Title
Electoral cycles in government employment: Evidence from US gubernatorial elections

Author
Cahan, Dodge

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Dodge Cahan†

Abstract

Elections create incentives for politicians to manipulate policy to improve their re-election chances, and employment conditions are an important consideration for many voters. Politicians may opportunistically design policies that increase employment before elections, or postpone cuts until afterwards. I investigate electoral cycles in public sector employment around US gubernatorial elections. Taking advantage of the staggered nature of gubernatorial elections across states, I use both fixed effects models and a geographic discontinuity design that compares neighboring counties at the borders of states with different election cycles. In the period leading up to November each year, growth in both local and state government employment is higher in counties in states that experience gubernatorial elections compared to counties in states that do not, by up to half a percentage point or more. In the post-electoral period, employment growth is lower by comparable magnitudes in counties that just experienced an election. Both of these findings are consistent with manipulation in government employment. I also study heterogeneity across different institutional and political environments, private sector employment, Senate elections, and show that the results hold up to a range of robustness and placebo tests.

1 Introduction

Political office is highly sought after, and politicians may go to substantial lengths to use the political resources at their disposal to their or their party's electoral advantage. This has led to the prediction of various "opportunistic political cycles" in a range of policy variables and outcomes. Such actions that are not necessarily motivated by the best interests of society have come to be referred to broadly as "manipulation" (Nordhaus, 1975). Since economic conditions play an important role when voters form their

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†Department of Economics, University of California, San Diego (dcahan@ucsd.edu).

opinions of politicians (Fair, 1981; Erikson, 1989), the period leading up to an election may produce particularly strong incentives for incumbents to create the impression of a strong economy, signaling ability (Rogoff and Sibert, 1988) or avoiding bad news at the wrong time (Akhmedov and Zhuravskaya, 2004).

One key component of general economic well being that politicians may seek to influence is employment. I investigate electoral cycles in public sector employment in the context of US gubernatorial elections. Governors and their party allies may have the ability to raise employment levels leading up to elections, or delay employment reducing decisions until afterwards. This could happen through the allocation of public funds in the state budget (Blais and Nadeau, 1992; Galli and Rossi, 2002; Akhmedov and Zhuravskaya, 2004), through the timing of employment policies, large projects and procurement contracts (Mechtel and Potrafke, 2013; Garmann, 2017), by favoring employment friendly policies such as lower business taxes during election periods (Foremny and Riedel, 2014), and so on.

Anecdotal evidence of manipulation by governors abounds. During his re-election campaign in 2014, Connecticut governor Dannel Malloy dismissed nonpartisan reports of a growing budget deficit only to, two weeks after winning re-election, announce a statewide freeze on hiring for all positions not "essential for critical agency operations" and on state contracting. Republican lawmakers accused Malloy of misleadingly delaying the bad news until after the election. As House Republican leader Larry Cafero put it: "The governor recognizes that the election's over. . . not more than a week in, it's like, 'Forget what I said on the campaign trail.'"[1]

In the 2014-2015 Florida budget, taking effect four months prior to the election, incumbent Rick Scott vetoed \$8 million out of \$280 million in legislator-approved "supplemental funding" (informally known as "sprinkle lists") aimed at providing additional or first time funding for various projects and programs. The following year, now reelected, he vetoed a much larger \$145 million out of \$301 million. Similarly in the overall budget, in 2015 Scott vetoed \$461 million out of \$78 billion, versus only \$69 million out of \$77 billion in the election year budget. Victims of the cuts included \$1.5 million in funding that was cut off for a storm risk center at Florida State University, and \$15 million for a new campus of the University of Central Florida in downtown Orlando. "There is just no consistency this year," said Senate Majority Leader Bill Galvano.[2]

I provide empirical evidence that electoral cycles, in both local and state government employment, around US gubernatorial elections are more than an anecdotal phenomenon, and their nature is consistent with the manipulation hypothesis. Gubernato-

[1] For an account, see http://www.courant.com/politics/hc-malloy-hiring-freeze-state-agencies-20141113-story.html.

[2] See http://www.politico.com/story/2015/06/rick-scott-vetoes-include-election-year-projects-119443 or http://www.tampabay.com/news/politics/stateroundup/gov-rick-scott-signs-state-budget-in-private-with-little-notice/2234704.

rial elections in the US offer a number of empirical advantages – they are staggered and follow fixed schedules, which allows me to exploit differences in election cycles across states. I use both traditional fixed effects specifications and a geographic discontinuity design that compares counties at state borders where, on one side of the border there is a gubernatorial election taking place, while on the other side there is not, and allows to control for the prevailing economic conditions at a very local level.

In the period leading up to November, growth in local government employment per capita is up to 0.3-0.5 percentage points higher in counties that have an election compared to those that do not. This corresponds to a change in employees that is about 16-25 employees larger for the average county, which, given the average number of counties per state is about 63, is a nontrivial number. In the two quarters following the election, on the other hand, local government employment growth is generally lower by similar magnitudes in those counties that have just experienced an election compared to those that did not. This is precisely what we might expect to observe if incumbent governors attempted to increase government employment leading up to an election or delay any unpopular employment reducing decisions until after the election. A similar pattern is observed for growth in state government employment, which is over half a percentage point higher in counties experiencing an election in the pre-electoral period, and up to 0.8 percentage points lower in the post-electoral period.

These effects are absent in private sector employment, as we might expect since the influence of the governor over private sector employment is less direct and, hence, manipulation is likely less feasible. I also examine whether the effects are different in close elections, when governors are term limited, in states with strict balanced budget rules, in presidential election years compared to other years, when the incumbent governor and the legislature are aligned, and when there is a change in the party in power. I show that elections to the US Senate, which are also staggered and can be studied using a similar strategy, do not appear to affect employment. This is consistent with the federal nature of a senator's work, with less direct power to influence state policy. Finally, I show that the results are robust to a number of placebo tests, alternative specifications, and other robustness checks.

Despite the importance of US gubernatorial elections and their natural suitability for study, the link between gubernatorial elections and government employment dynamics has been largely, surprisingly, overlooked by the literature. Besley and Case (2003) conduct a cursory analysis, but do not find effects of gubernatorial elections on unemployment or income figures. Levitt (1997) shows that police employment in 59 large cities is higher during mayoral and gubernatorial election years, and Bee and Moulton (2015) find some evidence that municipalities experience higher local government employment growth during mayoral election years.[3]

[3] Other studies in the US focusing on a variety of non-employment variables include Reynolds (2014), who studies tuition levels in public institutions, and Rose (2006), who examines how business cycles in

Because of mixed early evidence for cycles in developed economies, in particular the US, there was a shift in attention to developing countries, where political cycles are believed to be more prevalent (Shi and Svensson, 2006). Notable studies include Akhmedov and Zhuravskaya (2004), who investigate Russian budget cycles, and Labonne (2016), perhaps the closest to my paper in terms of the manipulation-related hypotheses investigated. He finds evidence of employment cycles in Phillipenne municipalities – employment is higher leading up to elections and lower afterwards, and the effect is stronger in the private rather than the public sector (I find the opposite in the US).

In studying the US, on the other hand, I contribute to a small but growing literature that revisits developed economies using new methods and better data, showing the persistence of electoral cycles despite economic and institutional maturity. The most closely related are those that investigate employment outcomes: in Greek municipalities there is a pre-election increase in contract employees; there is higher public sector employment in Swedish and Finnish municipalities; in Germany, the timing of hiring new public school teachers is influenced by electoral motives, and active labor market policies aimed at reducing unemployment are pushed in the period leading up to an election (Chortareas et al., 2016; Dahlberg and Mörk, 2011; Tepe and Vanhuysse, 2009; Mechtel and Potrafke, 2013).[4]

Unlike most studies of electoral cycles, especially in the US, I find cycles in a real economic outcome, public sector employment, rather than a policy instrument. The idea that direct policy instruments are more easily manipulable than real economic outcomes led researchers early on to deem it "more promising to focus empirical research for electoral cycles on taxes, transfers and government consumption" (Rogoff, 1990: 33) and most research since has followed this line of reasoning. Studying outcomes rather than policy instruments may be desirable in cases where the outcome can be manipulated through multiple diverse channels simultaneously or selectively depending on the circumstances (Dannel Malloy's alleged manipulation was through timing the use of his executive authority over state agencies, while Rick Scott's was through restrained use of his veto well before the election), or when cycles in policy variables that do not affect outcomes may in the end be of less consequence.

Many of the studies mentioned use annual data, Akhmedov and Zhuravskaya (2004) and Labonne (2016) being notable exceptions, which may obfuscate more short term effects such as differential outcomes before and after elections. My use of quaterly data from the Quarterly Census of Employment and Wages (QCEW) allows me to examine short time horizons with data that are broad in geographic coverage, span a 25 year period, and allow to disaggregate to local government, state government, and private sector employment, highlighting important differences. Moreover, most researchers

fiscal variables depend on balanced budget rules.

[4]In German states or municipalities, Foremny and Riedel (2014) and Garmann (2017) study non-employment variables including business tax rates and issuing of building licenses; Aidt et al. (2011) study various fiscal variables in Portuguese municipalities.

have used more traditional panel models. In recent years, estimation strategies taking advantage of discontinuities at political and administrative boundaries have become quite popular[5] but, to my knowledge, have not previously been applied to the study of political cycles. In many contexts, this may prove to be a source of clean identification for future electoral cycles researchers.

Finally, in 2016 there were over 19 million state and local government employees in the US (Bureau of Labor Statistics), about 13% of the total, and artificial volatility induced in the economy by electoral manipulation could have substantial welfare implications (Bee and Moulton, 2016). Abrupt deviations from optimal hiring trajectories, regardless of what one believes to be optimal, may hinder consumption smoothing and increase the level of uncertainty in the economy.

2 Research design

2.1 Institutional setting

The US state governments provide an ideal setting, a 'policy laboratory', in which to study the effect of certain government institutions on economic outcomes. In particular, each state's executive branch is headed by a governor, whose powers generally include appointing officials and judges, drafting budgets, making legislative proposals, and vetoing state legislature bills. These powers result in governors having significant influence over the direction of the state budget and policy environment. It is also important to note that these powers may allow the governor to circumvent the state legislature – in the two opening examples, for instance, Dannel Malloy used his executive authority to impose the hiring freeze on state agencies, while Rick Scott vetoed spending already approved by the legislature.

Governors are elected to four year terms[6] and they are not all elected at the same time. This is instrumental for my empirical study because it means there are sharp differences in election cycles across states and at state borders. Elections take place in November[7] and every year the governorship is up for election in a subset of the states. Figure 1 displays the variation in timing across states: 36 states hold their gubernatorial elections in midterm years; 9 have them in presidential election years; and, 5 in odd numbered years. These election schedules have been historically fixed for

[5] Among others, Chirinko and Wilson (2008) and Coomes and Hoyt (2008) study the effect of fiscal policy on business location decisions and migration decisions; Dube et al. (2010) study the minimum wage; Kahn and Mansur (2013) look at how labor laws, environmental regulations and energy prices affect manufacturing location decisions; and, Peltzman (2016) studies how fiscal policy in a neighboring state affects local economic activity.

[6] With the exception of New Hampshire and Vermont, where terms are two years long.

[7] With the exception of one special election during my sample period, West Virginia in 2011. I also drop the two recall elections, California 2003 and Wisconsin 2012, because their occurrence was unusual and highly related to economic conditions in those states, though this does not affect the results.

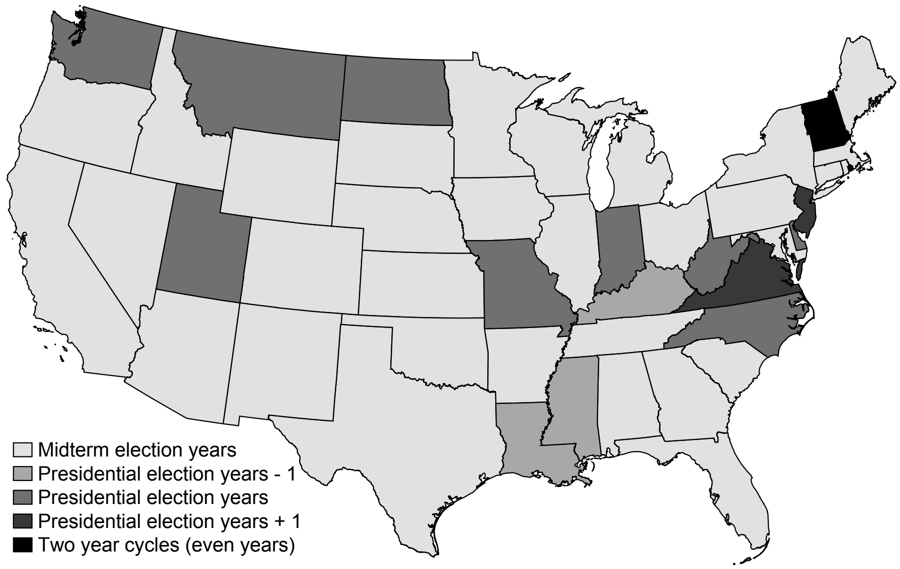

Figure 1: Timing of US gubernatorial elections by state.

some time, with few changes in the past 50 years.[8] Since election schedules are well established, and generally require an amendment to the state constitution to change, they are unlikely to be related to the variation in employment during my sample period.

