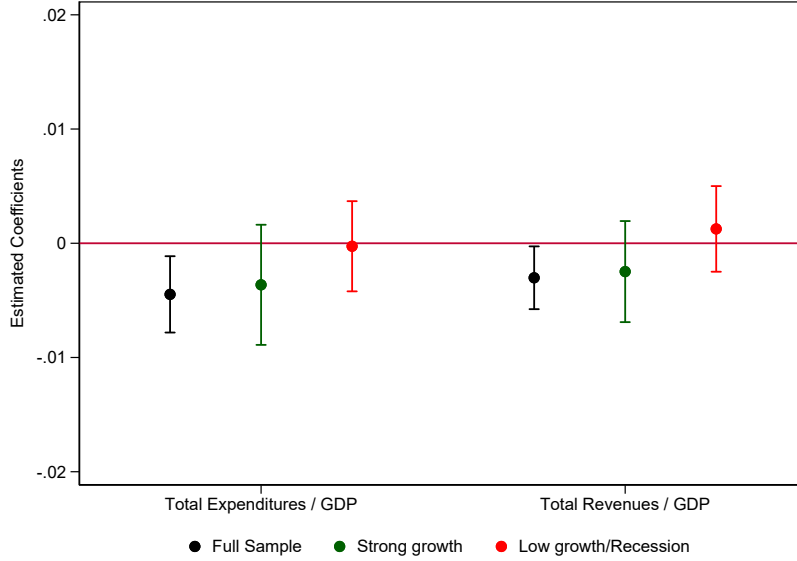
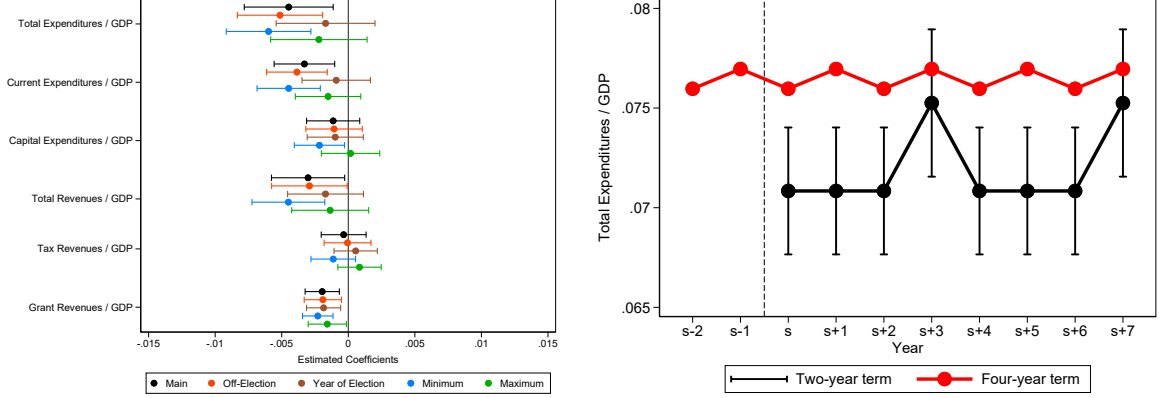Figure 7: Effect of adopting four-year term by the state's economic conditions

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state total revenues as a share of the state GDP. Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1, while coefficients and confidence intervals reported in green and red report, respectively, the effect estimated in periods of strong economic expansion (i.e., states such that the year-on-year GDP growth is greater than the median value in our sample for the same year) and in periods of slow economic growth or recession (i.e., states such that the year-on-year GDP growth is below the median value in our sample for the same year). The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.

is concentrated only in periods of strong economic expansion.[39] State governments are equally capable of addressing an economic recession under a two-year term and a four-year term but the longer term length ensures a relatively lower level of government spending during periods of economic growth. These results corroborate the normative conclusions drawn in Section 5: governors in power for a four-year term realize more effective fiscal policy than governors elected for a two-year term.

7.2   Implications for political budget cycles

If the governor serves for two years before a new election, one is pre-election and one is post-election. Hence, opportunistic governors may attempt to inflate the size of the public sector in the pre-election year and then reduce it after the election. Under a four-

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[39]In Figure A.5 in the Appendix, we replicate the exercise for the outcome variables used in Table 2.

(a) Formal estimates	(b) Visualization

Figure 8: Political Budget Cycle

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (c), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (d), the dependent variable is the state total revenues as a share of the state GDP; in Panel (e), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (f), the dependent variable is the amount of federal grants received by the state government as a share of the state GDP. Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1. Coefficients and confidence intervals reported in orange refer to regressions in which the dependent variables are measured as the average level of the outcome across the off-election years of a term – i.e., the post-election year under a two-year term length and three post-election years under a four-year term length. Coefficients and confidence intervals reported in brown refer to regressions in which the dependent variables are measured as their value in the year of the gubernatorial election. Light blue coefficients and confidence intervals refer to regressions in which the dependent variables are measured as their minimum value during the term. Green coefficients and confidence intervals refer to regressions in which the dependent variables are measured as their maximum value during the term. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.

year term, instead, in addition to one year that approaches the election and one year that follows the election – i.e., the pre-election and post-election periods, respectively – the term includes also two intermediate years. Therefore, it is reasonable to expect that the term length will have a large effect on the political budget cycle of state spending and revenues.

Constructing an empirical test of how longer terms affect the political budget cycle requires some care for the reasons explained just above. In Panel (a) of Figure 8, we estimate equation (1) by focusing on specific years within each term. Coefficients shown in orange correspond to regressions in which the dependent variable focuses only in off-election years. This definition implies that the regression compares the first year of a

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two-year term with the average of the first, second, and third years of a four-year term.[40] By contrast, coefficients reported in brown compare the final year of a two-year term with the final year of a four-year term – that is, the year in which the next election takes place (in November). Our results show that, across all dependent variables, the effects of longer gubernatorial terms are strong and statistically significant in off-election years. In the election year, instead, we estimate a small and statistically significant negative effect.

In Panel (b), we take the estimates obtained in Panel (a) and offer a visualization of our empirical results. We focus for simplicity on total expenditures (i.e., the estimates presented in the upper box of panel (a), but analogous procedures can be applied to every budget variable). The red cycle refers to two-year terms, while the black cycle represents four-year terms.[41] Total expenditures are consistently lower under a four-year term than under a two-year term, although the difference between the two institutional regimes is much larger in off-election years (i.e., $s+2$ and in $s+6$) than in election years (i.e., $s+3$ and $s+7$). Panel (b) also allows to visualize the estimated effect in years in which an election would have occurred under a two-year gubernatorial term but does not because of the reform (i.e., $s+1$ and $s+5$). The estimated effects is maximal on those years. Taken together, the results presented in Figure 8 document that the election-year budget spikes at similar levels under both institutional regimes; however, longer terms are associated with a deeper and more prolonged reduction in spending away from elections. These results highlight an unintended consequence of longer terms: longer terms result in greater within-term volatility of public finances (i.e., the breadth of the political budget cycle) which may increase economic policy uncertainty.[42]

7.3 Implications for intergovernmental allocation of resources

In Table 2 and Figure A.3, we documented that the negative effect of longer terms on state revenues is primarily driven by a reduction in the amount of grants that the federal government assigns to the state government. We hypothesize that the reduction may not be distributed across state governments equally: an extensive literature has

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[40]Figure A.6 in the Appendix shows that the results are robust to alternative definitions of off-election years, such as considering only the first year after an election or only the first half of the term (i.e., the year after the election for two-year terms versus the average of the first and second years for four-year terms).

[41]The numbers imputed in panel (b) of Figure 8 are calculated as follows: the values for off-election years and election years for two-year terms are computed using the sample mean of total expenditures (as a share of GDP). For four-year terms, instead, we diminish those means by the estimated treatment effects for the coefficients estimated in the upper box of Panel (a) – respectively, for off-elections years and for election years.

[42]This evidence is further confirmed by comparing the estimated coefficients and 95% confidence intervals reported in light blue – in which the dependent variable is the minimum level of the outcome measured at any point in time during the term, which usually is in the off-election period – with the coefficients reported in green – in which the dependent variable is the maximum level of the outcome during the term – which usually coincides with the year of the election.
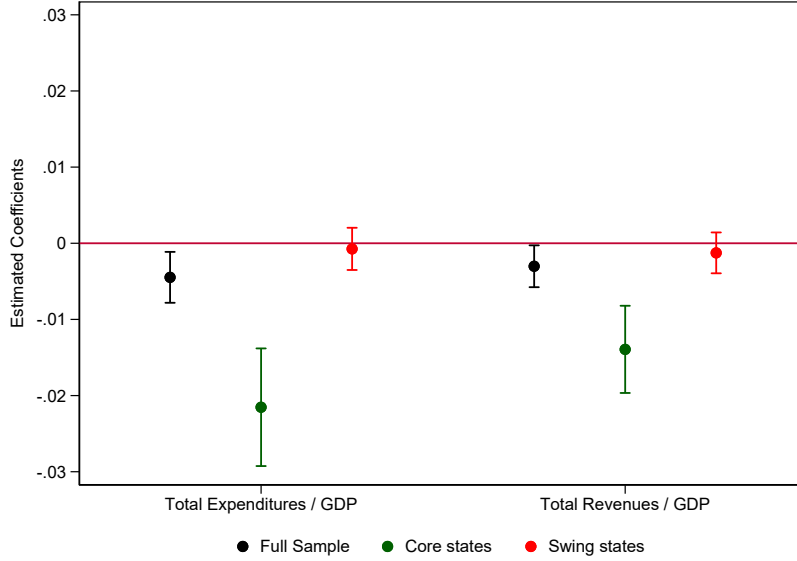
Figure 9: Core states vs. Swing states

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state total revenues as a share of the state GDP. Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1, while coefficients and confidence intervals reported in green and red report, respectively, the effects estimated in sub-samples defined based on whether the state is a *core* state or a *swing* state. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.

documented that central governments tend to distort the allocation of resources to lower-tier governments by assigning more grants to localities with high density of swing voters (Dahlberg and Johansson, 2002; Dixit and Londregan, 1996). This behavior is particularly accentuated in the United States because of the Electoral College voting system adopted in the presidential election (Strömberg, 2008).