It is true that the governorship is not identical in each state – each state has its own constitution laying down the powers and responsibilities of the governor. They are, however, fairly comparable and certainly moreso than heads of state as in cross-country studies (Alesina and Roubini, 1992; Shi and Svensson, 2006; Potrafke, 2010, 2012). Static state-specific features of the governorship will largely be controlled for by the inclusion of various county fixed effects. The other immediate concern is that gubernatorial elections will most of the time be taking place simultaneously to other elections – presidential, congressional, local. This is discussed in detail in the next section.

2.2 Empirical strategy

I use two complementary empirical strategies, each one offering its own advantages and caveats. In the first approach, I run fixed effect regressions of the following general

[8] There was only one change during my sample period of 1990-2015, when Rhode Island switched from two-year to four-year terms in 1994. The only other change in term length since the 70s was Arkansas in 1986, also a switch from two- to four-year terms. The three most recent changes in election timing, as opposed to term length, were Illinois in 1976, Louisiana in 1975 and Florida in 1964. Illinois and Florida switched from presidential election years to midterm years. Louisiana moved its elections back one year relative to presidential elections – previously they were concurrent.

form:

$$\Delta \log(Y_{cst}) = \sum_{q=1}^{4} \beta_q \cdot \text{Elec}_{st} \cdot Q_q + \gamma' \cdot X_{ct} + \alpha_{cq} + \lambda_t + \epsilon_{cst}, \quad (1)$$

where c, s, q, and t refer to county, state, quarter and year-quarter. The variable Elec_{st} is a dummy for whether the current quarter is in the election window, a year long period that starts in April, July or October of an election year, depending on the specification.

The dependent variable, $\Delta \log(Y_{cst}) = \log(Y_{cst}) - \log(Y_{cst-2})$, is county c's log two quarter change in employment per capita in some employment category. The coefficients of interest are β_q, which can be interpreted as the percentage difference in Y_{cst}/Y_{cst-2} when there is an election compared to when there is no election over a given two quarter period.[9] Since the mean value of Y_{cst}/Y_{cst-2} is fairly close to one for the variables considered, the coefficient estimate is also approximately equal to the difference in percentage points between the growth rate when there is an election compared to when there is no election.[10]

I use the two quarter change rather than the one quarter change because the cumulative effect is expected to be larger in magnitude, and hence easier to pick up. That is, if there is an effect x from the first quarter to the second, and another effect x from the second quarter to the third, then the combined impact $2x$ may be easier to differentiate from zero. The logic is similar if half of the counties respond from the first to second quarter and the other half respond during the second to third quarter. This dependent variable does lead to a complication in interpretation, since a change in quarter t will show up as a change in both $\Delta \log(Y_t)$ and $\Delta \log(Y_{t+1})$. Thus, the coefficients on consecutive quarters should not be interpreted independently from each other. The interpretation of β_q as the effect of an election on the growth rate over the two quarters leading up to quarter q is appropriate, but we need to keep in mind that growth during overlapping periods may be correlated. I investigate alternative specifications, including the use of one quarter changes, in Section A.1 in the Appendix.[11] I cluster the standard errors at the state level to account for serial correlation and the

[9] Strictly speaking, this would be the exponentiated coefficient, but all the numbers considered in this paper are small enough that exponentiating has a negligible effect.

[10] The average value of Y_{cst}/Y_{cst-2} varies moderately with the season and are, for the first through fourth quarters: 1.15, 1.00, .890 and 1.00 for local government employment; 0.99, 1.01, 1.02 and 1.00 for state government employment; and, 0.96, 1.01, 1.05 and 1.00 for private sector employment.

[11] In particular, using two quarter estimates we cannot extrapolate from a first to third quarter change and a second to fourth quarter change to a first to fourth quarter change. In Section A.1, I show that the results are not sensitive to restricting attention to quarters that are two periods apart and have no overlap (that is, two data points per year rather than four). This makes sense – in specification (2), the inclusion of the year-quarter effects together with county-quarter effects means that the estimate β_q is coming from the comparison of deviations in employment growth from usual across two groups of counties – those that experience an election and those that do not. If we drop the overlapping quarters, the source of the identifying variation for the remaining quarters does not change.

fact that the election variable is constant within a state.

The vector X_{ct}, included in some specifications, contains time invariant county characteristics interacted with linear and quadratic time trends. The included characteristics are: an urban dummy, equal to one if the county belongs to a Metropolitan Statistical area of 250,000 or more residents; dummies for the electoral cycle (whether the state holds elections in presidential, midterm, or off-years); and, January 1990 values of log income per capita, log of population, fraction of private sector employment in goods industries as opposed to services industries.[12] This is intended to control for long term trends affecting different types of counties such as, for example, the gradual decline experienced by manufacturing industries in the US, which may induce different long term trends in counties that were initially heavy in manufacturing employment.

There are two concerns that motivate interacting the baseline characteristics with time trends rather than using contemporaneous or lagged values of these covariates. First, contemporaneous values may also be affected by elections; second, they may open the door to reverse causality if, say, lower employment causes lower income.[13] As an even more saturated alternative that does not assume a functional form, I also consider interacting the time invariant county characteristics (dummies for above or below the median in the case of continuous variables) with the year-quarter effects.[14]

In all specifications I include county-quarter fixed effects, α_{cq}, to control for county-specific seasonal trends and time invariant county characteristics. I also include, unless they are subsumed by other fixed effects, year-quarter effects, λ_t, to control for common shocks in different time periods. The year-quarter fixed effects also should help to alleviate concerns about simultaneous federal elections – that is, presidential and House elections, which occur at the same time across the nation. Senate elections, however, do not all occur at once – they are also staggered across states. In Section 3.5, I incorporate Senate elections into my analysis and find that there is little evidence that they influence employment in the same way as gubernatorial elections, if at all. Local elections, such as mayoral or city council elections, are another potential omitted variable. First, I believe it is reasonable to assume that these elections are, on average, less important than gubernatorial and Senate elections, and should not confound the results in a systematic way, though I concede it is possible. Second, I study local and state government employment separately, and cycles are observed in both. While mayors may be able to affect local government employment, this is much less likely for state employment.

[12] Goods producing refers to NAICS sectors 11-33 and includes natural resources, mining, construction and manufacturing. Service providing refers to all remaining sectors: NAICS sectors 42-99.

[13] Nevertheless, the inferences are robust to using contemporaneous values of the control variables – see Section A.1.

[14] One might also consider adding a lagged dependent variable to the right hand side. Doing so in a fixed effect model, however, results in bias (Nickell, 1981), though the bias disappears asymptotically when the number of time periods becomes large. In Section A.1 I investigate the robustness to including lagged growth rates and lagged levels.

To help control for spatial heterogeneity across the US, in some specifications I include census division linear and quadratic time trends or census division-year-quarter fixed effects. This is to control for divergent long term employment trends in different parts of the country driven by, for example, large scale migration from the Rust Belt states to the western and southern states. Even including these fairly flexible controls, however, substantial spatial heterogeneity can be problematic in traditional fixed effects models that are run at the national level (Dube et. al, 2010). The implicit assumption in specifications such as (2) is that any county in the country is a suitable control for any other, which in many cases is unlikely to be true. Therefore, I present a complementary analysis following the approach of Dube et al. (2010) who, in a study of the employment effects of the minimum wage, implement a geographic discontinuity design that compares employment within pairs of counties that straddle a state border and generally have similar characteristics, belonging to the same local market and experiencing similar economic fluctuations.

The specification takes the following form:

$$\Delta \log(Y_{cjst}) = \sum_{q=1}^{4} \beta_q \cdot \mathrm{Elec}_{st} \cdot Q_q + \gamma' \cdot X_{ct} + \alpha_{cq} + \pi_{jt} + \epsilon_{cjst}, \tag{2}$$

where j now indexes county-pair. A county-pair is a pair of two contiguous counties in different states. Since a county may be paired with more than one county in the bordering state, I include each county as many times as it can be matched to another contiguous county across the border, so that the same county may appear multiple times in the regression. To guard against the mechanical correlation that will be induced by including duplicate counties along a border segment (i.e., pair of adjacent states), I cluster the standard errors both at the state level, as in the fixed effects specifications, and at the border segment level.[15] The key element is the inclusion of county-pair-year-quarter fixed effects π_{jt}. This controls for economic conditions at the very local level, using within county-pair variation in electoral cycles to identify the effect of experiencing an election on employment growth.

Figure 2 illustrates the general idea. It shows counties located along the border segment between North and South Carolina. The counties are shaded according to the deviation of the growth rate between the first and third quarter of the stated year from the usual growth rate for the same period. Leading up to an election period, the hypothesis is that the deviation from usual will be higher on the side of the border with an election coming up (which is North Carolina in 2004 and South Carolina in 2006), and in the post-electoral period, lower in the state that recently had an election (North Carolina in 2005 and South Carolina in 2007). There is some suggestion of a

[15]Counties with more pairs also implicitly receive unequal weight in the regression. In Section A.1, I show the results are not sensitive to weighting each county by the inverse of the number of pairs in which it appears.

pattern, but it is difficult to to tell with the naked eye. In the empirical analysis, the estimates for each border segment are essentially pooled across all segments to obtain the estimates β_q.

The county-pair approach offers a clean identification strategy that controls for very local economic conditions and thus avoids many of the pitfalls associated with the panel specifications. This strategy is not foolproof (Neumark et al. 2013). Counties at state borders may not be as similar as we would like to imagine, and border areas may be subject to different dynamics or spillovers across states. To take an extreme example: Hudson County, NJ, and New York City are certainy quite different, while at the same time having intricately linked economies and labor markets. Although it is well known that private sector firms may shift their activities across the border to escape unfavorable conditions (Holmes, 1998; Kahn, 2004; Kahn and Mansur, 2013), I expect this to be less of an issue with government employment, where jobs are often very state specific and have residency requirements.[16] There is also a practical issue – focusing on border counties reduces the sample size. Thus, the two methods complement each other, and it is comforting that the results are similar using either.

Since the county-pair approach relies on the use of county-level data, both analyses are conducted at the county level for comparability. By clustering the standard errors at the state level (Bertrand et al., 2004), I allow for arbitrary correlations across counties within a state. Including county level controls can also improve the precision of the estimates (Angist and Pischke, 2008; Foremny and Riedel, 2014). I choose to work at the quarterly level rather than at the monthly level since it is less noisy and aids in the presentation of the tables.

2.3 Data

Employment data come from the Quarterly Census of Employment and Wages (QCEW) of the Bureau of Labor Statistics (BLS), which provides monthly employment counts at the county level, from 1990 until the third quarter of 2015. Additionally, the QCEW disaggregates by ownership category and industry. The underlying data are derived from quarterly Unemployment Insurance contribution reports that are required to be filed by 97% of wage and salary civilian employers. The main exclusions are the self-employed, some agricultural categories, and the armed forces.

In my analysis I focus mainly on local government and state government employment. The main categories of both local and state government employment are education, law enforcement and corrections, hospitals and health, judicial and legal, public administration, and others. There are some categories that are always local or always

[16]Taking police officers as an example, training is naturally specific to the state laws and officers are generally not allowed to work in other states unless the move is permanent and they undergo a relicensing procedure. In Texas, for example, to transfer from another state one must submit an application, pass a number of examinations, make up for any lacking basic training, among other paperwork. See `https://www.tcole.texas.gov/content/out-state-peace-officers`.

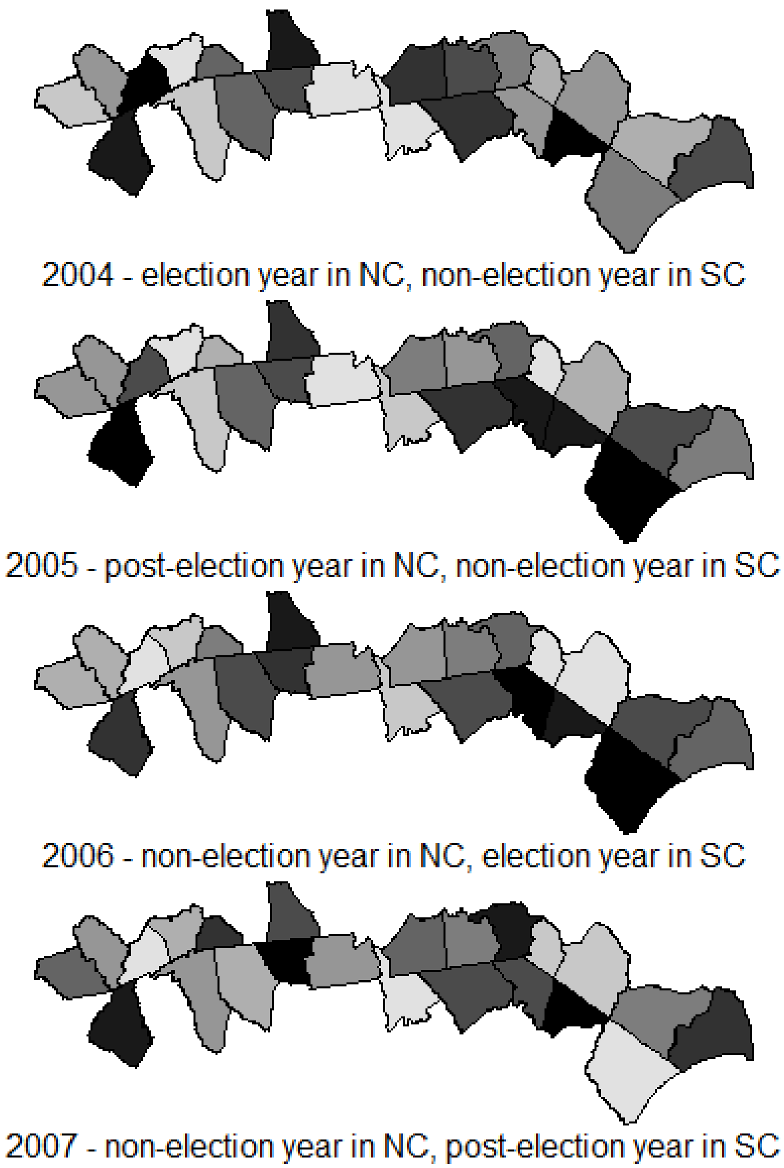

Figure 2: Relative changes in employment from the first to third quarter along the North and South Carolian border over the years 2004-2007. The shades indicate the deviation from the average in that quarter. North Carolina experienced a gubernatorial election in November, 2004, while South Carolina had one in November, 2006.

state (e.g., firefighters are local government employees) and, within a category, there are differences between who is a local and who is a state employee (in education, elementary and secondary school teachers are generally local government employees, while workers in higher education are usually state employees).[17] In Section 3.3 I also consider total private sector employment, consisting of all nongovernmental employment categories with the exceptions mentioned above.

In the first quarter of 2015, the QCEW recorded 14.1 million local government employees, 4.6 million state government employees, and 115.3 million private sector employees in the US. During my period of study, the average county in my sample had about 5, 1.3 and 28 local, state and private sector employees per one 100 residents. The aggregate trends in employees per 100 residents are shown in Figure 3. Local government employment was fairly stable, rising steadily between 1990 and 2010 from about 4.4 per 100 to almost 5.1. Between 2010 and 2015, this trend is reversed and the number of local government employees gradually declined back to about 4.9 per 100. State government employment is fairly stable at about 1.3 per 100. Private sector employment shows more variability, with a minimum of about 26.5 per 100 in 1991 and a maximum of over 29 in 2000. Troughs corresponding to the recessions of the early 1990s and 2000s, and the Great Recession are clearly visible. Seasonality is pronounced in local government and private sector employment – in the former, the abrupt yearly decreases are due mainly to school teachers who do not work during the summer.

My population and income data come from the Bureau of Economic Analysis[18] and the urban/rural dummy comes from a classification by the National Council for Health Statistics. Data on election outcomes are primarily from David Leip's Atlas on US Presidential elections[19] and sources such as individual state agency websites. My sample period of 1990-2015 consists of 1,223 full state-years, in 344 of which there were gubernatorial elections.

The main sample for the fixed effects analysis is constructed as follows. Starting from the current 3,143 county equivalents in the US, I exclude counties that have missing data for the variables considered and counties that were affected by boundary changes and could not be matched across datasets. One complication is that the Bureau of Economic Analysis, for statistical purposes, combines several county equivalents into one statistical area.[20] Where possible, I aggregated the QCEW employment data for counties within a statistical area so that they could be matched across datasets – thus,

[17] One might be concerned that the changes in government employment I observe are simply due to poll workers who supervise polling stations on election day. There are multiple reasons to believe this is not the case, discussed in Section A.2. One of the most important is that poll workers earning less than $1000 per annum, the vast majority, are not covered in the definition of employment for state unemployment insurance reporting purposes (Title 26, United States Code §3309(b)(3)(F), 2010).

[18] This data are annual and, hence, I impute missing values within years using linear interpolation.

[19] See `uselectionatlas.org.`

[20] This occurs predominantly in Virginia, where geographically small independent cities are combined with a larger neighboring county.

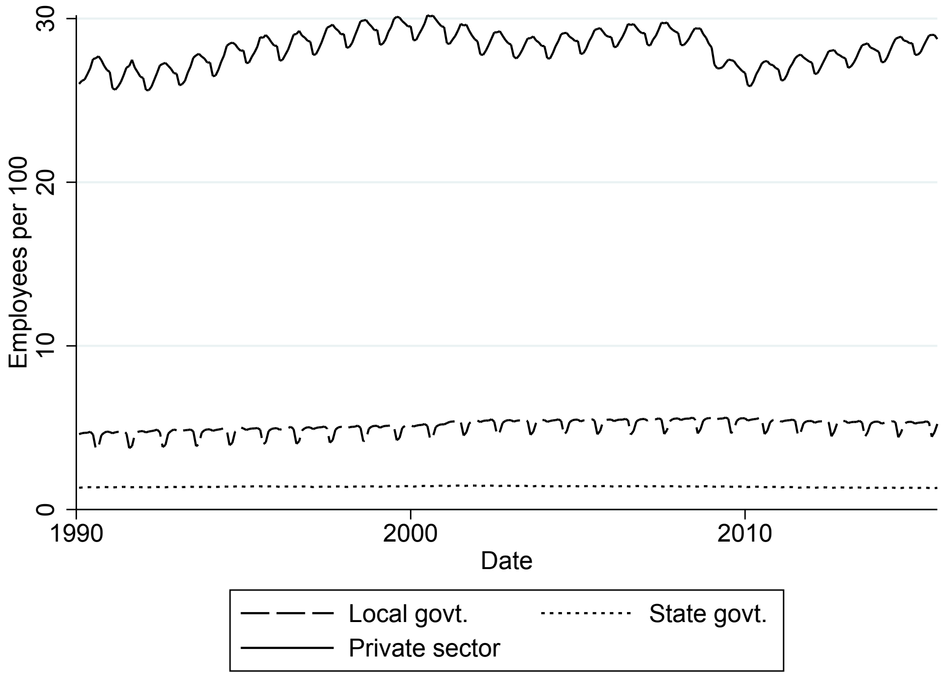

Figure 3: Trends in local government, state government, and private sector employment.

a few of my "counties" consist of more than one county equivalent area. Another issue is that the BLS suppresses employment data for some counties out of confidentiality concerns. I exclude all counties that have suppressed data during my sample period.

It can also happen that changes in QCEW employment numbers are due to reclassification of establishment industries, i.e., administrative rather than real economic changes. While the BLS attempts to account for this, some clearly implausible changes remain, though there is no reason to believe these anomalies are related to elections. To lessen the impact of such administrative changes in a consistent way, I exclude counties whose maximum (minimum) change in quarter-to-quarter employment per capita is in the top or bottom 1%.[21]

My final sample consists of 1,759 counties, with a combined population of 235.3 million in 2015, or about 74% of the total US population of 319.9 million (US Census Bureau). In terms of the workforce, in the first quarter of 2015, my sample includes a total of 10.2, 3.7 and 82.7 million local government, state government, and private sector employees out of 14.1, 4.6, and 115.3 million recorded by the QCEW, or about 72% coverage for each variable. Summary statistics are in Table 1, and the counties included are shown in Figure 4.[22]

[21]There remain some large, though plausible, outliers. In Section A.4, I check the robustness of the results to excluding outliers.

[22]The missing counties are often sparsely populated counties in the West, with the notable exception

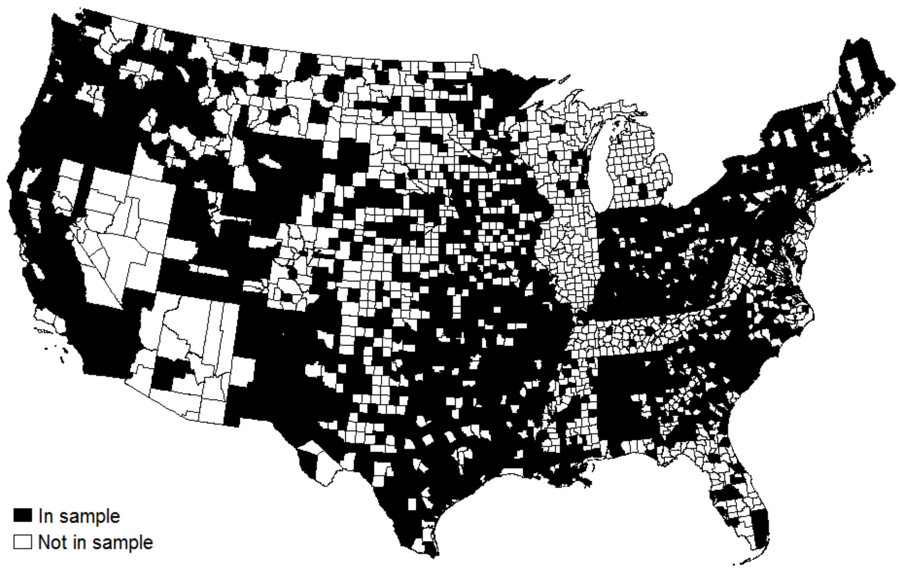

Figure 4: Main samples for the fixed effects specifications, based on all counties for which both local and state government employment are available.

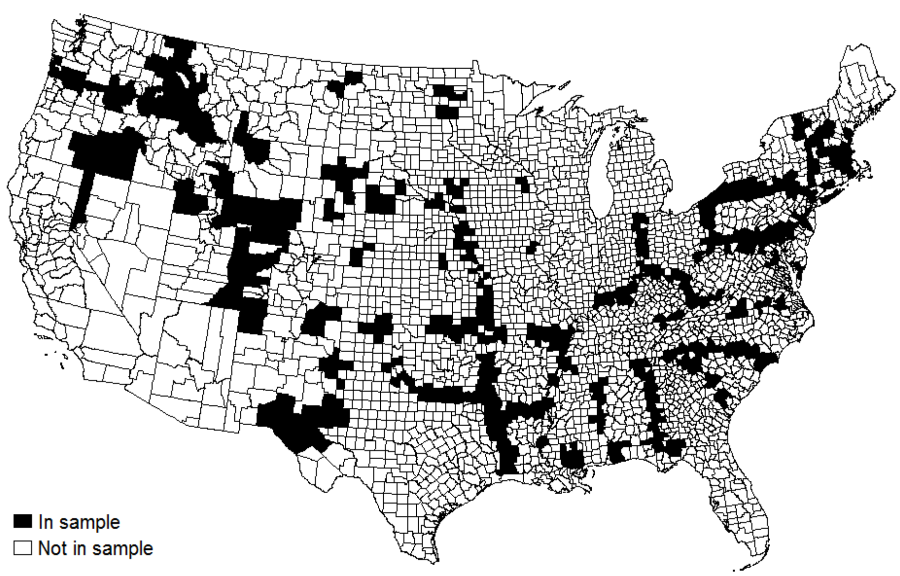

Figure 5: Main samples for the county-pair specifications, based on all border counties for which both local and state government employment are available.

	Mean	s.d.	Min	Median	Max
Employment					
Local government	5120	15114	105	1632	479116
State government	1771	5671	8	298	115099
Private sector	42719	138069	165	9263	3789452
Employment per capita					
Local government	0.050	0.016	0.005	0.046	0.292
State government	0.013	0.017	0.000	0.008	0.300
Private sector	0.280	0.107	0.039	0.270	1.217
Log two quarter change in employment per capita					
Local government	0.001	0.129	-1.302	0.001	1.713
State government	-0.001	0.109	-2.014	-0.002	2.422
Private sector	0.002	0.064	-0.806	0.002	0.885
Political variables					
Elec (Apr-Mar window)	0.265	0.441	0	0	1
Elec (Oct-Sep)	0.262	0.440	0	0	1
Senate election (Apr-Mar)	0.311	0.463	0	0	1
Senate election (Oct-Sep)	0.311	0.463	0	0	1
Presidential cycle dummy	0.198	0.398	0	0	1
Off-years cycle dummy	0.136	0.343	0	0	1
Close	0.089	0.284	0	0	1
Incumbent term limited	0.115	0.319	0	0	1
Strict balanced budget	0.379	0.485	0	0	1
Presidential elec. window	0.233	0.423	0	0	1
Governor and legislature aligned	0.221	0.415	0	0	1
Turnover	0.078	0.268	0	0	1
Time invariant controls					
Urban	0.296	0.456	0	0	1
1990 pop.	104271	315776	2905	31403	8878157
1990 inc. per cap.	27971	5911	10214	27418	62939
1990 goods fraction	0.360	0.140	0.047	0.346	0.788

Based on main sample of 1,759 counties in 49 states, 180,949 observations (177,431 for the log growth rates).

Table 1: Summary statistics for the sample of all counties used in the fixed effects regressions.

For the county-pair sample, I start from 1,069 counties that are contiguous to another state and proceed as above, only that now I exclude pairs where either county is excluded according to the above criteria. I end up with 504 counties, from which 433 county-pairs can be formed at 72 border segments (pairs of adjacent states). Summary statistics for this sample appear in Table 2 and the included counties are shown in Figure 5.[23] Out of 72 border segments: 32 have the same election cycle; for 21, the two

of most of Illinois, Michigan and Tennessee, which are excluded mainly due to suppressed state government figures. Since there are more local government employees than state, coverage of the former variable tends to be better, and in Section A.4 I check whether the results for local government continue to hold on a larger sample (see Figure A1) that only requires that counties have local government figures, rather than both local and state.