Figure 9 compares the effect of extending the gubernatorial term length from two to four years in core vs. swing states. Detecting comparable effects in the two groups would indicate that the strategic considerations in the inter-governmental allocation of resources do not depend on the length of gubernatorial terms. On the contrary, estimating that the reduction in the amount of federal grants received (and in the overall budget size) is stronger in swing states (resp., core states) than in core states (resp., swing states) would strongly suggest that the gubernatorial term length also affects inter-governmental relations.

Why may longer gubernatorial terms affect the strategic considerations in the inter-governmental allocation of resources? The governor is responsible for investing the federal

grants that enter the state balance sheet. Longer terms reduce the uncertainty that the federal government faces upon deciding the amount of resources to assign to each state. Indeed, longer terms increase the likelihood that the same governor that is in office when grants are allocated will also be responsible for investing the amount. If this conjecture holds, then longer terms should worsen allocation distortions – i.e., to increase the gap between states in which the incumbent U.S. president experiences large support and swing states.

In Figure 9, we present empirical evidence which strongly supports this hypothesis. More specifically, we estimate equation (1) in two mutually exclusive sub-samples, defined based on whether the absolute value of the distance between the share of votes received by the incumbent US president in state i and 50 percent is above or below the median (calculated among all states in our sample in the same election). Our empirical results demonstrate that the reduction in the size of the state budget due to longer term lengths is stronger in states in which the incumbent US President won or lost the state presidential race by a large margin of votes. In Figure A.7 in the Appendix, we focus on the composition of the state budget. Unsurprisingly, we find that longer terms cause a reduction in the amount of federal grants received by the state government only in core states (i.e., states in which the electoral support for the incumbent US president is either low or high). Moreover, governors holding office in *core* states seem to reconstruct their budget more extensively, as we do also estimate for this sub-sample a negative effect of longer terms on capital spending – the effect was insignificant in the full sample. Conversely, the negative effect on state current spending is homogeneous across the two subgroups.

Taken together, the results presented in Figure 9 and Figure A.7 shed new light on another unintended negative consequence of longer executive terms. Longer terms increase the salience of political distortions in the inter-governmental allocation of resources – a direct consequence of our estimates is that the financial benefit of being a *swing* state is more pronounced when the gubernatorial term lasts four years compared to when the term lasts two years.

8   Robustness checks

This section presents an extensive battery of robustness checks to validate our main results.

Placebo analysis: failed attempts to extend the term length.   We exploit the requirement that a majority of voters must approve constitutional amendment proposals in a statewide election to conduct a placebo analysis. In our data, we observe many failed attempts to extend the governors' term length from two to four years due to the

proposed amendment not reaching the required majority threshold. These cases are ideal to perform a *placebo* analysis because failed attempts should not have any impact on subsequent economic policy decisions. Identifying an effect of the placebo treatment would raise concerns about our empirical analysis. In Table A.6, we estimate a version of equation (1) in which we replace the Post Reform$_{i,t,s}$ indicator with the indicator Post Failed Attempt$_{i,t,s}$, which takes the value one in the years after the failed attempt. We also reconstruct the stacked panel accordingly – one stack per each failed attempt in which the treatment group receives the *placebo* treatment at time s and the control group is restricted to states that have not yet received the *placebo* treatment or the term length extension before time s.[43] The results presented in Table A.6 document that statistically significant *placebo* effects cannot be estimated for any of the outcome variables.

Changes in the time window τ used to construct the stacked dataset. Our main estimates are based on constructing a stacked state-cohort-year panel that covers $\tau = 10$ years before or after each state reform s. In Figure A.8, we provide evidence that our results are not contingent on our choice of $\tau = 10$. Specifically, we replicate all our main results for a smaller window of $\tau = 6$ and larger windows $\tau = 14$ and $\tau = 18$. The estimates are qualitatively unaltered, suggesting that the short- and long-run effects of extending the gubernatorial term length are comparable.

Alternative estimators for staggered Difference-in-Differences. In Table A.7, we report the coefficients obtained utilizing the estimator proposed by Callaway and Sant'Anna (2021). The Callaway and Sant'Anna (2021) estimator is a suitable alternative to the stacked-by-event specification to estimate a Difference-in-Differences model when the treatment timing is staggered. The results document that our conclusions hold also using the Callaway and Sant'Anna (2021) estimator. If anything, the estimated coefficients reported in Table A.7 are larger in magnitude than the results presented in Tables 1 and 2. We signal only a small discrepancy between the two estimators: in Table A.7, we detect a very small (0.3 percentage points of GDP) increase in capital spending – which according to our main estimates is not significantly affected by the reform.[44]

Excluding that reforms to term limits confound the results. Besley and Case (1995) estimate that the presence of term limits shapes the fiscal policy of US state governments. Therefore, it is paramount to document that our results do not depend on a limited number of states that amended their term limit policies at the same time of or

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[43]When constructing the stacked panel for this exercise, we ensure that states are excluded from the sample when they eventually adopt an actual four-year term.

[44]Despite this discrepancy, the Callaway and Sant'Anna (2021) estimator confirms that longer term length cause a reduction in the size of the state budget and an increase – actually, even stronger – in the capital-to-current spending ratio.

close to the extension of the term length.

First, we remind the reader that the specifications estimated in Columns (3) and (5) of Table 1 speak loudly against this risk because they include dummies for the adoption of term limit policies. Nevertheless, in Figure A.9, we assess the robustness of our estimates to concurrent or quasi-concurrent reforms to term limits. We compare our main results with effects estimated in the sub-sample of states-cohorts in which term limit policies remain constant throughout the window $[s - \tau; s + \tau - 1]$ and with the effects estimated in the sub-sample of states-cohorts in which term limit policies were never effective during the window $[s - \tau; s + \tau - 1]$. The coefficients obtained in these sub-samples of states are consistently similar to those estimated in the full sample.

Alternative definitions of the dependent variables. In Table A.8, we establish that our conclusions do not depend on the functional form utilized to specify our dependent variables. Our results survive when measuring our dependent variables using the level (in real per-capita dollars) of state spending and revenues or when adopting logarithmic transformations. Also, our results are not affected by using bi-yearly data instead of relying on yearly budget entries.

Additional robustness checks. First, in Table A.9, we replicate the results obtained in Table 1 by extending the sample to New Jersey, which extended the gubernatorial term length from three years to four years in 1947. The results remain qualitatively unaltered. Second, in Figure A.10, we report the coefficients for Post Reform$_{i,t}$ obtained removing each of the state reforms from the sample, one at a time. Figure A.10 reassures that none of the treated states in our sample is essential to estimate a negative effect of the extension of the term length. Third, in Table A.10, we extend our panel backwards until 1922. Extending the sample back to 1922 allows us to leverage the reforms approved in South Carolina in 1926 and in New York in 1937.[45] The results presented in Table A.10 confirm that the term-length extension reduced state spending and revenues also when New York and South Carolina are included in the sample.

9 Concluding remarks

What is the *optimal* length of political officers' term? Should societies aim for longer terms, which improve the continuity of the bureaucratic activity, may attract higher-quality individuals into the political profession, and provide incentives to focus on far-

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[45]Our main sample starts in 1937 because budget data are missing for the years 1933–1936 and coverage is incomplete for 1932. Moreover, data on state personal income is only available since 1929. To perform this robustness check, we complete the panel for the years in which budget data are missing by performing a linear interpolation of each state's budget entry and we utilize the level of state GDP spending and revenues in 1982 per capita US dollars (i.e., not rescaling the dependent variables by the state GDP).

oriented policies? Or, conversely, should we aim for shorter political terms to ensure that voters can replace poorly-performing incumbents promptly and that the elected officials' preferences align with those of the median voter? A consensus is lacking as we observe a sizable variation in term lengths across countries and across positions within the same country.

In this paper, we provide new empirical evidence on the effect of executive term length on government fiscal policy by focusing on the case of US State Governors, which is relevant for our purposes for a variety of reasons. Crucially, governors have the authority to meaningfully impact the policy decisions taken by their administration. Moreover, each state independently decides the gubernatorial term length via constitutional amendments. This institutional feature allows us to identify the causal effect of extending the length of governor's term in office from two years to four years using a staggered Difference-in-Differences.

At the beginning of our sample, which spans more than seventy years of data, twenty-three states were following a two-year term. Over time, twenty-one of them amended their Constitution to grant a four-year term to elected governors. Our results document that the extension of the term length causes a significant improvement in the state's budget. Specifically, we estimate a reduction in current spending concentrated in periods of economic growth. Thus, increasing the gubernatorial term reduced the overall size of the public budget whilst impacting neither the fiscal space for counter-cyclical macroeconomic policy nor the realization of far-sighted policies. The estimated effects come from a combination of stronger incentives to perform in a way that increases the chances of re-election and the selection of higher-quality individuals as governors.

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Appendix

A   Figures and Tables
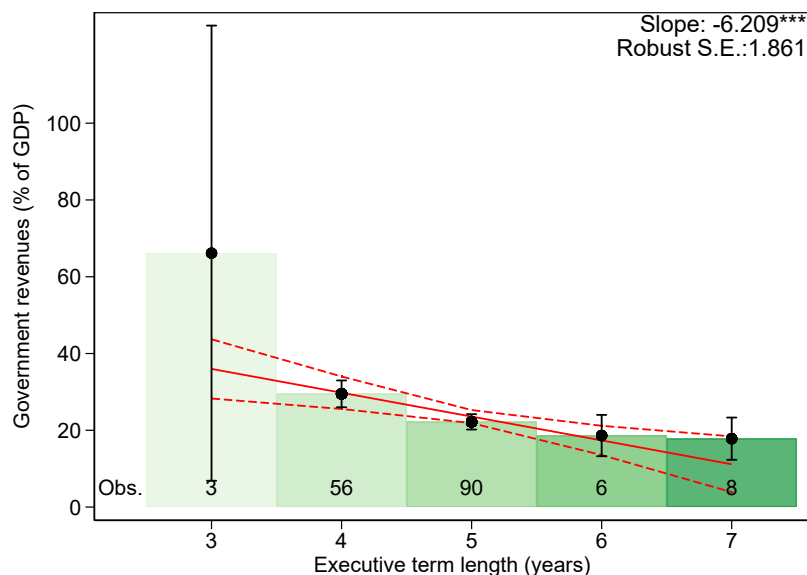
Figure A.1: Cross-country correlation between Head of Government's term length and government revenues