[23]In Section A.4, I again consider the larger sample of counties that have local government numbers

	Mean	s.d.	Min	Median	Max
Employment					
Local government	4135	7762	94	1636	82714
State government	1302	3559	10	327	78690
Private sector	34874	76336	205	9430	795938
Employment per capita					
Local government	0.048	0.017	0.005	0.045	0.293
State government	0.012	0.014	0.000	0.007	0.340
Private sector	0.279	0.105	0.065	0.270	1.217
Log two quarter change in employment per capita					
Local government	0.001	0.132	-0.883	0.001	1.713
State government	-0.000	0.110	-1.980	-0.001	1.935
Private sector	0.002	0.067	-0.766	0.002	0.885
Political variables					
Elec (Apr-Mar window)	0.266	0.442	0	0	1
Elec (Oct-Sep)	0.264	0.441	0	0	1
Senate election (Apr-Mar)	0.311	0.463	0	0	1
Senate election. (Oct-Sep)	0.311	0.464	0	0	1
Presidential cycle dummy	0.206	0.405	0	0	1
Off-years cycle dummy	0.142	0.350	0	0	1
Close	0.092	0.290	0	0	1
Incumbent term limited	0.120	0.326	0	0	1
Strict balanced budget	0.407	0.491	0	0	1
Presidential elec. window	0.233	0.423	0	0	1
Governor and legislature aligned	0.223	0.416	0	0	1
Turnover	0.081	0.272	0	0	1
Time invariant controls					
Urban	0.284	0.451	0	0	1
1990 pop.	90768	180663	2905	33561	1584293
1990 inc. per cap.	28019	6241	14595	27158	62620
1990 goods fraction	0.369	0.144	0.047	0.356	0.719

Based on sample of 504 counties, forming 433 county-pairs, along 72 border segments, in 43 states, 89,198 observations (87,466 for the log growth rates).

Table 2: Summary statistics for the sample of all counties used in the county-pair regressions.

states have elections that are two years apart; for 15, one state leads the other by one year (so that elections are one or three years apart); and, for 4 segments, one state has two year cycles and the other has four year cycles, so that their elections coincide in presidential election years but not midterm years.

2.4 Descriptive statistics

Table 3 compares, for each quarter, the average two quarter growth rate of counties in states that experience an election with counties in states that do not. For local government employment, those counties experiencing an election year grow more than those

but may or may not have state government (see Figure A2).

	No election		Election			
	Mean	s.e.	Mean	s.e.	Diff.	t-stat.
Local govt. employment growth						
Q3	-12.61	0.07	-12.55	0.11	0.06	0.48
Q4	0.33	0.02	0.39	0.04	0.06	1.41
Q1	13.08	0.07	13.03	0.11	- 0.05	0.41
Q2	0.16	0.02	-0.12	0.05	-0.28	-5.72
State govt. employment growth						
Q3	1.39	0.06	2.02	0.10	0.63	5.34
Q4	-0.98	0.05	-0.05	0.08	0.42	4.78
Q1	-1.86	0.07	-1.52	0.12	0.34	2.43
Q2	0.67	0.06	0.50	0.10	-0.18	-1.57

Table 3: Comparison of employment growth between counties experiencing and not experiencing an election.

	No election		Election			
	Mean	s.e.	Mean	s.e.	Diff.	t-stat.
Local govt. employment growth						
Q3	-0.06	0.03	0.17	0.05	0.23	3.36
Q4	-0.02	0.02	0.07	0.03	0.10	2.70
Q1	0.06	0.04	-0.16	0.07	-0.21	-2.82
Q2	0.03	0.02	-0.09	0.04	-0.13	-2.90
State govt. employment growth						
Q3	-0.11	0.04	0.30	0.08	0.41	4.82
Q4	-0.02	0.04	0.05	0.07	0.08	0.96
Q1	0.02	0.06	-0.07	0.09	-0.10	-0.92
Q2	0.05	0.05	-0.14	0.09	-0.19	-1.77

Table 4: Comparison of employment growth between counties experiencing and not experiencing an election, net of county-quarter and year-quarter fixed effects.

that do not, in the quarters leading up to the election. The pre- to post-election change in employment, corresponding to Q1, however, is lower in counties that experienced an election. The same is observed in the following quarter, Q2, and a t-test rejects the null hypothesis of equal means. For state government employment, the pattern is similar, with counties experiencing elections growing faster in the pre-November period than those that do not experience elections, but slower afterwards. Moreover, we can reject equality of means at conventional levels for the first three of the four quarters.

Trying to discern patterns in the raw data is made difficult by large baseline differences across counties, pronounced seasonal hiring patterns, and fluctuations in national economic conditions over time. In Table 4, I compare the growth rates net of county specific seasonality and time trends. That is, I plot the residuals from regressions including only county-quarter and year-quarter fixed effects. Here the patterns of higher growth before and lower growth after in counties experiencing an election is even more

	(1)	(2)	(3)	(4)	(5)	(6)
	Local government			State government		
Apr-Mar window						
Elec·Q2	0.18**	0.17*	0.16**	0.31	0.32	0.20
	(0.09)	(0.09)	(0.08)	(0.31)	(0.31)	(0.38)
Elec·Q3	0.35*	0.35*	0.35*	0.61***	0.59***	0.50***
	(0.19)	(0.19)	(0.19)	(0.12)	(0.12)	(0.15)
Elec·Q4	0.15***	0.14***	0.12**	0.11	0.08	-0.04
	(0.05)	(0.04)	(0.05)	(0.15)	(0.15)	(0.13)
Elec·Q1	-0.32*	-0.33*	-0.30*	-0.16	-0.19	-0.11
	(0.16)	(0.17)	(0.16)	(0.33)	(0.33)	(0.35)
Oct-Sep window						
Elec·Q4	0.15***	0.14***	0.12**	0.11	0.08	-0.05
	(0.05)	(0.04)	(0.05)	(0.15)	(0.15)	(0.13)
Elec·Q1	-0.32*	-0.33*	-0.30*	-0.16	-0.19	-0.12
	(0.16)	(0.17)	(0.16)	(0.33)	(0.33)	(0.36)
Elec·Q2	-0.19**	-0.20**	-0.18**	-0.28	-0.32	-0.06
	(0.09)	(0.09)	(0.07)	(0.27)	(0.26)	(0.25)
Elec·Q3	-0.13	-0.13	-0.12	-0.72**	-0.75**	-0.44**
	(0.12)	(0.12)	(0.09)	(0.34)	(0.33)	(0.18)
N	177431	177431	177431	177431	177431	177431
R^2	0.82	0.82	0.82	0.34	0.34	0.36
County· quarter	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y		Y	Y	
Controls·trends		Y			Y	
Controls·year·quarter			Y			Y
CD·year·quarter			Y			Y

Based on sample of 1759 counties in 49 states. Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 5: Fixed effects results for local and state government employment.

pronounced, and the null of equal means is rejected in 5 of 8 cases.

These observations are quite suggestive and are consistent with the manipulation hypothesis. I now proceed to the regression analyses.

3 Results

3.1 Local government employment

Columns (1)-(3) of Table 5, top panel, present the results of the fixed effects specifications for growth in local government employment when the year long election period is defined as starting in the second quarter of the election year. Column (1) includes only the main independent variables of interest, in addition to county-quarter and year-quarter fixed effects. Column (2) incorporates linear and quadratic time trends for each of the nine census divisions. It also includes time invariant control variables interacted with linear and quadratic time trends: an urban/rural dummy, electoral cycle dummies, and the January 1990 values of log population, log income per capita, and fraction of employment in goods producing industries. Column (3) includes census division-year-quarter fixed effects and interacts the time invariant county characteristics (for continuous variables, a dummy for above or below the median value) with the year-quarter fixed effects. I retain the electoral cycle dummies interacted with linear and quadratic time trends, since interacting these variables with year-quarter effects would sweep out the identifying variation.

All three specifications indicate that growth in local government employment per capita is higher in counties that have a gubernatorial election coming up than in counties that do not. The estimated coefficient peaks at 0.35% higher for Elec·Q3, which captures the growth rate from the first quarter to the third quarter. Its magnitude can also be interpreted as a growth rate over this period that is 0.31 percentage points higher, since the average value of Y_{cst}/Y_{cst-2} is 0.89 for this quarter. This amounts to a change in employees over this period that is larger by about 18 local government employees for the average sized county. Given the typical state has about 63 counties, this is an economically substantial effect. The estimate for Elec·Q1, the first quarter of the post-election year is negative and similar in magnitude, significant at the 10% level. This coefficient captures the change in local government employment from the third quarter of the election year to the first quarter of the following year – i.e., the pre- to post-election change. That it is negative suggests that counties that experienced an election go from higher growth leading up to the election, to lower growth immediately following, consistent with the manipulation hypothesis.

In the bottom panel of Table 5, I repeat these specifications, redefining the year long election period to start in the fourth quarter. That is, the election window is shifted back by two quarters to examine more of the post-election period. The coefficients for the fourth and first quarters, included in both windows, remain similar, so changing the window does not affect inferences. The pattern of lower growth in local government employment per capita in counties that experienced a gubernatorial election continues through the second quarter of the post-election year, again consistent with manipulation of local government employment.

In columns (1)-(4) of Table 6, top panel, I show the results of the county-pair analysis for the election window starting in the second quarter of the election year. Column (1) includes county-quarter and pair-year-quarter fixed effects. Column (2) includes control variables interacted with linear and quadratic time trends, with the exception of census division time trends, since local economic shocks are already captured by the pair-year-quarter effects. Column (3) replaces these trends with dummy variables interacted with time trends, as described above. Since moving to the county-pair method involves a

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Local government				State government		
Apr-Mar window								
Elec·Q2	0.22^{**}	0.23^{**}	0.25^{**}	0.13	0.37	0.38	0.40	0.53
	(0.11)	(0.11)	(0.11)	(0.11)	(0.37)	(0.36)	(0.36)	(0.37)
Elec·Q3	0.55^{**}	0.54^{**}	0.54^{***}	0.57^{**}	0.61^{***}	0.62^{***}	0.61^{***}	0.43^{***}
	(0.21)	(0.21)	(0.20)	(0.26)	(0.20)	(0.20)	(0.21)	(0.14)
Elec·Q4	-0.06	-0.08	-0.08	0.15^{*}	-0.08	-0.11	-0.18	-0.31
	(0.11)	(0.11)	(0.10)	(0.08)	(0.19)	(0.18)	(0.18)	(0.19)
Elec·Q1	-0.46	-0.48^{*}	-0.50^{*}	-0.54^{*}	-0.56	-0.59^{*}	-0.65^{*}	-0.43^{*}
	(0.28)	(0.28)	(0.27)	(0.29)	(0.34)	(0.32)	(0.33)	(0.24)
Oct-Sep window								
Elec·Q4	-0.06	-0.09	-0.08	0.15^{*}	-0.08	-0.12	-0.19	-0.32^{*}
	(0.11)	(0.11)	(0.10)	(0.08)	(0.19)	(0.18)	(0.18)	(0.19)
Elec·Q1	-0.46	-0.49^{*}	-0.50^{*}	-0.54^{*}	-0.56	-0.60^{*}	-0.65^{*}	-0.44^{*}
	(0.28)	(0.28)	(0.27)	(0.29)	(0.34)	(0.32)	(0.33)	(0.24)
Elec·Q2	-0.15	-0.17	-0.20	-0.24^{**}	-0.67^{**}	-0.71^{**}	-0.74^{***}	-0.54^{*}
	(0.17)	(0.16)	(0.17)	(0.11)	(0.29)	(0.28)	(0.26)	(0.27)
Elec·Q3	-0.10	-0.12	-0.10	-0.06	-0.79^{***}	-0.83^{***}	-0.87^{***}	-0.95^{***}
	(0.10)	(0.11)	(0.10)	(0.14)	(0.26)	(0.27)	(0.28)	(0.32)
N	87466	87466	87466	39231	87466	87466	87466	39231
R^2	0.91	0.91	0.91	0.83	0.68	0.68	0.68	0.83
County· quarter	Y	Y	Y		Y	Y	Y	
Pair·year·quarter	Y	Y	Y		Y	Y	Y	
Controls·trends		Y				Y		
Controls·year·quarter			Y				Y	
Panel regression				Y				Y

Based on sample of 504 counties, forming 433 county-pairs, along 72 border segments, in 43 states. Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) two way clustered at the state and border segment level. The panel regressions uses the preferred specification from columns (2) and (5) of Table 5. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 6: County-pair results for local and state government employment.

change in sample, column (4) runs a fixed effects regression on the sample of border counties, using the specification (2) of Table 5.

The results are stable across specifications and, again, consistent with manipulation – we observe higher growth in local government employment in counties that are coming up to a gubernatorial election, up to 0.55% higher in the third quarter of the election year (almost 0.5 percentage points). As in the fixed effects models, we observe negative coefficients after the election, becoming significant at the 10% level in the first quarter of the post-election year. The main difference is that now the estimate for the fourth quarter, Elec·Q4, turns out to be small, negative, and insignificant, whereas previously it was positive and significant. Column (4), the fixed effects specification run on the border counties sample, is largely consistent with the county-pair results, although there are some differences in the estimates for Elec·Q2 and Elec·Q4 – the former is attenuated and does not turn out to be significant, while the latter remains significant.