Notes: The unit of observation is a country. Executive term length is calculated based on presidential terms (presidential countries) or on lower house terms (parliamentary countries). The figure reports the average government revenues as a share of GDP for each group of countries and the binary correlation between government revenues (excluding grants) over GDP and executive term length. The dependent variable is each country's government revenue (excluding grants) as a percentage of each country's GDP. Executive term length is calculated based on presidential terms (presidential countries) or on lower house terms (parliamentary countries). San Marino (which is the only country in which the term for the two executive officers lasts 6 months) and countries in which executive term lengths are unlimited are excluded. See Appendix B for further information on the dataset utilized for this analysis. 95% confidence intervals are based on standard errors robust to heteroskedasticity.
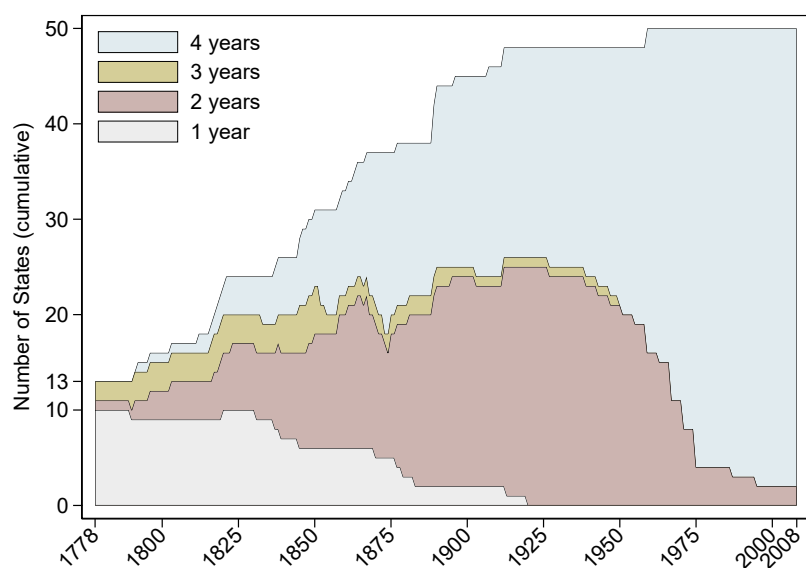
Figure A.2: Evolution of gubernatorial term length across US states

Notes: The figure shows the evolution of gubernatorial term length since the foundation of the United States. The gray-shaded area counts the number of states subject to one-year term length. The brown-shaded area counts the number of states subject to two-year term length. The light brown-shaded area counts the number of states subject to three-year term length. Lastly, the light blue-shaded area counts the number of states subject to four-year term length. At each point in time, the sum between the four shaded areas represent the total number of existing states (i.e., between 13 in 1778 and 50 in 2008).

Figure A.3: Dynamic specifications

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (c), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (d), the dependent variable is the amount of federal grants received by the state as a share of the state GDP. The estimated equation is (2). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.

Figure A.4: Incentives

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (c), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (d), the dependent variable is the amount of federal grants received by the state as a share of the state GDP. Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1, while coefficients and confidence intervals reported in green and red report, respectively, the effects estimated in sub-samples defined on the vertical axis: green coefficients and confidence intervals refer to values that satisfy the condition, relative to the median observation, while red coefficients and confidence intervals refer to values that do not satisfy the condition, relative to the median observation. The *Contestable Gubernatorial Elections* indicator is created by averaging all elections occurred within a window of 10 years before the reform in the treated state(s). The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.

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Figure A.5: Effect of adopting four-year term by the state's economic conditions

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (c), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (d), the dependent variable is the amount of federal grants received by the state as a share of the state GDP. Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1, while coefficients and confidence intervals reported in green and red report, respectively, the effect estimated in periods of strong economic expansion (i.e., states such that the year-on-year GDP growth is greater than the median value in our sample for the same year) and in periods of slow economic growth or recession (i.e., states such that the year-on-year GDP growth is below the median value in our sample for the same year). The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.

Figure A.6: Political Budget Cycle

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (c), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (d), the dependent variable is the state total revenues as a share of the state GDP; in Panel (e), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (f), the dependent variable is the amount of federal grants received by the state government as a share of the state GDP. Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1. Coefficients and confidence intervals reported in green refer to regressions in which the dependent variables are measured as the level of the outcome in the first year of a term – i.e., the post-election year. Coefficients and confidence intervals reported in brown refer to regressions in which the dependent variables are measured as the average level of the outcome across the first half of a term – i.e., the post-election year under a two-year term length and two post-election years under a four-year term length. Coefficients and confidence intervals reported in orange refer to regressions in which the dependent variables are measured as the average level of the outcome across the off-election years of a term – i.e., the post-election year under a two-year term length and three post-election years under a four-year term length. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.
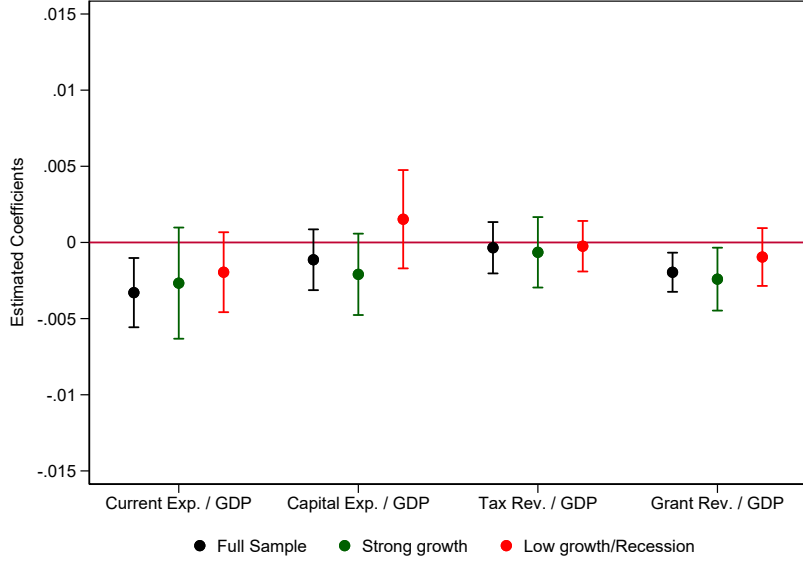
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Figure A.7: Core states vs. Swing states

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (c), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (d), the dependent variable is the amount of federal grants received by the state as a share of the state GDP. Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1, while coefficients and confidence intervals reported in green and red report, respectively, the effects estimated in sub-samples defined based on whether the state is a *core* state or a *swing* state. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.
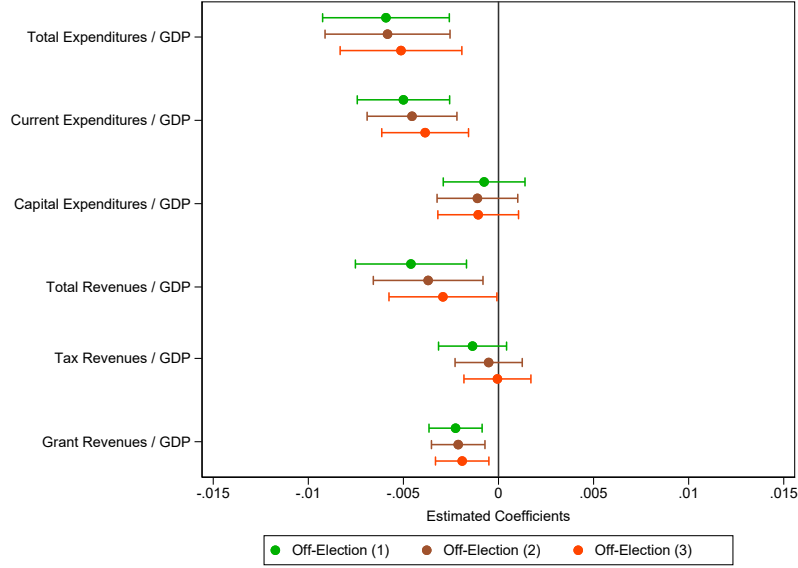
Figure A.8: Effect of adopting four-year term on state expenditures and revenues –
multiple time windows

Notes: The unit of observation is a state-year-stack. In each regression, as specified in the horizontal axis, each stack is reconstructed to include data contained within a different time window – i.e., from -6/+6 years since the reform in the treated state(s) to -18/+18 years since the reform in treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state total revenues as a share of the state GDP; in Panel (c), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (d), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (e), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (f), the dependent variable is the amount of federal grants received by the state as a share of the state GDP. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.

Figure A.9: Effect of adopting four-year term on state expenditures and revenues – by presence/absence of term limits

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (c), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (d), the dependent variable is the state total revenues as a share of the state GDP; in Panel (e), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (f), the dependent variable is the amount of federal grants received by the state as a share of the state GDP. The estimated equation is (1). Coefficients and confidence intervals reported in black replicate the results estimated in Column (2) of Table 1. Coefficients and confidence intervals reported in brown report the effects estimated in the sub-sample of state-stacks for which a gubernatorial term limit was not in place throughout the entire observation period. Coefficients and confidence intervals reported in orange report the effects estimated in the sub-sample of states-stacks for which a gubernatorial term limit was either always in place (one-term, two-term, or more) or never in place throughout the entire observation period. 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.
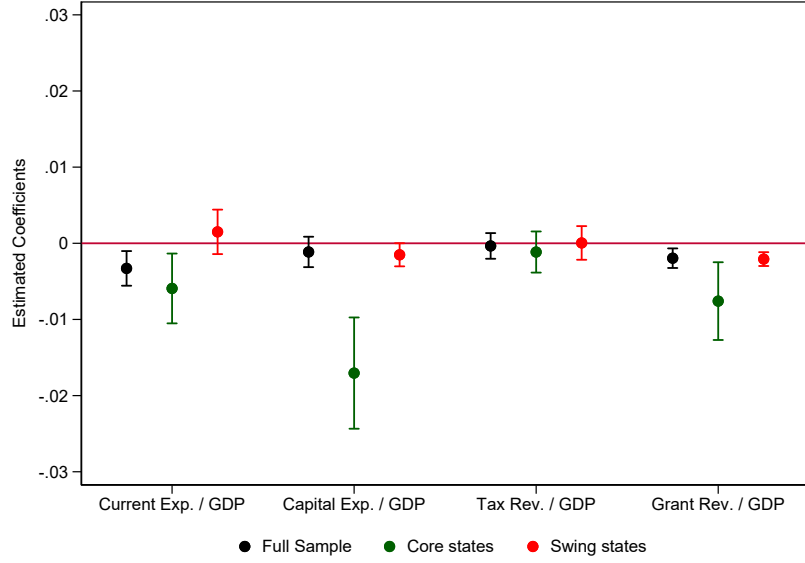
Figure A.10: Robustness check: Remove one stack from the sample

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable is the state total expenditures as a share of the state GDP; in Panel (b), the dependent variable is the state current expenditures as a share of the state GDP; in Panel (c), the dependent variable is the state capital expenditures as a share of the state GDP; in Panel (d), the dependent variable is the state total revenues as a share of the state GDP; in Panel (e), the dependent variable is the state tax revenues as a share of the state GDP; in Panel (f), the dependent variable is the amount of federal grants received by the state as a share of the state GDP. The estimated equation is (1). At each iteration, we remove one stack from the estimation sample as indicated in the horizontal axis. 95% confidence intervals are reported in brackets and are based on standard errors robust to clustering at the state-presidential term level.
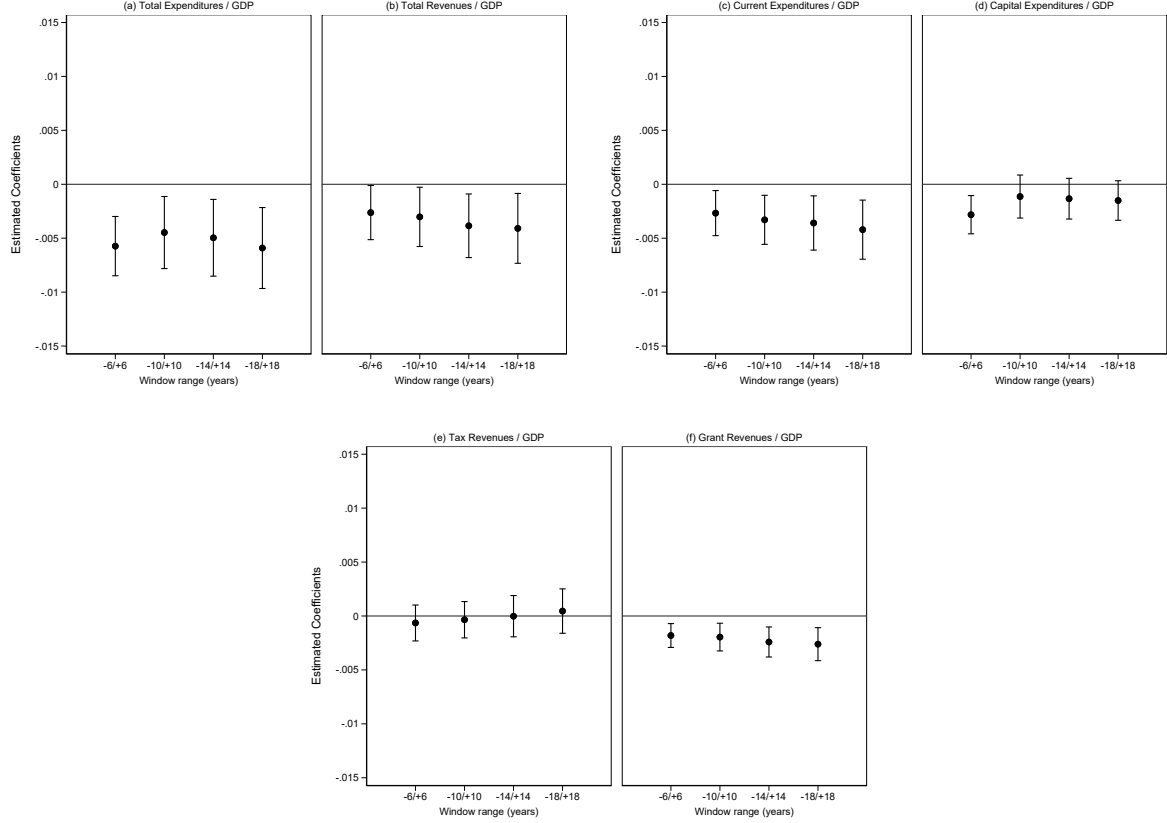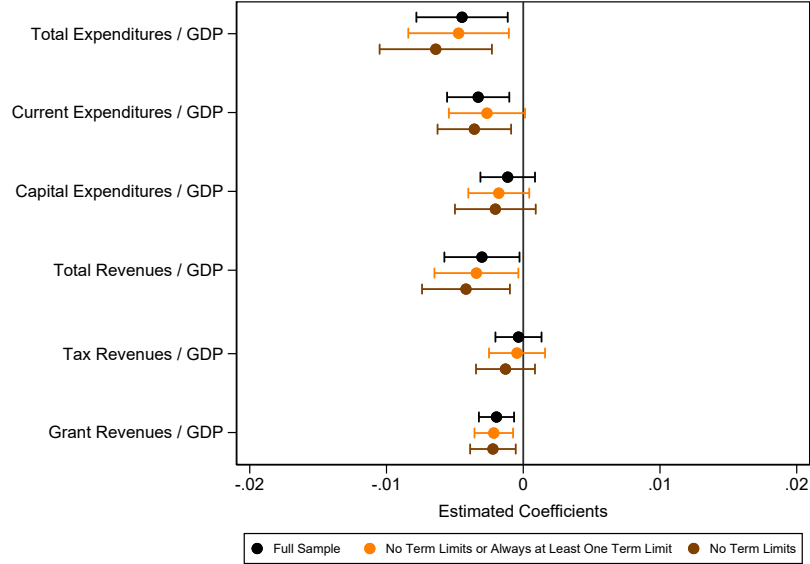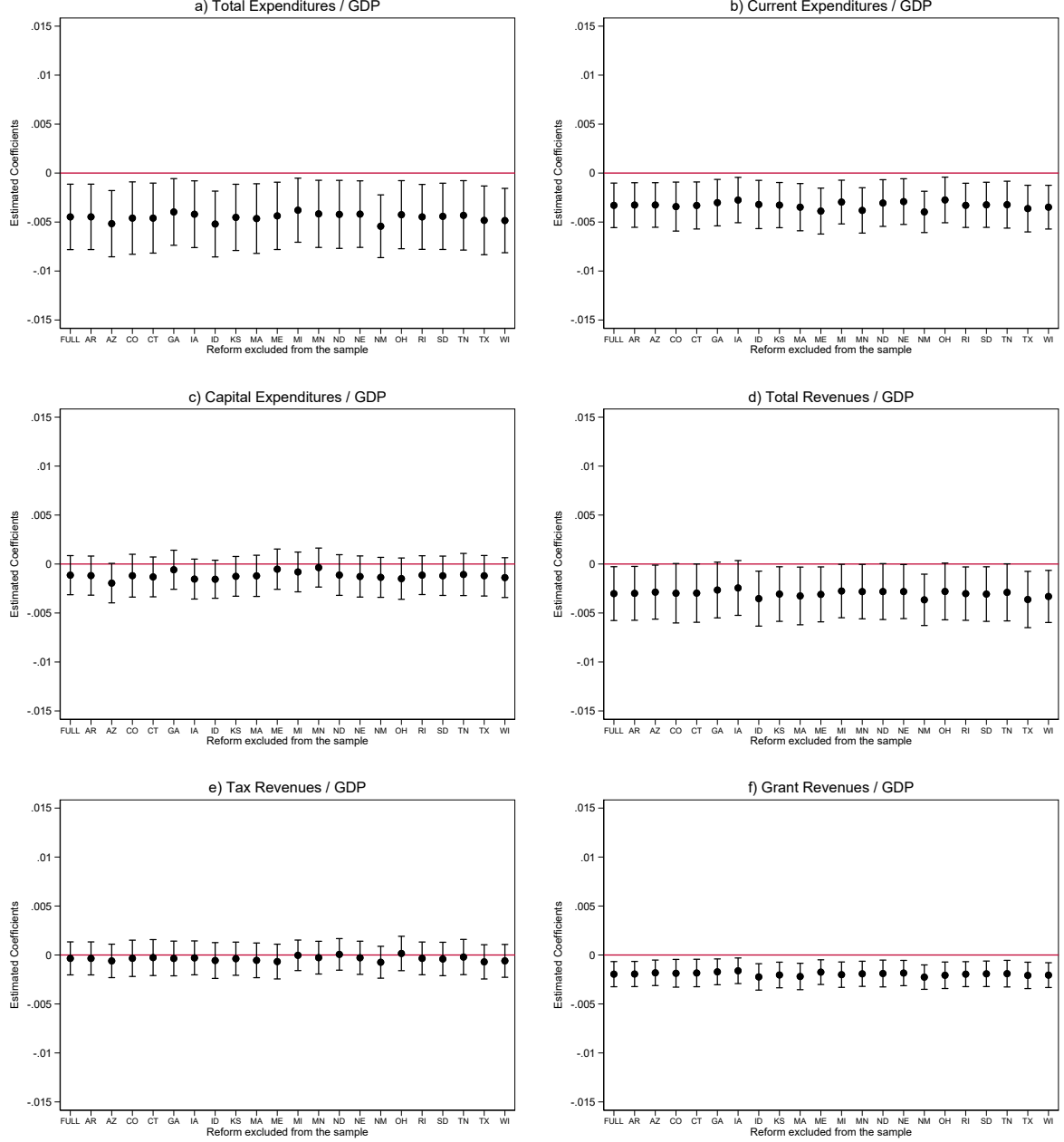
Table A.1: Cross-country correlation between Head of Government's term length and the size of the public sector

	(1)	(2)	(3)	(4)	(5)
Panel A. Dep. var.: Government spending (% of GDP)					
Executive term length (years)	–6.770***	–3.356***	–3.256***	–3.454**	–3.457**
	(1.405)	(1.232)	(1.220)	(1.507)	(1.509)
Observations	164	164	164	164	163
R^2	0.151	0.423	0.668	0.741	0.741
Mean dep. var.	27.91	27.91	27.91	27.91	27.81
Panel B. Dep. var.: Government revenues (% of GDP)					
Executive term length (years)	–6.209***	–3.099**	–2.756***	–1.845*	–1.847*
	(1.862)	(1.412)	(0.985)	(1.086)	(1.087)
Observations	164	164	164	164	163
R^2	0.129	0.379	0.663	0.711	0.711
Mean dep. var.	25.26	25.26	25.26	25.26	25.17
Population		✓	✓	✓	✓
Geographical controls		✓	✓	✓	✓
World region FE		✓	✓	✓	✓
Population weights			✓	✓	✓
Parliamentary FE				✓	✓
Regime FE				✓	✓