Shifting the election window back by two quarters, the bottom panel shows that local government employment growth is lower in counties that had an election in the post-electoral period, as observed in the fixed effects regressions. The estimates are, however, less precise, and attain lower levels of statistical significance.

As the results are highly stable across the specifications, I choose the specifications in columns (2) of Tables 5 and 6 as the preferred specifications for the fixed effects and county-pair approaches and, moving forward, I will often present results for these specifications only to save space.

3.2 State government employment

Columns (4)-(6) of Table 5 and columns (5)-(8) of Table 6 show the results of analogous fixed effects and county-pair specifications with growth in state government employment per capita as the dependent variable. There are two patterns that stand out and are robust across specifications. First, state government employment per capita growth is higher in counties that have a gubernatorial election coming up by about 0.61% (0.64 percentage points), significant at the 1% level. Second, following the election the signs switch and counties that had an election have lower growth, by about 0.7% or even more, than those that did not. The estimates become highly significant in the two to three quarters after the election. These effects in the pre-electoral period corresponds to a change in state government employees that is larger by about 11 employees per county. In the post-electoral period, the effects corresponds to a change in employees that is about 15 employees smaller.

These effects are consistent with manipulation and follow a similar pattern to that observed for local government employment. Again, I choose the models in column (5) of Table 5 and column (6) of Table 6 as the preferred specifications for studying state government employment going forward.

3.3 Private sector employment

We might expect private sector employment to behave differently under the manipulation hypothesis than government employment. It is likely more within the governor's ability to manipulate government employment than private sector employment, e.g., the governor has influence over the allocation of resources to public agencies and organizations through the state budget, as well as through the governor's direct executive powers (recall Dannel Malloy and Rick Scott from the Introduction). Private sector firms, on the other hand, are generally less dependent on the state government, so I expect that manipulation should be less prevalent in private sector employment compared to government employment.

At the same time, there is a plausible second channel through which elections are likely to affect employment – heightened uncertainty about future policies (Bloom, 2009; Baker et al., 2015). In the case of publicly listed firms, elections are associated with decreased investment – firms find it optimal to adopt a "wait and see" approach to investment that may be affected by any future changes in policy (Julio and Yook, 2012; Jens, 2016). Indeed, Jens (2016) also studies US gubernatorial elections and finds very large effects of uncertainty (up to 15% in close elections), and hiring is likely impacted in similar ways. Thus, policy uncertainty might be predicted to give rise to lower employment growth prior to elections, followed by a "rebound" effect – a period of increased growth afterwards. Note that these predicted effects would push in the opposite direction to the effects that we expect to see due to manipulation, and uncertainty could plausibly apply to both government and private sector employment. To the extent that government employment is subject to such effects, the estimated manipulation effects in the previous section are likely to be lower bounds. It is plausible, however, to hypothesize that the private sector should in general be more impacted by policy uncertainty than the governmental sector, in which case the manipulation and uncertainty effects would offset each other to a greater degree.

In Table 7, I show the results for private sector employment. In the fixed effects approach, columns (1)-(3) show estimates that are positive in the two quarters leading up to the election. The estimates are negative, in most cases, in the quarter of the election and the following three quarters. However, the magnitudes of these coefficient estimates are small and they do not turn out to be distinguishable from zero, with a few exceptions where they are weakly significant. Those coefficients that are significant are no longer significant in the county-pair results, shown in columns (4)-(7), where we also do not see much going on.[24] This lack of a clear effect in the private sector is consistent with a combination of: (i) weaker or non-existent manipulation in the private sector compared to the government sector; (ii) stronger uncertainty effects in

[24] Border area, however, may be subject to different dynamics than non-border areas, since it is widely documented that there may be cross border spillovers and firms may locate on the side of the border offering a more favorable business environment. See footnote 5 for many papers on these issues.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Private sector, fixed effects			Private sector, county-pair			
Apr-Mar window							
Elec·Q2	0.09	0.08	0.09	-0.06	-0.07	-0.09	0.06
	(0.06)	(0.06)	(0.07)	(0.10)	(0.10)	(0.10)	(0.10)
Elec·Q3	0.05	0.06	0.09**	0.10	0.09	0.08	0.08
	(0.04)	(0.04)	(0.04)	(0.07)	(0.07)	(0.07)	(0.06)
Elec·Q4	-0.07	-0.06	-0.02	-0.07	-0.08	-0.10	-0.11
	(0.05)	(0.04)	(0.04)	(0.06)	(0.07)	(0.07)	(0.09)
Elec·Q1	-0.11*	-0.11*	-0.07	-0.06	-0.07	-0.08	-0.17*
	(0.06)	(0.06)	(0.06)	(0.11)	(0.12)	(0.11)	(0.10)
Oct-Sep window							
Elec·Q4	-0.07	-0.06	-0.02	-0.07	-0.08	-0.10	-0.11
	(0.05)	(0.04)	(0.04)	(0.06)	(0.07)	(0.07)	(0.09)
Elec·Q1	-0.11*	-0.11*	-0.07	-0.06	-0.07	-0.08	-0.17*
	(0.06)	(0.06)	(0.06)	(0.11)	(0.12)	(0.11)	(0.10)
Elec·Q2	-0.07	-0.06	0.04	0.17*	0.17*	0.17*	0.00
	(0.09)	(0.09)	(0.07)	(0.09)	(0.09)	(0.09)	(0.12)
Elec·Q3	-0.06	-0.06	-0.03	0.02	0.01	0.01	0.07
	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
N	177431	177431	177431	87466	87466	87466	39231
R^2	0.70	0.71	0.71	0.87	0.87	0.87	0.71
County· quarter	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y					Y
Pair·year·quarter				Y	Y	Y	
Controls·trends		Y			Y		Y
Controls·year·quarter			Y			Y	
CD·year·quarter			Y				

Columns (1)-(3) based on sample of 1759 counties in 49 states, columns (4)-(7) based on sample of 504 counties, forming 433 county-pairs, along 72 border segments, in 43 states. Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level in the fixed effects regressions and, in the county-pair regressions, additionally at the border segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 7: Fixed effects and county-pair results for private sector employment.

the private sector than in the government sector.

3.4 Heterogeneity

I test whether the effects of gubernatorial elections are different in certain settings, such as whether or not the election is close, by splitting the sample. In Tables 8 and 9, if Close is the dummy variable of interest, then Elec·Q3-Elec·Q2 refer to the estimates for the counties for which Close=0. The estimates for Close·Elec·Q3-Close·Elec·Q2 correspond to counties with Close=1. For both sets of variables, the reference group is counties in states that have no elections. If an estimate for the group where Close=1 is in boldface, this means that it is statistically distinguishable from the corresponding estimate when Close=0, and analogously for other variables.[25] In this section, I restrict attention to the fixed effects specifications, since in the county-pair specification identifying variation may be coming from too few border segments.[26]

The analysis has so far considered all elections uniformly. However, not all elections are the same in terms of the incentives for political manipulation they produce. One important factor is how closely contested the election is. When an election is closer, it is possible that incumbents have greater incentives to manipulate the economy to raise employment, since small changes may be more likely to be decisive. Thus, I split the sample based on whether or not the election is close (having a winning margin of 5% or less). Columns (2) and (5) of Table 8 shows the results for local and state government employment. There is a pattern of decreasing estimates for both groups of counties, suggesting that manipulation occurs in both close and non-close elections, though the estimates are most of the time lower for close elections (the difference is significant at the 10% level in the third quarter for local government). This is not necessarily evidence of lower manipulation in close elections, however, since there is an endogeneity problem if areas that experience lower growth tend to have closer elections.

Term limits may also affect incentives to manipulate. Term limits are fixed by law in many states, so endogeneity issues are less of a concern.[27] One hypothesis is that incentives for governors to manipulate employment should be lower when they are not eligible for another term, though the effect could be the opposite if, e.g., open elections tend to be more competitive, so the party of the governor finds it more desirable to manipulate. The results, in columns (3) and (6) of Table 8, are mixed. For local government, the pattern of positive estimates before elections and negative estimates after does not arise when the incumbent is term limited – it appears that the election effects are driven more by elections with non-term limited incumbents, which is consistent with stronger incentives to manipulate for governors that are eligible for re-election. However, for state government employment, whether the incumbent is term limited

[25] Equivalently, I could estimate the direct effect of an election for all counties, and add an interaction term between the election variable and the dummy of interest to estimate a differential effect. I believe it is more intuitive to tabulate separate election effects for the two groups, since it is easier to compare the two groups of counties with elections to the counties without elections.

[26] For example, to estimate separate effects of elections when the incumbent is or is not term limited, I need to both compare states that have an election with a term limited incumbent to states with no elections, and compare states with non-term limited incumbents with states without elections, which drastically reduces statistical power because in the county-pair approach the comparison additionally needs to be across *contiguous* states.

[27] I prefer to look at elections where the incumbent is term limited rather than open elections more broadly, since the decision not to run for re-election by an incumbent who is not term limited is likely to be closely related to the economic conditions, whereas term limits bind for any incumbent. It is, of course, possible that better performing governors are more likely to survive in office to face a binding term limit. However, Jens (2016) argues term limits are exogenous and uses them as an instrument for closeness. I attempted to do this in my sample, but I obtain a small F-statistic in the first stage, potentially leading to weak instrument problems.

	(1)	(2)	(3)	(4)	(5)	(6)
		Local govt.			State govt.	
Elec·Q3	0.35*	0.31	0.54**	0.59***	0.63***	0.39***
	(0.19)	(0.19)	(0.22)	(0.12)	(0.14)	(0.14)
Elec·Q4	0.14***	0.20***	0.31***	0.08	0.13	0.07
	(0.04)	(0.06)	(0.06)	(0.15)	(0.17)	(0.17)
Elec·Q1	-0.33*	-0.20	-0.32	-0.19	-0.15	-0.08
	(0.17)	(0.19)	(0.25)	(0.33)	(0.33)	(0.40)
Elec·Q2	-0.20**	-0.15	-0.25**	-0.31	-0.12	-0.47
	(0.09)	(0.10)	(0.11)	(0.26)	(0.21)	(0.33)
Close·Elec·Q3		0.52			0.42	
		(0.34)			(0.43)	
Close·Elec·Q4		-0.07			-0.09	
		(0.20)			(0.35)	
Close·Elec·Q1		**-0.81*** ***			-0.34	
		(0.27)			(0.68)	
Close·Elec·Q2		-0.40***			-1.04	
		(0.13)			(0.86)	
TL·Elec·Q3			-0.08			**1.05*** ***
			(0.45)			(0.28)
TL·Elec·Q4			**-0.26*** *			0.11
			(0.15)			(0.22)
TL·Elec·Q1			-0.34			-0.47
			(0.46)			(0.40)
TL·Elec·Q2			-0.07			0.05
			(0.12)			(0.26)
N	177431	177431	177431	177431	177431	177431
R^2	0.82	0.82	0.82	0.34	0.34	0.34
County· quarter	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y

Based on sample of 1759 counties in 49 states. Dependent variable is the two quarter log growth rate. The variables Elec·Q3-Elec·Q2 are the estimates for the counties for which the dummy variable of interest is equal to zero. Boldface indicates that the estimate is statistically different from the corresponding estimate for the group where the dummy variable is equal to zero, at the 10% level (that is, there is some evidence of a differential effect). Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 8: Heterogeneity.

does not appear to dampen the usual pattern – in fact, the effect in the third quarter is significantly larger when term limits bind compared to when they do not.

I next consider institutional differences in state budgeting processes. First, states have budget rules with different levels of stringency, based on whether balanced budget provisions affect the *enactment* or the *execution* of the state budget (Clemens and Miran, 2012). Rules of the former type require only that the state legislature pass a balanced budget (in expectation), while the latter type restricts carrying (actual) deficits through to the next year – any deficit is deducted from next year's revenue. States are scored on the stringency of budget rules in a 1987 Advisory Commission on Intergovernmental Relations (ACIR) report. Following Clemens and Miran (2012), I define as strict balanced budget states the 19 states with scores of 7 or more on the ACIR's 1-10 scale and with annual budgetary and legislative cycles.[28,29] Since opportunistic manipulation of public employment puts pressure on the state budget, it may be that post-election cuts will be more likely and greater in magnitude in states with strict balanced budget laws where any deficit should be eliminated before the end of the budget cycle. Fiscal years typically start on July 1, so that in a state with an annual budget cycle, any "unexpected" deficit incurred due to manipulation should be eliminated within 8 months following the election. For states with less stringent budget laws or non-annual budget cycles, such post-election cuts should be less expedient.[30]

The results are in columns (1) and (4) of Table 9. Counties in both strict and lenient states exhibit the usual pattern of positive estimates before elections and negative afterwards. For local government, counties in states with strict balanced budget rules cannot be differentiated, in the pre-period, from counties in states without elections. In the post-period, counties in states with lenient budget rules cannot be differentiated from counties without elections, while counties with strict rules have substantially lower growth than counties without elections. This is consistent with it being easier to manipulate in lenient states and a more urgent need to balance the budget in strict states, though we cannot say that the election effects are different between strict and lenient states. For state government, the results do not make as much sense – the post-election effects are more negative in magnitude in lenient states than strict states, though the difference is not statistically significant.