Notes: The unit of observation is a country. In Panel (a), the dependent variable is the country's government expenditures as a share of the country's GDP; in Panel (b), the dependent variable is the country's government revenues as a share of the country's GDP. Executive term length is calculated based on presidential terms (presidential countries) or on lower house terms (parliamentary countries). Countries in which executive term lengths are unlimited are excluded. The specifications in column (2) includes geographical controls and world region fixed effects. The specifications in column (3) weights the estimates of column (2) by the country's population. The specifications in column (4) augment the specifications in column (3) with controls for the form of government and the regime. The specifications in column (5) exclude San Marino (which is the only country in which the term for the two executive officers lasts 6 months). Geographical controls include latitude, longitude, and altitude of the capital city. See Appendix B for further information on the dataset utilized for this analysis. Standard errors robust to heteroskedasticity are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

Table A.2: Dates Constitutional Amendments

State	Constitution Amended	Treatment (t)	Last 2-year term election	First 4-year term election	Unsuccessful Attempts
Arizona	1968–11–05	1969	1968	1970	3
Arkansas	1984–11–06	1985	1984	1986	3
Colorado	1956–11–06	1957	1956	1958	2
Connecticut	1948–11–02	1949	1948	1950	0
Georgia	1941–06–03	1942	1940	1942	0
Idaho	1944–11–07	1945	1944	1946	2
Iowa	1972–11–07	1973	1972	1974	0
Kansas	1972–11–07	1973	1972	1974	0
Maine	1957–09–09	1958	1956	1958	0
Massachusetts	1964–11–03	1965	1964	1966	0
Michigan	1963–04–01	1964	1964	1966	0
Minnesota	1958–11–04	1959	1960	1962	0
Nebraska	1962–11–06	1963	1964	1966	0
New Hampshire					1
New Mexico	1970–11–03	1971	1968	1970	1
North Dakota	1964–06–30	1965	1962	1964	2
Ohio	1954–11–02	1955	1956	1958	1
Rhode Island	1992–11–03	1993	1992	1994	2
South Dakota	1972–11–07	1973	1972	1974	1
Tennessee	1953–11–03	1954	1952	1954	0
Texas	1972–11–07	1973	1972	1974	1
Vermont					1
Wisconsin	1967–04–04	1968	1968	1970	1

Notes: New Jersey switched from three-year terms to four-year terms in 1947. South Carolina and New York approved a four-year gubernatorial term on November 2, 1926 and November 2, 1937, respectively. These states are included in the robustness checks performed in Table A.9 and Table A.10, respectively.

Table A.3: Descriptive Statistics: 1937–2008

Variable	Obs.	Mean	St. Dev.	Min	Max
State Total Expenditures / GDP	1,656	0.093	0.034	0.028	0.216
State Current Expenditures / GDP	1,656	0.077	0.032	0.012	0.196
State Capital Expenditures / GDP	1,656	0.013	0.007	0	0.063
State Total Revenues / GDP	1,656	0.094	0.035	0.009	0.206
State Tax Revenues / GDP	1,656	0.054	0.015	0.021	0.106
State Grants Revenues / GDP	1,656	0.025	0.013	0.003	0.072
Population	1,656	3,385,838.164	3,483,944.886	315,000	24,309,000
Share Kids Elderly	1,656	0.327	0.027	0.238	0.506
Adopted Balance Budget Rule	1,656	0.617	0.486	0	1
Adopted General Civil Service Reform	1,656	0.760	0.427	0	1
Adopted Stabilization Funds	1,656	0.320	0.467	0	1
Adopted Item Veto	1,656	0.804	0.397	0	1
Adopted Individual Income Tax	1,656	0.793	0.405	0	1
Adopted Corporate Income Tax	1,656	0.787	0.409	0	1
Adopted General Sale Tax	1,656	0.795	0.404	0	1
Adopted Tax or Expenditure Limits	1,656	0.156	0.363	0	1
No. of term limits	1,656	0.599	0.905	0	3
Share of families reporting radios in 1930	1,656	0.376	0.156	0.091	0.576
Term Limited Governor	1,656	0.147	0.354	0	1
Governor's share of votes	1,656	0.581	0.107	0.354	1
U.S. President's share of votes by state	1,656	0.533	0.101	0.284	0.871
State personal income growth rate	1,656	0.067	0.069	−0.254	0.606
Governor's years in office	1,656	3.893	2.779	1	16
Post Reform	1,656	0.565	0.496	0	1
Post Failed Referendum	1,656	0.412	0.492	0	1
Share of rural population (1920)	1,656	0.585	0.213	0.081	0.864
Share of female population (1920)	1,656	0.485	0.014	0.451	0.509
Share of white population (1920)	1,656	0.936	0.102	0.583	0.998
Share of 0–17 population (1920)	1,656	0.377	0.041	0.319	0.448
No. of families (1920)	1,656	0.232	0.011	0.209	0.246
No. of farms (1920)	1,656	2.754	1.811	0.088	6.301

Notes: The unit of observation is a state-year. The table reports information from all years between 1937 and 2008. States that adopted a four-year term prior to 1937 and New Jersey are excluded.

Table A.4: Descriptive Statistics: Stacked Panel

Variable	Obs.	Mean	St. Dev.	Min	Max
State Total Expenditures / GDP	4,626	0.075	0.028	0.028	0.174
State Current Expenditures / GDP	4,626	0.057	0.021	0.012	0.158
State Capital Expenditures / GDP	4,626	0.016	0.009	0	0.063
State Total Revenues / GDP	4,626	0.075	0.028	0.009	0.177
State Tax Revenues / GDP	4,626	0.046	0.012	0.021	0.087
State Grants Revenues / GDP	4,626	0.019	0.012	0.003	0.061
Population	4,626	2,472,314.397	2,542,910.257	315,000	15,331,000
Share Kids Elderly	4,626	0.335	0.029	0.238	0.401
Adopted Balance Budget Rule	4,626	0.589	0.492	0	1
Adopted General Civil Service Reform	4,626	0.569	0.495	0	1
Adopted Stabilization Funds	4,626	0.015	0.120	0	1
Adopted Item Veto	4,626	0.687	0.464	0	1
Adopted Individual Income Tax	4,626	0.685	0.464	0	1
Adopted Corporate Income Tax	4,626	0.690	0.462	0	1
Adopted General Sale Tax	4,626	0.642	0.480	0	1
Adopted Tax or Expenditure Limits	4,626	0.003	0.055	0	1
No. of term limits	4,626	0.202	0.641	0	3
Share of families reporting radios in 1930	4,626	0.384	0.155	0.091	0.576
Term Limited Governor	4,626	0.034	0.180	0	1
Governor's share of votes	4,626	0.589	0.105	0.358	1
U.S. President's share of votes by state	4,626	0.547	0.096	0.308	0.871
State personal income growth rate	4,626	0.062	0.078	−0.254	0.606
Governor's years in office	4,626	3.066	2.053	1	14
Post Reform	4,626	0.045	0.208	0	1
Post Failed Referendum	4,626	0.309	0.462	0	1
Share of rural population (1920)	4,626	0.584	0.227	0.081	0.864
Share of female population (1920)	4,626	0.485	0.014	0.451	0.509
Share of white population (1920)	4,626	0.946	0.080	0.583	0.998
Share of 0–17 population (1920)	4,626	0.374	0.041	0.319	0.448
No. of families (1920)	4,626	0.232	0.011	0.209	0.246
No. of farms (1920)	4,626	2.574	1.557	0.088	6.301

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). Each stack is constructed to ensure that only states that are treated at time s and states that are not-yet treated by time s are included.

Table A.5: Descriptive Statistics: Individual governors and gubernatorial candidates' characteristics

Panel A. Governors

Variable	Obs	Mean	St. Dev.	Min	Max
Attended College	595	0.941	0.235	0.000	1.000
Completed College	595	0.852	0.355	0.000	1.000
Previous experience in State administration	595	0.237	0.426	0.000	1.000
Missing individual characteristics	595	0.000	0.000	0.000	0.000

Panel B. All candidates

Variable	Obs	Mean	St. Dev.	Min	Max
Governor	1,248	0.477	0.500	0.000	1.000
Attended College	1,059	0.916	0.278	0.000	1.000
Completed College	1,059	0.825	0.380	0.000	1.000
Previous experience in State administration	1,059	0.231	0.422	0.000	1.000
Missing individual characteristics	1,248	0.151	0.359	0.000	1.000

Panel C. Stacked state-year panel

Variable	Obs	Mean	St. Dev.	Min	Max
Attended College (Governor)	3,960	0.932	0.252	0.000	1.000
Completed College (Governor)	3,960	0.837	0.369	0.000	1.000
Previous experience in State administration (Governor)	3,960	0.171	0.377	0.000	1.000
Attended College (Mean all cand.)	3,960	0.910	0.237	0.000	1.000
Completed College (Mean all cand.)	3,960	0.813	0.321	0.000	1.000
Previous experience in State administration (Mean all cand.)	3,960	0.189	0.314	0.000	1.000

Notes: Panel (a): The unit of observation is a governor-term. Panel (b): The unit of observation is a gubernatorial candidate-term. Panel (c): The unit of observation is a state-year-stack. States that adopted a four-year term prior to 1937 and New Jersey are excluded.