I also test whether gubernatorial elections have different effects in presidential election years compared to non-presidential election years. One might expect the effect of a gubernatorial election to be different in a presidential election year compared to other years, since the bulk of media attention is generally focused on the presidential race rather than gubernatorial races. However, it is not obvious what effect this should have. On the one hand, coattail effects may reduce the incentive to manipulate for individual governors; on the other, governors likely still want to influence the results of the presidential election since low turnout there will imply low turnout for the gubernatorial election. The results in columns (2) and (6) of Table 9 show that the usual pattern shows up in both presidential and non-presidential election years. The post-

[28]Some states have strict rules but their budget cycles last more than one year, in which case we expect that any post-election cuts required to balance the budget are less immediately expedient.

[29]These states are AL, AZ, CO, DE, GA, ID, IA, KS, MS, MO, NJ, NM, OK, RI, SC, SD, TN, UT and WV.

[30]However, Boylan (2008) shows how state governments use overly optimistic revenue forecasts to circumvent balanced budget requirements, and increase spending prior to elections. Alternatively, having stringent balanced budget laws may be correlated with the presence of other forms of fiscal and political discipline, in which case there would be less manipulation.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local govt.				State govt.			
Elec·Q3	0.24	0.19	0.37*	0.40*	0.67***	0.52***	0.55**	0.40**
	(0.14)	(0.13)	(0.22)	(0.22)	(0.14)	(0.17)	(0.23)	(0.19)
Elec·Q4	0.17***	0.34*	0.09	0.21**	0.26	0.35	0.20	0.03
	(0.06)	(0.18)	(0.08)	(0.09)	(0.18)	(0.21)	(0.19)	(0.18)
Elec·Q1	-0.18	-0.28**	-0.52*	-0.26	-0.28	0.21	-0.41	-0.10
	(0.13)	(0.12)	(0.28)	(0.20)	(0.37)	(0.31)	(0.44)	(0.38)
Elec·Q2	-0.10	-0.09	-0.25**	-0.19*	-0.48*	-0.18	-0.58	-0.23
	(0.12)	(0.19)	(0.11)	(0.10)	(0.28)	(0.25)	(0.42)	(0.33)
Strong BB·Elec·Q3	0.56				0.45**			
	(0.35)				(0.22)			
Strong BB·Elec·Q4	0.10				**-0.25**			
	(0.11)				(0.22)			
Strong BB·Elec·Q1	-0.61**				-0.03			
	(0.29)				(0.56)			
Strong BB·Elec·Q2	-0.39**				-0.01			
	(0.16)				(0.53)			
Pres·Elec·Q3		0.73*				0.75*		
		(0.43)				(0.41)		
Pres·Elec·Q4		-0.30				**-0.52**		
		(0.34)				(0.33)		
Pres·Elec·Q1		-0.43				**-1.08***		
		(0.34)				(0.57)		
Pres·Elec·Q2		-0.45				-0.62		
		(0.34)				(0.56)		
Aligned·Elec·Q3			0.32				0.64**	
			(0.46)				(0.30)	
Aligned·Elec·Q4			0.20***				-0.05	
			(0.07)				(0.21)	
Aligned·Elec·Q1			-0.12				0.05	
			(0.41)				(0.33)	
Aligned·Elec·Q2			-0.15				-0.02	
			(0.10)				(0.22)	
Turnover·Elec·Q3				0.26				0.96***
				(0.23)				(0.31)
Turnover·Elec·Q4				-0.00				0.18
				(0.16)				(0.29)
Turnover·Elec·Q1				-0.48**				-0.38
				(0.24)				(0.33)
Turnover·Elec·Q2				-0.23**				-0.51*
				(0.11)				(0.26)
N	177431	177431	177431	177431	177431	177431	177431	177431
R^2	0.82	0.82	0.82	0.82	0.34	0.34	0.34	0.34
County· quarter	Y	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y	Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y	Y	Y

Based on sample of 1759 counties in 49 states. Dependent variable is the two quarter log growth rate. The variables Elec·Q3-Elec·Q2 are the estimates for the counties for which the dummy variable of interest is equal to zero. Boldface indicates that the estimate is statistically different from the corresponding estimate for the group where the dummy variable is equal to zero, at the 10% level (that is, there is some evidence of a differential effect). Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 9: Heterogeneity.

election effects are more negative in presidential election years, significantly so for state government employment.

In columns (3) and (7) of Table 9, I consider whether the incumbent state legislature is ideologically aligned with the incumbent governor. When both branches of power are controlled by the same party, it may be substantially easier to implement policies affecting public sector employment growth (Alt and Lowry, 1994; Bjørnskov and Potrafke, 2013).[31] Columns (3) and (6) of Table 9 show the results. The usual pattern shows up for both aligned and unaligned legislatures, and the estimates are usually smaller or more negative for unaligned legislatures. This is consistent with it being more difficult to manipulate when the legislature is not of the same party, although endogeneity is a potential problem, since states that perform poorly may be less likely to be dominated by one party.

Finally, I interact the election variables with a dummy for whether the party of the governor changes. The estimates, in columns (4) and (8) of Table 9, show that the usual pattern arises both when the party changes and when it does not. This is important because it implies the findings are not simply driven by cases where a new governor of the opposition party takes power with a mandate to implement fiscally conservative policies or to undo the policies of the previous administration. The estimates in the post-period are more negative in magnitude when there is turnover, although not statistically different from when there is no turnover.

3.5 Senate elections

One of the principal threats to identification is other elections that occur simultaneously to gubernatorial elections. Federal elections that occur at the same time throughout the nation should be controlled for by the year-quarter fixed effects. There are other elections, however, that take place in some locations but not others and are not controlled for by the various fixed effects. Examples include Senate elections, mayoral elections, city council elections, and other statewide elected offices besides governor. In general it is impossible to include all these elections in my models, though the fact that both state and local government employment exhibit cycles helps to alleviate concerns that the results are driven by local elections. I am, however, able to investigate perhaps the most important of these potentially confounding elections: elections to the US Senate.

The system works as follows. Each state is assigned two senators, with each senator belonging to one of three possible classes. Every even numbered year, one of the classes of senators is up for re-election to a six year term. Thus, each state experiences a Senate

[31] Although, again, there are multiple alternative possibilities. First, alignment also suggests that one party dominates state politics, so re-election incentives may be weaker; second, state legislators are also incumbents with re-election incentives – even if poor economic conditions are detrimental to the incumbent governor's chances, it is not clear that incumbent legislators benefit at the governor's expense; third, governors in some cases are able to circumvent the state legislature through executive actions or vetoes.

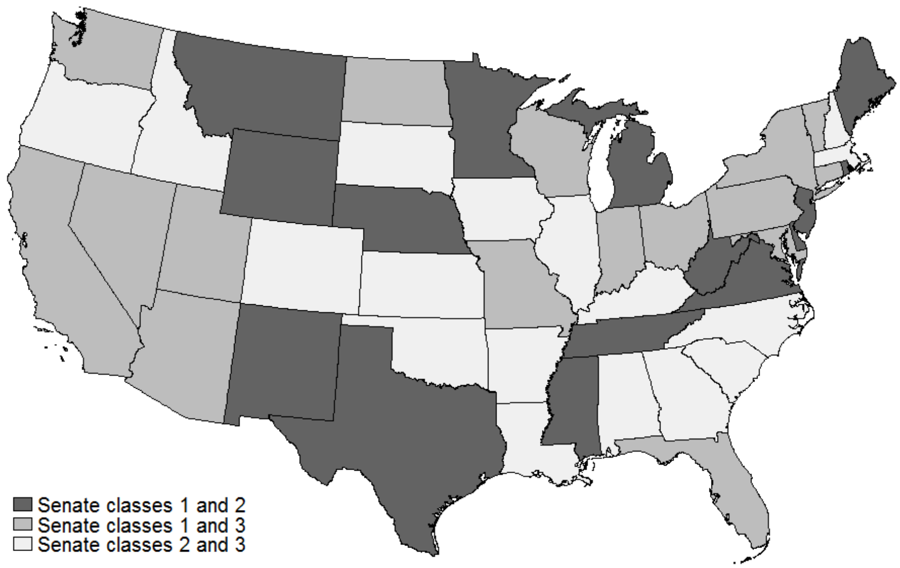

Figure 6: Distribution of US Senate election classes.

election every two out of three even numbered years, and there are sharp differences in Senate election cycles across states and at state borders, shown in Figure 6. There were 450 Senate elections during my period of study.

I add to my models Senate election variables, defined analogously as in the case of gubernatorial elections. The results are displayed in Table 10 – there is not much evidence of employment effects. The exception is the estimate for SenElec·Q3 for local government employment, which is negative and significant, indicating that local government employment growth is lower during between the first and third quarters leading up to a Senate elections. It is not clear what is the source of this effect, though it is not consistent with manipulation. Moreover, local and state government employment do not appear to follow any common pattern, as is the case with gubernatorial elections, nor does the addition of the Senate variables affect conclusions with respect to gubernatorial elections. It is not surprising that Senate elections do not appear to impact government employment to the same extent since a senator's work is federal in nature and does not entail direct authority over state policy levers.

4 Further robustness checks

To further investigate the robustness of the results, I ran a number of sensitivity tests, alternative specifications, and placebo tests. I provide a brief overview here – the details are reported in the Appendix.

First, one potential concern with the main specifications is serial correlation in the growth rate of employment over time. I ran specifications including the one year lagged

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local govt.		State govt.		Local govt.		State govt.	
Elec·Q2	0.17*		0.33		0.23**		0.38	
	(0.09)		(0.31)		(0.11)		(0.37)	
Elec·Q3	0.35*		0.59***		0.53**		0.62***	
	(0.18)		(0.12)		(0.20)		(0.20)	
Elec·Q4	0.14***	0.14***	0.08	0.07	-0.08	-0.08	-0.11	-0.12
	(0.04)	(0.04)	(0.14)	(0.14)	(0.11)	(0.11)	(0.18)	(0.18)
Elec·Q1	-0.33*	-0.33*	-0.19	-0.20	-0.48*	-0.49*	-0.59*	-0.60*
	(0.17)	(0.17)	(0.32)	(0.32)	(0.27)	(0.28)	(0.32)	(0.32)
Elec·Q2		-0.20**		-0.32		-0.18		-0.72**
		(0.09)		(0.26)		(0.16)		(0.28)
Elec·Q3		-0.14		-0.75**		-0.13		-0.84***
		(0.12)		(0.33)		(0.11)		(0.27)
SenElec·Q2	-0.12		0.36		-0.00		0.31	
	(0.10)		(0.38)		(0.18)		(0.54)	
SenElec·Q3	-0.37**		0.12		-0.47*		0.10	
	(0.14)		(0.20)		(0.27)		(0.26)	
SenElec·Q4	0.00	0.00	-0.11	-0.12	0.14	0.14	-0.04	-0.04
	(0.10)	(0.10)	(0.26)	(0.26)	(0.12)	(0.12)	(0.33)	(0.33)
SenElec·Q1	0.17	0.17	-0.18	-0.18	0.02	0.02	0.08	0.08
	(0.20)	(0.20)	(0.40)	(0.40)	(0.34)	(0.34)	(0.36)	(0.36)
SenElec·Q2		-0.02		-0.19		-0.41		-0.27
		(0.14)		(0.32)		(0.26)		(0.44)
SenElec·Q3		-0.12		0.03		-0.30		-0.38
		(0.21)		(0.29)		(0.31)		(0.39)
N	177431	177431	177431	177431	87466	87466	87466	87466
R^2	0.82	0.82	0.34	0.34	0.91	0.91	0.68	0.68
County· quarter	Y	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y				
Pair·year·quarter					Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y	Y	Y

Columns (1)-(4) based on sample of 1759 counties in 49 states; columns (5)-(8) based on sample of 504 counties, forming 433 county-pairs, along 72 border segments, in 43 states. Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level in the fixed effects regressions and, in the county-pair regressions, additionally at the border segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 10: Fixed effects and county-pair results for Senate elections.

growth rate or level, which should cause minimal bias due to the long panel, and the results were largely similar. The only exception is local government employment in the fixed effects approach, where the estimates in the pre-period are no longer significant. The post-period decrease remains significant, however, and the results for state government employment are not affected.

Throughout the analysis, I have used the two quarter growth rate to capture cumulative effects over the period leading up to or following an election. Since this implies overlap between consecutive two quarter periods, I also tried restricting the sample to quarters six months apart to eliminate the overlap, with very similar estimates and unchanged inferences. I also investigated using the one quarter growth rate, and the pattern of positive coefficients in the period leading up to the election season, and negative coefficients afterwards, persists. The main difference is that, for local government employment, the estimates for the two quarters before the election are attenuated (as expected) and fall short of statistical significance, though they are significant both jointly and in sum. The results for the post-period and for state government employment are consistent with the main results.

I also ran specifications that: include contemporaneous values of the control variables rather than time trends for different kinds of counties; or, in the county-pair specifications, weight counties by the inverse of the number of pairs, because a county with more pairs implicitly receives more weight in the regression; or, exclude states with gubernatorial elections in odd years, since the comparison between states with elections two years apart may not be the same as the comparison between states with elections one or three years apart. The latter test is also important because, as explained in the Appendix, it helps alleviate concerns that I am just picking up poll workers hired to work on election day (among several other compelling reasons this is unlikely the case).