Table A.6: Robustness check: No effect of failed attempt to extend term length

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var.:	Total exp. / GDP	Current exp. / GDP	Capital exp. / GDP	Total rev. / GDP	Tax rev. / GDP	Grant rev. / GDP
Post Failed Referendum	−0.002	0.001	−0.002	−0.003	−0.001	−0.001
	(0.003)	(0.003)	(0.002)	(0.003)	(0.001)	(0.001)
Observations	1,820	1,820	1,820	1,820	1,820	1,820
R^2	0.968	0.963	0.954	0.968	0.951	0.976
Mean at baseline	0.0640	0.0490	0.0130	0.0630	0.0420	0.0150
State-Stack FE	✓	✓	✓	✓	✓	✓
Year-Stack FE	✓	✓	✓	✓	✓	✓
Pre-determined covs × Year FE	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the *placebo* reform in the treated state(s). Each stack is constructed to ensure that only states that receive the *placebo* treatment at time s and states that have not yet received the *placebo* treatment or the term length extension by time s are included. In Column (1), the dependent variable is the state total expenditures as a share of the state GDP; in Column (2), the dependent variable is the state current expenditures as a share of the state GDP; in Column (3), the dependent variable is the state capital expenditures as a share of the state GDP; in Column (4), the dependent variable is the state total revenues as a share of the state GDP; in Column (5), the dependent variable is the state tax revenues as a share of the state GDP; in Column (6), the dependent variable is the amount of federal grants received by the state government as a share of the state GDP. The estimated equation is a version of (1) in which the Post Reform$_{i,t,s}$ indicator is replaced by the indicator Post Failed Attempt$_{i,t,s}$. Post Failed Attempt$_{i,t}$ is an indicator equal to 1 in the years after the first attempt to extend the gubernatorial term length that was rejected by voters. All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). Standard errors robust to clustering at the state-presidential term level are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

Table A.7: Robustness check: Callaway and Sant'Anna (2021) estimator

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var.:	Total exp. / GDP	Current exp. / GDP	Capital exp. / GDP	Total rev. / GDP	Tax rev. / GDP	Grant rev. / GDP
Post Reform	−0.012***	−0.012***	0.003**	−0.013***	0.000	−0.006***
	(0.003)	(0.002)	(0.001)	(0.003)	(0.001)	(0.001)
Observations	1,633	1,633	1,633	1,633	1,633	1,633
Mean at baseline	0.0750	0.0590	0.0140	0.0760	0.0460	0.0190
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is a state-year. The table reports information from all years between 1937 and 2008. States that adopted a four-year term prior to 1937 and New Jersey are excluded. In Column (1), the dependent variable is the state total expenditures as a share of the state GDP; in Column (2), the dependent variable is the state current expenditures as a share of the state GDP; in Column (3), the dependent variable is the state capital expenditures as a share of the state GDP; in Column (4), the dependent variable is the state total revenues as a share of the state GDP; in Column (5), the dependent variable is the state tax revenues as a share of the state GDP; in Column (6), the dependent variable is the amount of federal grants received by the state government as a share of the state GDP. The estimated equation is the model proposed by Callaway and Sant'Anna (2021). The estimator automatically exclude the last year of observations (2008). Standard errors robust to clustering at the state-presidential term level are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

Table A.8: Robustness check: Alternative measures of the dependent variables

	(1)	(2)	(3)	(4)
	Per Capita ($1982)	Logs(Per Capita) ($1982)	Share of GDP, Mean Legislature	Share of GDP, Collapsed Biennium
Panel A. Dep. var.: Total expenditures				
Post Reform	−29.943**	−0.054***	−0.004***	−0.004**
	(15.193)	(0.018)	(0.002)	(0.002)
Observations	4,620	4,620	4,620	1,992
R^2	0.986	0.987	0.971	0.972
Mean at baseline	141.1	141.1	141.1	0.0730
Panel B. Dep. var.: Total revenues				
Post Reform	−18.893	−0.035***	−0.003**	−0.003*
	(12.844)	(0.014)	(0.001)	(0.001)
Observations	4,620	4,620	4,620	1,992
R^2	0.989	0.985	0.976	0.977
Mean at baseline	141.1	141.1	141.1	0.0730
State-Stack FE	✓	✓	✓	✓
Year-Stack FE	✓	✓	✓	✓
Pre-determined covs × Year FE	✓	✓	✓	✓

Notes: In columns (1)–(3), the unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). In column (4), the unit of observation is a state-biennium-stack. We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. In Panel (a), the dependent variable measures the level of state total expenditures; in Panel (b), the dependent variable measures the level of state total revenues. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). In Column (1), the dependent variable is measured in 1982 US dollars per capita; in Column (2), the dependent variable is measured as the natural logarithm of 1982 US dollars per capita; in Column (3), the dependent variable is measured as a share of the state GDP, averaged across the full term; in Column (4), the dependent variable is the measured as a share of the state GDP, averaged and collapsed across each biennium. In Column (2), 1920 density of farms, 1920 household size, and 1920 manufacturing plants are transformed into natural logarithms for consistency. Standard errors robust to clustering at the state-presidential term level are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

Table A.9: Robustness check: New Jersey in the sample

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var.:	Total exp. / GDP	Current exp. / GDP	Capital exp. / GDP	Total rev. / GDP	Tax rev. / GDP	Grant rev. / GDP
Post Reform	−0.004***	−0.003***	−0.001	−0.003**	−0.000	−0.002***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	4,658	4,658	4,658	4,658	4,658	4,658
R^2	0.964	0.971	0.918	0.972	0.955	0.965
Mean at baseline	0.0740	0.0560	0.0160	0.0740	0.0460	0.0190
State-Stack FE	✓	✓	✓	✓	✓	✓
Year-Stack FE	✓	✓	✓	✓	✓	✓
Pre-determined covs × Year FE	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is a state-year-stack. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time s and states that are not-yet-treated by time s are included. For the sake of this exercise, the state of New Jersey, which expanded its gubernatorial term from three years to four years in 1947, is included in the stacked data as a treated state. In Column (1), the dependent variable is the state total expenditures as a share of the state GDP; in Column (2), the dependent variable is the state current expenditures as a share of the state GDP; in Column (3), the dependent variable is the state capital expenditures as a share of the state GDP; in Column (4), the dependent variable is the state total revenues as a share of the state GDP; in Column (5), the dependent variable is the state tax revenues as a share of the state GDP; in Column (6), the dependent variable is the amount of federal grants received by the state government as a share of the state GDP. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). Standard errors robust to clustering at the state-presidential term level are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

Table A.10: Robustness check: Extended sample from 1922 to 2008

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var.:	Total exp.	Current exp.	Capital exp.	Total rev.	Tax rev.	Grant rev.
Post Reform	−25.511*	−17.438*	−4.874	−13.355	4.504	−14.364***
	(13.310)	(9.726)	(6.657)	(11.366)	(7.718)	(5.216)
Observations	5,636	5,636	5,636	5,636	5,339	5,188
R^2	0.988	0.991	0.939	0.991	0.985	0.983
Mean at baseline	473.2	361.5	100.8	471.4	299.4	131.6
State-Stack FE	✓	✓	✓	✓	✓	✓
Year-Stack FE	✓	✓	✓	✓	✓	✓
Pre-determ covs × Year FE	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is a state-year-stack. Each stack is formed utilizing yearly data from 1922 to 2008. Due to data limitations, figures that refer to the years between 1932 and 1936 are linear interpolations of the values measured in 1931 and in 1937. Each stack includes data contained within a time window of -10/+10 years since the reform in the treated state(s). We construct each stack to ensure that only states treated at time *s* and states that are not-yet-treated by time *s* are included. For the sake of this exercise, the states of South Carolina and New York, which expanded their gubernatorial term from two years to four years between 1922 and 1936, are included in the stacked data as a treated state. In Column (1), the dependent variable is the state total expenditures in per capita 1982 US dollars; in Column (2), the dependent variable is the state current expenditures in per capita 1982 US dollars; in Column (3), the dependent variable is the state capital expenditures in per capita 1982 US dollars; in Column (4), the dependent variable is the state total revenues in per capita 1982 US dollars; in Column (5), the dependent variable is the state tax revenues in per capita 1982 US dollars; in Column (6), the dependent variable is the amount of federal grants received by the state government in per capita 1982 US dollars. The estimated equation is (1). All specifications include the interaction between year dummies and % rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile, all measured at baseline (1920 census). Standard errors robust to clustering at the state-presidential term level are in parentheses. *,**,*** represent the 10%, 5%, and 1% significance levels, respectively.

B Data Appendix

B.1 Cross-country correlation dataset

Executive term length. Our source of data is Wikipedia, which provides information on the president's and of the lower house's term length for 197 countries.[46] We create our variable *Executive term length* to be equal to the length (measured in years) of the presidential term in presidential countries, and to the length of the lower house in parliamentary countries. We rely on Wikipedia also to identify presidential and parliamentary countries.[47]

Government spending and revenue over GDP. We constructed a cross-section of 164 countries for which the executive term length is not unlimited by combining data from the International Monetary Fund (IMF) and data from the World Bank on government spending as a share of the country's GDP and of government revenues (excluding grants) as a share of the country's GDP. Specifically, we start from data from the World Bank and, for each country, we select the most recent available data as of June, 2024. The World Bank provide spending information for 145 countries and revenue information for 147 countries. We then add to the dataset information, using the most recent available data from the IMF for the missing countries.

Control variables. Data on country population, income class, and world region are from the World Bank, while data on the latitude, longitude, and altitude of the capital city are from Omar Nomar's Github platform[48], and data on the government regime are from the VDEM country-year panel. For all countries, data refer to the most recent available information as of June, 2024. The VDEM country-year panel does not cover 21 of the countries in our sample. The *Regime* variable takes a specific value for these countries.

B.2 Data sources for the main analysis

State government finances. Data on state government finances are from the US Census Bureau. More specifically, data covering the period 1922–1950 are collected from the *The Statistical Abstract of the United States* (SAUS) and/or the *Financial Statistics of States* (FSS), while data covering the period 1951–2008 are collected from the *Data Base on Historical Finances of State Governments*.

The main data source for the period 1922–1950 is SAUS, with the following exceptions. First, SAUS reports information about the total spending and revenue items of state

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[46]https://en.wikipedia.org/wiki/Term_of_office, accessed in June, 2024.

[47]https://en.wikipedia.org/wiki/List_of_countries_by_system_of_government, accessed in June, 2024.

[48]https://gist.github.com/ofou/df09a6834a8421b4f376c875194915c9, accessed in June, 2024.

governments (i.e., including state enterprises and insurance trusts) until 1932, while it reports information about the total spending and revenue items of the general government (i.e., excluding state enterprises and insurance trusts) afterwards. To ensure that our panel consistently identifies the same budget items over time – spending and revenue items of the general government – we complement SAUS data with FSS data in order to partial out state enterprises and insurance trusts in the period 1922–1932. Second, SAUS was not published or did not include tables on state government finances for the years 1930, 1939, and 1941. For these years, we exclusively rely on FSS. Third, neither SAUS nor FSS cover the years 1933–1936 for all states and the year 1932 for Arizona, Georgia, Idaho, Indiana, Louisiana, Massachusetts, Maryland, Missouri, Mississippi, New Hampshire, Oklahoma, Texas, and Wyoming.