I implemented two placebo tests in which I randomly assign gubernatorial elections to state-years or election cycles to states. Running my main specifications repeatedly using the fake election dates allows me to estimate an empirical distribution of the coefficients under the null hypothesis that elections do not influence employment growth, against which the estimates based on the true election dates can be compared. The tests do not yield any surprises – my results would be very unlikely to arise by chance.

Finally, I investigated a number of alternative samples. First, I conducted two jackknife analyses following Foremny and Riedel (2014): I sequentially excluded each of nine census divisions and each of six time periods to see whether the results might be driven by shocks to a few specific states or years. The results suggest this is not the case. Second, I investigated local government employment on the larger sample of counties for which I do not require that state government employment not be suppressed. The results are consistent with the main sample, though in the county-pair approach the post-period estimates are less precise and fall short of statistical significance – the inclusion of these mainly small, rural counties appears to introduce a fair amount of noise. Third, I investigated a smaller sample that is more strict in excluding outliers than the main sample. The usual pattern reappears, and becomes even more pronounced. This suggests that outliers are not driving the results – rather, they tend to add noise, making it more difficult to pick up effects.

5 Conclusion

I investigate electoral cycles in local and state government employment around US gubernatorial elections. Exploiting staggered election timing across states, I use both traditional fixed effects models and a geographic discontinuity design that zooms in on county-pairs straddling state borders. I find that counties in states that experience a gubernatorial election, compared to counties in states that do not, see higher growth in public sector employment leading up to elections, and lower growth afterwards. This pattern is consistent with policy manipulation that could occur through, returning to the allegations against Dannel Malloy and Rick Scott, delaying a hiring freeze until after an election, or more restrained use of veto powers with elections approaching. Several additional analyses support the link to manipulation – there is little strong evidence of cycles in the private sector, over which the governor has less influence, or in the case of Senate elections, which makes sense as senators work in the federal legislature and do not have direct power over state policy. The estimated effects are economically substantial in magnitude and stand up to a range of placebo and robustness checks.

These results stand in contrast to early studies that did not find electoral cycles in real economic outcomes such as employment in the United States (Besley and Case, 2003), and highlight the importance of considering short time horizons to pick up differential effects in the pre- and post-electoral periods (Akhmedov and Zhuravskaya, 2004). Given the attention paid to manipulation in developing countries (Labonne, 2016), showing evidence of electoral cycles in advanced economies, in particular such an economic giant as the United States, is important, and suggests that electoral cycles are widespread, economic and institutional development notwithstanding.

The approach I follow is national in scope and covers a long time period, which is essential to uncover broad patterns in a comprehensive statistical framework. The drawback is that disaggregating further to study in more detail the channels through which governors might influence public sector employment is difficult. Future research should delve into the specifics of which policies are most often implemented or timed with the electoral cycle in mind, how this process plays out in practice in the legislative and executive branches, and which government employees are particularly impacted.

A Appendix

A.1 Alternative specifications

One potential concern with my main specifications is serial correlation in the dependent variable over time. The main reason I do not use lagged dependent variables on the right-hand side in my main specifications is because the lagged value will soak up any manipulation that occurred prior to the time of the lag. If governors adjust the timing of certain policies or actions in anticipation of an election long in advance, then this manipulation would affect the current growth rate through the lagged growth rate rather than through the election dummies. The second reason for not including lagged dependent variables is that doing so in the presence of fixed effects gives rise to biased estimates (Nickell, 1981). However, as noted by Akhmedov and Zhuravskaya (2004) and Labonne (2016), the Nickell bias disappears at the rate $1/T$. Since my panel is quite long, with 103 quarters, these asymptotics should come into play. Therefore, in Table A5 I consider specifications that include lags. Columns (1)-(4) correspond to the fixed effects approach, while the results for the county-pair approach are in columns (5)-(8). Odd numbered columns include the one year lag of the dependent variable, the growth rate, on the right hand side, while even numbered columns include the lagged level of employment per capita. The pattern is similar to that observed with no lags included, with the exception of the pre-electoral period for local government in the fixed effects model. Here, the coefficient estimates for Elec·Q3 and Elec·Q4 in the top panel are somewhat attenuated and no longer attain statistical significance, though they are still positive and the point estimates follow the usual pattern. Moreover, in the post-electoral period we continue to observe negative, significant estimates.

In the specifications considered prior to this point, I avoided including contemporaneous values of the control variables out of endogeneity concerns. Columns (1) and (5) of Table A2 and columns (1) and (6) of Table A3 show that inferences do not change when the contemporaneous values of the log income per capita, log population, and fraction of goods producing employment in total employment are included instead of the initial values interacted with linear and quadratic time trends.

Another issue is that, given the choice of the two quarter growth rate as the dependent variable, there is overlap in consecutive two quarter periods (see Section 2.2). To make sure this does not affect the results, I restrict the sample to quarters that are six months apart to eliminate this overlap. The results are shown in columns (3)-(4) and (7)-(8) of Table A2 and columns (3)-(4) and (8)-(9) of Table A3. The estimates are very similar to those obtained previously and inferences are unaffected, which is not surprising given the inclusion of year-quarter and county-quarter fixed effects in the main specifications. In Table A4, I show the estimates using one quarter growth rates. The usual pattern of positive coefficients in the pre-electoral period, and negative coefficients in the post-period, continues to be observed for the most part. While the coefficients remain statistically significant for state government employment, the pre-electoral period estimates for local government employment fall short of statistical significance. However, the sum of the coefficient estimates for Elec·Q2 and Elec·Q3 is statistically significant (p-values of 0.065 and 0.016 in the fixed effects and county-pairs specifications), and F-tests of joint significance are highly significant in the county-pair approach and just short of 10% significance in the fixed effects approach (p-values 0.025 and 0.121). Moreover, we still observe negative, significant estimates in the post-period.

In the county-pair analysis, I have allowed a county to appear in the regression as many times as it can be paired with a contiguous county in the neighboring state. This means that counties with more pairs implicitly receive greater weight in the regressions. I check the robustness of the results to weighting each county by the inverse of the number of pairs so that each county receives equal weight. Columns (5) and (10) of Table A3 show that the results are similar to before.

A.2 Differences in election cycles and poll workers

I have assumed implicitly that states experiencing an election are comparable to any states that are not experiencing an election, regardless of how long ago these control states held their most recent election. I have included in most of my specifications dummy variables for which of the possible electoral cycles (presidential years, midterm years, off-years) a state has, interacted with linear and quadratic time trends, to control for different long term trajectories across these groups of states. However, given the periodic nature of electoral cycles, the comparison between states with elections two years apart may not be the same as the comparison between states with elections one or three years apart.

I therefore run specifications that drop counties in states that have elections in off years (i.e., odd numbered years), so that I am only comparing counties in states that have gubernatorial elections two years apart. These results are shown in columns (2) and (6) of Table A2 and columns (2) and (7) of Table A3. Even excluding these counties, which account for a substantial share of the identifying variation, the signature pattern persists: counties that experience an election experience higher government employment growth in the pre-period and lower growth in the post-period than counties that do not. Only in the case of the county-pair specification for state government employment is the coefficient somewhat attenuated and no longer significant in the pre-electoral period, though the post-electoral dip is pronounced.

There is a second reason excluding counties in states with off-year gubernatorial elections is an important robustness check: we may simply be observing increases in government employment due to the hiring of poll workers to supervise polling stations on election day, rather than any kind of political manipulation. There are several reasons this is unlikely to be the case. First, services performed by election workers are excluded from unemployment insurance coverage if the worker makes less than $1,000 per year.[32] Given prevailing compensation rates,[33] the vast majority of workers should fall below this threshold.[34] Second, election workers are typically only compensated for their work on election day and, in many cases, a training session usually held a few weeks before the election. Thus, this employment would show up mainly in November, or October for some training sessions, both of which are contained in the fourth quarter. In many cases, however, the observed effects are consistent with increases or decreases in other parts of the year. Third, most poll workers are hired by local governments, in particular counties, rather than state governments, but effects are observed for both categories.[35] Finally, although there is variation across states in which ones experience gubernatorial elections in a given year, all federal and state level offices up for election generally appear on a single ballot. Thus, gubernatorial elections that coincide with federal elections (midterm or presidential) are unlikely to entail significantly more election workers over and above those involved in manning the federal elections. The exception would be states that have elections in off-years, but as described above, the results are robust to their exclusion.

A.3 Placebo and jackknife tests

I implement two placebo tests. First, I randomize whether each state-year is a gubernatorial election year. With probability 0.25, a state-year is assigned an election. Using these fake election dates, I then run the preferred specification (using a July-June window for the sake of presentation). Since the fake elections are randomly assigned, we should not observe any effects. The columns in Table A1 corresponding to "Test 1" summarize the frequency that certain coefficient estimates are obtained in 1000 iterations, for both fixed effects and county-pair models and for both local and state government employment. "Elec·Q3 or Elec·Q4 more significant" means that the t-statistic for at least one of these quarters exceeds the larger of the t-statistics for the true election dates (a conservative requirement, since for the true election dates multiple estimates may be significant). These numbers are informative about how unlikely it would be to observe findings similar to my main results if elections had no effect on employment.

[32] Title 26, United States Code §3309(b)(3)(F) (2010), regulates under the Federal Unemployment Tax Act (FUTA) coverage of state unemployment insurance laws, and implies that establishments do not include exempted employees in their quarterly contributions reports.

[33] E.g., in San Diego County in the 2016 general election, workers could make between $75 and $175, depending on the position, for work on election day (see `sdvote.com/pollworkers/`).

[34] There is also some higher level election administration, usually carried out by permanent county officials, e.g., the county clerk, as only one component of their duties. In some states, there is a county board of elections or election comission, consisting of members elected or appointed to a fixed, continuous, term. Similarly at the state level, usually those tasked with election administration are permanent employees. Even if such officials work more around elections, their increased hours would show up in their compensation rather than in the number of employees.

[35] National Conference of State Legislatures (2016), `http://www.ncsl.org/research/elections-and-campaigns/election-administration-at-state-and-local-levels.aspx`.

	# of times observed							
	Local govt.				State govt.			
	Test 1		Test 2		Test 1		Test 2	
Regression outcome	FE	BP	FE	BP	FE	BP	FE	BP
Elec·Q3 or Elec·Q4 more significant	4	69	6	52	0	19	0	15
Elec·Q1 or Elec·Q2 more significant	55	294	45	274	394	60	358	63
Both of the above	1	41	1	14	0	1	0	1

Based on 1000 iterations, using preferred specifications and a July-June window. Test 1 assigns each state-year a gubernatorial election with probability 0.25. Test 2 assigns each state a four year electoral cycle with probability one quarter each. "Elec·Q3 or Elec·Q4 more significant" means that at least one of these estimates has a larger t-statistic (in absolute value) than the larger of the corresponding t-statistics for the true election dates. For the fixed effects approach, the relevant t-statistics are 3.29 and -2.34 for local government, 4.95 and -1.20 for state government; for the county-pair approach they are 2.58 and -1.74 for local government, 3.13 and -2.52 for state government.

Table A1: Placebo tests.

The second placebo test, instead of randomly assigning elections to state-years, randomly assigns each state one of the four possible four year election cycles. The last two columns of Tables A1 under "Test 2" tabulate the frequency of different estimates in 1000 iterations for both employment categories and both methodologies. For both placebo tests, the results indicate that it would be very unlikely to obtain my results under the null that elections do not affect employment.

Another possibility is that the estimates are driven by a handful of extreme state level shocks that coincidentally occurred around elections. Following Foremny and Riedel (2014), I perform a jackknife analysis to test the sensitivity of the results to the exclusion of a handful of states at a time. I sequentially exclude each of the nine census divisions. Tables A6 and A7 show the results for local and state government employment.[36] In each case, the usual pattern appears, and in only a few do some coefficients lose significance. Similarly, I divide my sample period into six consecutive periods of 16-17 quarters in length and sequentially exclude each one. The results are displayed in Tables A8 and A9, and are largely unchanged. The biggest difference is that excluding the last period appears to affect the timing of the observed effects – it is shifted forward by about a quarter. This suggests that, during this last period (the fourth quarter of 2011 through the third quarter of 2015), employment effects tended to show up a bit closer to the elections than in other periods.

[36]For the fixed effects sample only – the county-pair sample is already relatively small without losing additional observations, which is exacerbated by, on top of the omitted counties, the loss of counties that border on the omitted census division, since we need both counties in a pair for the pair-year-quarter effects to be identified.

A.4 Alternative samples

In my main samples, I restricted to counties that do not have suppressed data for both local and state government employment. There are less state government employees than local in general, and the majority of the loss comes from counties with suppressed state data. Thus, I restrict attention to electoral cycles in local government employment and relax the requirement that state data not be suppressed. Doing so allows me to increase the number of counties from 1759 to 2102 in the fixed effects sample, and from 504 to 643 (433 to 580 pairs) in the county-pair sample. The expanded samples are shown in Figures A1 and A2. In particular, I recover a substantial number of the counties in Illinois and Michigan, which were largely missing from the main sample.

Columns (1) and (2) of Table A10 shows the results for local government employment on the larger sample for both approaches. The coefficient estimates follow the same pattern observed in previous regressions: positive coefficients in the pre-electoral period and negative coefficients in the post-period. In the pre-electoral period, the estimates are very similar to before, and highly statistically significant. In the post-electoral period, the estimates are all negative, as before, though they are somewhat attenuated towards zero and attain lower levels of statistical significance. The standard errors in few cases are substantially larger than before, which suggests that the additional counties have added a lot of noise. This is not surprising when we remember that the new counties are those with suppressed state employment, which are usually smaller, more rural, and have lower baseline levels of local government employment.