Our data sources report data that refer to a fiscal year whose end date belongs to the specified calendar year. For example, data for 1950 report information for all states related to the fiscal year ended at some point during 1950. Some states in our sample (Alabama, Maryland, Massachusetts, Missouri, New York, Ohio, Texas, and Wyoming) modified the structure of fiscal years at some point during our sample, thus altering the timing of budget reporting in our primary sources. This change results in one missing observation for each affected state in the year of transition. To address this data gap, we interpolate the missing values using a linear interpolation method.

State personal income and population. To compute state government finance data as a share of GDP, we first divide the total amounts by the state population, using yearly population data from the Bureau of Economic Analysis (BEA). We then divide the per-capita value by the per-capita state personal income, also collected from the BEA, which serves as our proxy for the per-capita state GDP. We use state personal income data from BEA also to identify periods of low/high economic growth. Population and state income data from BEA are available since 1929. For the years prior to 1929, used for the analysis performed in Table A.10, we calculate the state population dividing SAUS data for the total state government spending with its per-capita value, available in the original source.

Additional data sources The data on the proportion of children and elderly are collected from the decennial censuses for the years prior to 1965 and from SAUS for 1965 and onwards, while US-wide data on the consumer price index (all urban consumers), that we use to calculate our dependent variable in real terms (1982 dollars) are collected from the Bureau of Labour Statistics.

The data on gubernatorial elections and on the term limit discipline for governors comes from four sources: i) the database *Candidate Name and Constituency Totals, 1788-1990* (ICPSR), ii) the National Governors Association's (NGA) former governor biograph-

ical webpages, iii) Dave Leip's Atlas of US Presidential elections, and iv) Wikipedia.[49]

Information on approved and failed constitutional amendments to extend the term length are retrieved from Ballotpedia.

Data on the share of families using the radio in 1930 are collected from the 1930 Census, while data used to control for time-unvarying state characteristics in 1920 (% rural population, % female population, % white population, % population aged 0–17 years, mean household size, and number of farms per square mile) are collected from the 1920 Census.

Date on other relevant institutional reforms are collected from multiple sources: data on the adoption of balance-budget requirements are from Table 2 in Smith and Hou (2013)[50]; data on the adoption of the civil service reforms are from Table 1 in Ting et al. (2013); data on the adoption of state budget stabilization funds are from Table 1 in Wagner (2003); data on the adoption of the gubernatorial veto power are retrieved from the website of the National Conference of State Legislatures[51]; data on the adoption of the individual income tax, corporate income tax and general sale tax are from Table 3 in Garrett and Wagner (2004); data on the adoption of tax and expenditure limits are from Table 1 in New (2010)[52].

Data on US presidential elections results by state are collected from Psephos - Adam Carr's Election Archive for the years 1920-1972 and from the MIT Election and Science Lab for the years 1976-2008.

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[49]Wikipedia lists of state governors of the United States, accessed in June, 2024.

[50]We consider the first in time type of adoption, either a Constitutional or Statutory. We also consider a balanced-budget requirement to be in place only when "The legislature must pass a balanced budget", i.e. only when requirement 4 from Table 1 in Smith and Hou (2013) is adopted.

[51]National Conference of State Legislatures, accessed in June, 2024.

[52]We consider the first in time type of adoption.

C   Constitutional amendments concurrent to term length reforms

Table C.1: List of all constitutional amendments approved by voters at the same time of the term length extension

State	Y-M-D	Title	Description
AR	1984-11-06	Proposed Amendment 62	Allows local governments to issue bonds.
AR	**1984-11-06**	**Proposed Amendment 64**	**Creates four-year term limits for state constitutional officers**
AZ	1968-11-05	Veterans Tax Exemption Amendment	Would exempt veterans from certain tax exemptions.
AZ	1968-11-05	Tax Exemption Claim Amendment	Would make false claims for tax exemptions punishable by law.
AZ	1968-11-05	Household Goods Amendment	Would exempt certain households goods from property taxation.
AZ	1968-11-05	License Tax Amendment	Would implement a license tax on registered vehicles.
AZ	**1968-11-05**	**Term Limit Amendment**	**Would implement four year terms for the Governor, Secretary of State, State Auditor.**
AZ	1968-11-05	Insurer Control Amendment	Remove the control of domestic and foreign insurers from the commission.
AZ	1968-11-05	State Examiner Repeal Amendment	Repeal the office of State Examiner.
AZ	1968-11-05	State Auditor Repeal Amendment	Repeal the office of State Auditor.
AZ	1968-11-05	Travel Expense Amendment	Relating to travel expense reimbursements for state legislators.
AZ	1968-11-05	Legislative Districts	Revises legislative districts.
AZ	1968-11-05	County Boards of Supervisors Question	Would create county boards of supervisors, set standards depending on population.
CO	**1956-11-06**	**Measure 1**	**Provided for four-year long terms for executive state officers**
CO	1956-11-06	Measure 2	Exempts from taxation household furnishings and personal effects
CO	1956-11-06	Measure 5	Establishes a monthly allocation of $100 to old age pension recipients, adjusts the pension amount to increased living costs and provides for a stabilization fund and medical fund
CT	1948-11-02	Question 1	Concerns the terms of office for probate judges.
CT	**1948-11-02**	**Question 2**	**Concerns election procedure and terms of office for state executive officials.**
CT	1948-11-02	Question 3	Concerns the forfeiture of electoral privileges upon conviction.
CT	1948-11-02	Question 4	Concerns judicial nomination and appointment procedures.
CT	1948-11-02	Question 5	Concerns the strengthening of the United Nations charter.
GA	**1941-06-03**	**Amendment 1**	**Provides for four year terms for the governor and other constitutional officers.**
GA	1941-06-03	Amendment 2	Provides for gubernatorial elections every four years.
GA	1941-06-03	Amendment 3	Authorizes Hart County to issue funding bonds.
GA	1941-06-03	Amendment 4	Authorizes Toombs County to issue funding bonds.
GA	1941-06-03	Amendment 5	Authorizes Paulding County to issue funding bonds.
GA	1941-06-03	Amendment 6	Authorizes Jeff Davis County to issue funding bonds.
GA	1941-06-03	Amendment 7	Authorizes Oglethorpe County to issue funding bonds.
GA	1941-06-03	Amendment 8	Authorizes Dodge County to issue funding bonds.
GA	1941-06-03	Amendment 9	Authorizes Wilcox County to issue funding bonds.
GA	1941-06-03	Amendment 10	Authorizes the city of Doerun to issue refunding bonds.
GA	1941-06-03	Amendment 11	Authorizes Miller County to issue warrant funding bonds.
GA	1941-06-03	Amendment 12	Authorizes the Jeff Davis County Excelsior Consolidated School to issue refunding bonds.
GA	1941-06-03	Amendment 13	Authorizes the Wilcox County Abbeville Consolidated School to issue refunding bonds.
GA	1941-06-03	Amendment 14	Authorizes the city of Crawford to issue refunding bonds.
GA	1941-06-03	Amendment 15	Authorizes Cobb County to issue funding bonds.
GA	1941-06-03	Amendment 16	Authorizes the city of Claxton to issue refunding bonds.
GA	1941-06-03	Amendment 17	Exempts new buildings from ad valorem taxes in the city of Macon for five years.

GA	1941-06-03	Amendment 18	Authorizes the city of Waycross to incur indebtedness.
GA	1941-06-03	Amendment 19	Authorizes Irwin County to issue funding bonds.
GA	1941-06-03	Amendment 20	Increases the jurisdiction of justice courts.
GA	1941-06-03	Amendment 21	Relates to contracts with counties, municipalities and corporations for hospitals.
GA	1941-06-03	Amendment 22	Authorizes the Wrightsville Consolidated School to issue refunding bonds.
GA	1941-06-03	Amendment 23	Authorizes Hancock County to issue refunding bonds.
GA	1941-06-03	Amendment 24	Authorizes Cook County to issue funding bonds.
GA	1941-06-03	Amendment 25	Authorizes the city of Macon to incur debt for defense.
GA	1941-06-03	Amendment 26	Authorizes the Mitchell County Board of Education to make loans.
GA	1941-06-03	Amendment 27	Authorizes the city of Macon to levy taxes for future territory.
GA	1941-06-03	Amendment 28	Authorizes the city of Vidalia to issue refunding bonds.
GA	1941-06-03	Amendment 29	Authorizes the city of Hazlehurst to issue refunding bonds.
GA	1941-06-03	Amendment 30	Authorizes Oglethorpe County Crawford School to issue refunding bonds.
GA	1941-06-03	Amendment 31	Authorizes Irwin County to issue funding bonds.
GA	1941-06-03	Amendment 32	Authorizes the Washington County Davisboro Consolidated School to issue refunding bonds.
GA	1941-06-03	Amendment 33	Authorizes Bibb County to make temporary loans.
GA	1941-06-03	Amendment 34	Authorizes the city of Gainesville to issue refunding bonds.
GA	1941-06-03	Amendment 35	Authorizes the Adrian 1st Consolidated School to issue bonds.
GA	1941-06-03	Amendment 36	Authorizes the city of Reidsville to issue refunding bonds.
GA	1941-06-03	Amendment 37	Authorizes the city of Abbeville to issue refunding bonds.
GA	1941-06-03	Amendment 38	Authorizes Wilcox County to incur bonded indebtedness.
GA	1941-06-03	Amendment 39	Authorizes Dade County to issue refunding bonds.
GA	1941-06-03	Amendment 40	Authorizes the city of Cordele to issue refunding bonds.
GA	1941-06-03	Amendment 41	Authorizes the city of Lexington to issue refunding bonds.
GA	1941-06-03	Amendment 42	Authorizes Chatham County to levy a school tax.
GA	1941-06-03	Amendment 43	Authorizes the Franklin County Sandy Springs School to issue refunding bonds.
GA	1941-06-03	Amendment 44	Authorizes Cook County to issue refunding bonds.
GA	1941-06-03	Amendment 45	Authorizes Quitman County to issue funding bonds.
GA	1941-06-03	Amendment 46	Authorizes Miller County to issue warrant funding bonds.
GA	1941-06-03	Amendment 47	Authorizes the Gwinnett County Sunny Hill Consolidated School to issue bonds.
GA	1941-06-03	Amendment 48	Authorizes the Toombs County Johnson Corder School to issue refunding bonds.
GA	1941-06-03	Amendment 49	Authorizes Evans County to issue funding bonds.
GA	1941-06-03	Amendment 50	Authorizes the city of Stone Mountain to issue refunding bonds.
GA	1941-06-03	Amendment 51	Authorizes Walker County to issue funding bonds.
GA	1941-06-03	Amendment 52	Relates to sewer, water and fire prevention for DeKalb County.
GA	1941-06-03	Amendment 53	Authorizes Jefferson County School District No. 10 to issue refunding bonds.
GA	1941-06-03	Amendment 54	Exempts rural electric corporations from taxes.
GA	1941-06-03	Amendment 55	Fixes the term of office for the state school superintendent.
GA	1941-06-03	Amendment 56	Authorizes the Cook County Sparks-Adel School to issue refunding bonds.
GA	1941-06-03	Amendment 57	Authorizes the Evans County Claxton Consolidated School to issue refunding bonds.
GA	1941-06-03	Amendment 58	Authorizes Effingham County to issue retirement bonds.
GA	1941-06-03	Amendment 59	Authorizes Chattooga County to issue funding bonds.
GA	1941-06-03	Amendment 60	Authorizes Jefferson County School District No. 1 to issue refunding bonds.
GA	1941-06-03	Amendment 61	Authorizes Baker County to issue retirement bonds.
GA	1941-06-03	Amendment 62	Authorizes the city of Cochran to issue refunding bonds.
GA	1941-06-03	Amendment 63	Authorizes the city of Unadilla to issue refunding bonds.
GA	1941-06-03	Amendment 64	Authorizes Calhoun County to issue funding bonds.
GA	1941-06-03	Amendment 65	Relates to the payment of $50,000 by the city of Savannah to the National Gypsum Company.
GA	1941-06-03	Amendment 66	Authorizes the city of Washington to issue refunding bonds.

GA	1941-06-03	Amendment 67	Authorizes Catoosa County to issue funding bonds.
GA	1941-06-03	Amendment 68	Relates to advertising for the state of Georgia.
GA	1941-06-03	Amendment 70	Authorizes Bibb County to issue debt certificates for defense purposes.
IA	1972-11-07	Amendment 1	Allows for the retirement of judges for disability and to discipline or remove them for good cause.
IA	**1972-11-07**	**Amendment 2**	**Establishes four-year terms for certain state executive officials.**
IA	1972-11-07	Amendment 3	Repeals the prohibition against lotteries and the sale of lottery tickets.
ID	**1944-11-07**	**SJR 1**	**Sets four year terms for state officers.**
ID	1944-11-07	SJR 3	Replaces the State Board of Equalization with a State Tax Commission.
ID	1944-11-07	SJR 4	Exempts federal government property from taxation.
KS	1970-11-03	Amendment 2	Proposes to outline the process of amending the constitution.
KS	**1970-11-03**	**Amendment 3**	**Proposes to outline the power and guidelines governing the state executive branch.**
MA	**1964-11-03**	**Question 1**	**Mandates four year terms for constitutional officers.**
MA	1964-11-03	Question 2	Provides procedures for filling a vacant office in the event of an enemy attack.
MA	1964-11-03	Question 3	Authorizes the disbursement of state credit upon a two-thirds vote in each branch of the legislature.
MA	1964-11-03	Question 4	Requires the governor to seek opinions of justices of the Supreme Judicial Court on questions of law.
MA	1964-11-03	Question 5	eliminated the statutory authority of the governor's executive council.
ME	1957-09-09	Referendum Question No. 1	Issues $24 million in bonds for the construction of state highways.
ME	1957-09-09	Referendum Question No. 2	Issues $2.5 million in bonds for ferry services to North Haven, Vinalhaven, Islesboro and Swan's Island. Y
ME	1957-09-09	Proposed Constitutional Amendment No. 1	Provides a $20 million bond issue for guaranteed loans for industrial purposes.
ME	1957-09-09	Proposed Constitutional Amendment No. 2	Moves general election date to Tuesday after the first Monday in November.
ME	**1957-09-09**	**Proposed Constitutional Amendment No. 3**	**Creates four-year term for Governor; prohibits more than two consecutive terms.**
MI	**1963-04-01**		**FOR THE 1963 CONSTITUTION.**
MN	1958-11-04	Amendment 1	Permits the legislature to revise provisions for home rule.
MN	**1958-11-04**	**Amendment 2**	**Provides for four year terms for state constitutional officers.**
ND	1964-06-30	Referendum 1	Allows separate budgets and appropriation measures for some agricultural projects.
ND	1964-06-30	Referendum 3	Renames the office of Police Magistrate to Municipal Judge.
ND	**1964-06-30**	**Initiative 5**	**Lengthens terms from two years to four for some state and county offices.**
ND	1964-11-03	Referendum 1	Abolishes voter pamphlet publication requirement.
ND	1964-11-03	Initiative 3	Repeals state statutes requiring certain number of crew members on freight trains and self-propelled equipment.
ND	1964-11-03	Initiative 6	Allows alcohol sales in some food serving establishment and restricts age of entrance for in such places.
NE	1962-11-02	Amendment 1	Authorizes lotteries and gift enterprises.
NE	1962-11-02	Amendment 2	Provides for district elections for the state railway commission.
NE	1962-11-02	Amendment 3	Relates to the candidacy of the lieutenant governor for office of the governor.
NE	**1962-11-02**	**Amendment 4**	**Provides four year term limits for the governor and lieutenant governor.**
NE	1962-11-02	Amendment 5	Provides four year term limits for legislators.
NE	1962-11-02	Amendment 6	Relates to the appointment of judges.
NE	1962-11-02	Amendment 7	Relates to the reapportionment of legislative districts.
NM	**1970-11-03**	**CA 3**	**Proposing an amendment to Article 5, Section 1 of the constitution of New Mexico pertaining to State Executive Officers and their terms of office.**
NM	1970-11-03	CA 1	The referendum proposed amending the constitution to provide for the Municipal Home Rule.
NM	1970-11-03	CA 2	Proposing to amend the constitution of New Mexico by repealing Article 7 and adopting a new Article 7 pertaining to the Elective Franchise.
OH	**1954-11-02**	**Amendment 1**	**Sets four year terms for state executive officials.**
RI	**1992-11-03**	**Proposal 1**	**Provisions to recall statewide elected officials.**
RI	1992-11-03	Proposal 2	Limits state revenue appropriations and establishes a budget reserve account.
RI	1992-11-03	Proposal 3	Permits the Governor of Rhode Island to serve while physically absent from the state.
RI	1992-11-03	Proposal 4	Issues $12 million in general obligation bonds.
RI	1992-11-03	Proposal 5	Issues $2.5 million in general obligation bonds.
RI	1992-11-03	Proposal 6	Issues $9.5 million in general obligation bonds.
RI	1992-11-03	Proposal 7	Issues $4.5 million in general obligation bonds.

RI	1992-11-03	Proposal 8	Issues $10 million in general obligation bonds.
RI	1992-11-03	Proposal 9	Issues $15 million in general obligation bonds.
SD	1972-11-07	Amendment A	Relates to the voting age.
SD	**1972-11-07**	**Amendment B**	**Reorganizes the executive branch.**
SD	1972-11-07	Amendment C	Reorganizes the judicial branch.
SD	1972-11-07	Amendment D	Reorganizes local governments.
SD	1972-11-07	Amendment E	Revises the constitution.
TN	**1953-11-03**		**Four year terms for the governor.**
TN	1953-11-03		Repeal of the poll tax.
TN	1953-11-03		Authorization of consolidated city-county (or "metropolitan") governments.
TN	1953-11-03		Governor gets line-item veto on appropriations
TN	1953-11-03		Raise the pay to legislators from $5 to $15 a day.
TN	1953-11-03		Allows the governor up to ten days to sign bills reaching him or her in the last five days of the Legislature is adjourned.
TN	1953-11-03		The governor must explain his or her refusal to sign any bill reaching him or her during the last five days of the legislative session.
TN	1953-11-03		Requires amendments to be supported by a majority of voters in the gubernatorial elections, rather than in the state representatives election.
TX	1972-11-07	Proposition 2	Abolishes the Lamar County Hospital District.
TX	1972-11-07	Proposition 3	Requires that all justices of the peace be compensated on a salary basis.
TX	1972-11-07	Proposition 4	Provides for a constitutional revision commission as a constitutional convention in January.
TX	1972-11-07	Proposition 5	Allows certain tax exemptions to disabled veterans and surviving spouses and surviving minor children of those killed on active duty.
TX	1972-11-07	Proposition 6	Exempts not less than $3,000 of the value of residence homesteads of all persons 65 and older from ad valorem taxes.
TX	1972-11-07	Proposition 7	Provides that equality under the law shall not be denied or abridged because of sex, race, color, creed or national origin.
TX	**1972-11-07**	**Proposition 8**	**Provides a four-year term of office for state executive officials.**
TX	1972-11-07	Proposition 10	Revises provisions on the time and method of proposing amendments to the state constitution.
TX	1972-11-07	Proposition 12	Permits state employees to serve as members of the governing bodies of local governmental districts.
TX	1972-11-07	Proposition 13	Sets a six percent weighted average annual interest rate for bonds.
TX	1972-11-07	Proposition 14	Allows counties to reduce the county permanent school fund.
WI	**1967-04-04**	**Question 1**	**Sets four-year term of office for Governor and Lieutenant Governor.**
WI	1967-04-04	Question 2	Sets four-year term of office for Secretary of State.
WI	1967-04-04	Question 3	Sets four-year term of office for State Treasurer.
WI	1967-04-04	Question 4	Sets four-year term of office for Attorney General.
WI	1967-04-04	Question 5	Governor and Lieutenant Governor are elected jointly.
WI	1967-04-04	Question 6	Sheriffs are no longer restricted to two consecutive terms of office.
WI	1967-04-04	Question 7	Permits legislature to provide use of public transportation to students of private schools.
WI	1967-04-04	Question 8	Permits legislature to equalize judicial salaries.