Even though my main sample excludes counties whose maximum (minimum) change in quarter-to-quarter employment per capita is in the top or bottom 1%, some very large values of the dependent variable remain. In multiple instances, the number of employees per capita more than doubled or halved over two quarters. Though in some cases this could be due to administrative changes in industry classifications, I am hesitant to exclude these counties from the main sample as often these are real changes and not necessarily even extreme – imagine, say, the closure of a large state prison that accounts for most of a county's state government employment. In any case, to ensure the results are robust to such outliers, I exclude counties whose maximum value of Y_{cst}/Y_{cst-2} is greater than 2 or less than 0.5 for either local or state government employment. This leaves 1520 counties in 49 states for the fixed effects sample and 417 counties (339 pairs) for the county-pair sample. The results are shown in columns (3)-(6) of Table A10. The usual pattern persists and, in fact, becomes even more pronounced. The estimates are larger in magnitude for local government and attain higher levels of significance, especially in the post-period. For state government, now the estimates for Elec·Q2, previously insignificant, also turn out to be significant, even more suggestive of manipulation. These results suggest that outliers are not driving the results – if anything, they tend to add noise and work against finding effects.

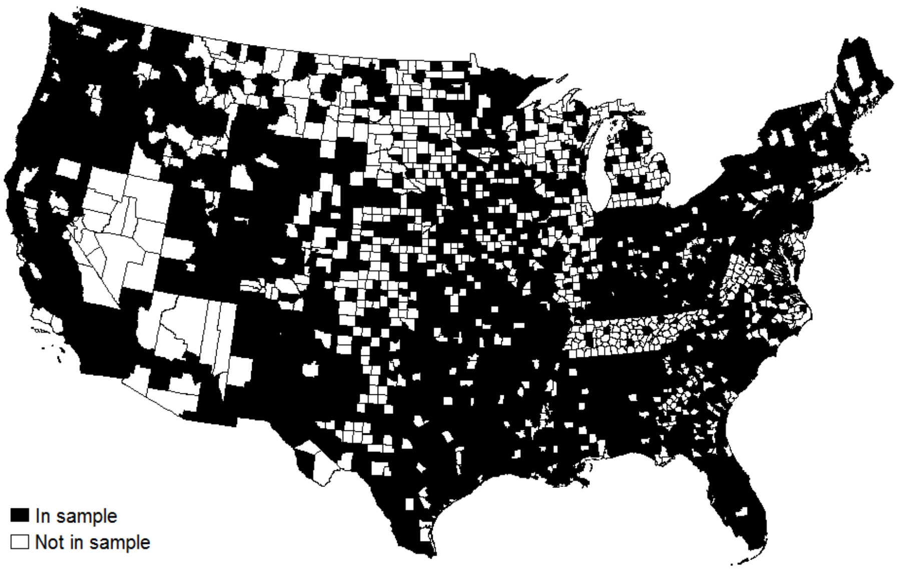

Figure A1: Alternative sample for the fixed effects specifications, based on all counties for which only local government is required to be available.

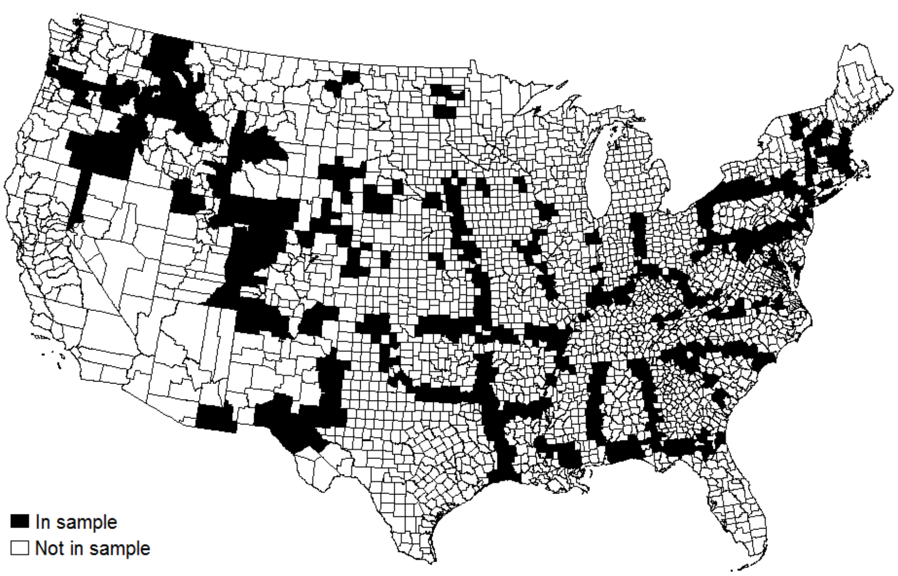

Figure A2: Alternative sample for the county-pairs specifications, based on all counties for which only local government is required to be available.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local govt.				State govt.			
Apr-Mar window								
Elec·Q2	0.17*	0.03	0.17**		0.32	0.62*	0.32	
	(0.09)	(0.11)	(0.08)		(0.31)	(0.36)	(0.31)	
Elec·Q3	0.35*	0.50*		0.35*	0.59***	0.55***		0.58***
	(0.19)	(0.27)		(0.18)	(0.12)	(0.18)		(0.12)
Elec·Q4	0.14***	0.15***	0.16***		0.08	-0.15	0.09	
	(0.04)	(0.05)	(0.04)		(0.15)	(0.15)	(0.15)	
Elec·Q1	-0.33*	-0.40		-0.34*	-0.19	-0.63		-0.20
	(0.17)	(0.29)		(0.17)	(0.33)	(0.40)		(0.33)
Oct-Sep window								
Elec·Q4	0.14***	0.15***	0.15***		0.08	-0.15	0.09	
	(0.04)	(0.05)	(0.04)		(0.15)	(0.15)	(0.15)	
Elec·Q1	-0.33*	-0.41		-0.34*	-0.19	-0.63		-0.20
	(0.17)	(0.30)		(0.17)	(0.33)	(0.40)		(0.33)
Elec·Q2	-0.20**	-0.12	-0.19**		-0.32	-0.39	-0.31	
	(0.09)	(0.13)	(0.09)		(0.26)	(0.36)	(0.26)	
Elec·Q3	-0.14	-0.09		-0.15	-0.75**	-0.41		-0.76**
	(0.12)	(0.18)		(0.12)	(0.33)	(0.30)		(0.33)
N	177431	153292	87836	89595	177431	153292	87836	89595
R^2	0.82	0.83	0.22	0.85	0.34	0.34	0.18	0.43
County· quarter	Y	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y	Y	Y	Y	Y
Controls·trends		Y	Y		Y	Y	Y	Y
Contemporaneous controls	Y				Y			
Drop odd years		Y				Y		
Two quarters			Y	Y			Y	Y

Based on sample of 1759 counties in 49 states. Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table A2: Alternative specifications in the fixed effects approach.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Local govt.					State govt.		
Apr-Mar window										
Elec·Q2	0.23*	0.01	0.24**		0.23**	0.37	0.49	0.37		0.38
	(0.11)	(0.15)	(0.11)		(0.11)	(0.36)	(0.40)	(0.37)		(0.36)
Elec·Q3	0.54**	0.63**		0.53**	0.54**	0.62***	0.22		0.62***	0.62***
	(0.21)	(0.28)		(0.21)	(0.21)	(0.20)	(0.20)		(0.20)	(0.20)
Elec·Q4	-0.09	0.05	-0.08		-0.08	-0.12	-0.28	-0.11		-0.11
	(0.11)	(0.13)	(0.11)		(0.11)	(0.18)	(0.22)	(0.18)		(0.18)
Elec·Q1	-0.49*	-0.46		-0.49*	-0.48*	-0.60*	-1.07***		-0.59*	-0.59*
	(0.28)	(0.32)		(0.28)	(0.28)	(0.32)	(0.31)		(0.32)	(0.32)
Oct-Sep window										
Elec·Q4	-0.09	0.04	-0.08		-0.09	-0.12	-0.28	-0.12		-0.12
	(0.11)	(0.13)	(0.11)		(0.11)	(0.18)	(0.22)	(0.18)		(0.18)
Elec·Q1	-0.49*	-0.46		-0.50*	-0.49*	-0.60*	-1.08***		-0.60*	-0.60*
	(0.28)	(0.32)		(0.28)	(0.28)	(0.32)	(0.31)		(0.32)	(0.32)
Elec·Q2	-0.17	-0.01	-0.17		-0.17	-0.71**	-0.75*	-0.71**		-0.71**
	(0.16)	(0.18)	(0.16)		(0.16)	(0.28)	(0.40)	(0.28)		(0.28)
Elec·Q3	-0.12	-0.11		-0.13	-0.12	-0.83***	-0.24		-0.83***	-0.83***
	(0.11)	(0.15)		(0.11)	(0.11)	(0.27)	(0.27)		(0.27)	(0.27)
N	87466	64640	43300	44166	87466	87466	64640	43300	44166	87466
R^2	0.91	0.92	0.61	0.93	0.91	0.68	0.68	0.59	0.73	0.68
County· quarter	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Pair·year·quarter	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls·trends		Y	Y	Y		Y	Y	Y	Y	Y
Contemporaneous controls	Y					Y				
Drop odd years		Y					Y			
Two quarters			Y	Y				Y	Y	
Weight by duplicates					Y					Y

Based on sample of 504 counties, forming 433 county-pairs, along 72 border segments, in 43 states. Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) two way clustered at the state and border segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table A3: Alternative specifications in the county-pair approach.

	(1) Local	(2) State	(3) Local	(4) State
Apr-Mar window				
Elec·Q2	-0.01	0.36***	0.17	0.53***
	(0.07)	(0.10)	(0.11)	(0.16)
Elec·Q3	0.36	0.23**	0.35	0.09
	(0.22)	(0.09)	(0.23)	(0.17)
Elec·Q4	-0.27	-0.17	-0.44*	-0.20
	(0.20)	(0.16)	(0.24)	(0.23)
Elec·Q1	-0.08	-0.02	-0.03	-0.37
	(0.08)	(0.21)	(0.16)	(0.31)
Oct-Sep window				
Elec·Q4	-0.27	-0.17	-0.45*	-0.21
	(0.20)	(0.16)	(0.24)	(0.23)
Elec·Q1	-0.08	-0.02	-0.03	-0.37
	(0.08)	(0.21)	(0.16)	(0.31)
Elec·Q2	-0.13***	-0.33*	-0.17*	-0.32
	(0.04)	(0.18)	(0.10)	(0.21)
Elec·Q3	-0.02	-0.43**	0.03	-0.52**
	(0.09)	(0.17)	(0.14)	(0.21)
N	179190	179190	88332	88332
R^2	0.85	0.36	0.93	0.68
County· quarter	Y	Y	Y	Y
Pair·year·quarter			Y	Y
Year·quarter	Y	Y		
Controls·trends	Y	Y	Y	Y

Columns (1)-(2) based on sample of 1759 counties in 49 states; columns (3)-(4) based on sample of 504 counties, forming 433 county-pairs, along 72 border segments, in 43 states. Dependent variable is the one quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) two way clustered at the state level and, for the county-pair models, additionally at the border segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table A4: Baseline regressions using one quarter log growth rate.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Local govt.		State govt.		Local govt.		State govt.	
Apr-Mar window								
1 yr lagged d.v.	40.12***		7.00**		45.48***		9.68***	
	(4.09)		(2.63)		(3.32)		(3.51)	
1yr lagged level		2.83		-6.51***		4.79**		-5.68***
		(2.01)		(0.63)		(2.17)		(0.50)
Elec·Q2	0.14	0.17*	0.17	0.31	0.28	0.24*	0.28	0.37
	(0.10)	(0.08)	(0.37)	(0.29)	(0.19)	(0.12)	(0.41)	(0.35)
Elec·Q3	0.17	0.22	0.43***	0.45***	0.37*	0.56**	0.53**	0.56**
	(0.18)	(0.27)	(0.14)	(0.14)	(0.20)	(0.24)	(0.23)	(0.24)
Elec·Q4	0.08	0.09	0.05	0.09	-0.33**	-0.17	-0.16	-0.07
	(0.08)	(0.06)	(0.19)	(0.17)	(0.16)	(0.12)	(0.22)	(0.22)
Elec·Q1	-0.26	-0.33*	-0.22	-0.18	-0.41	-0.49*	-0.56*	-0.56*
	(0.16)	(0.17)	(0.41)	(0.31)	(0.26)	(0.28)	(0.33)	(0.32)
Oct-Sep window								
1 yr lagged d.v.	40.14***		7.03**		45.52***		9.73***	
	(4.09)		(2.63)		(3.33)		(3.52)	
1yr lagged level		2.83		-6.51***		4.79**		-5.67***
		(2.01)		(0.63)		(2.17)		(0.50)
Elec·Q4	0.08	0.09	0.05	0.09	-0.33**	-0.17	-0.16	-0.08
	(0.08)	(0.06)	(0.19)	(0.17)	(0.16)	(0.12)	(0.22)	(0.22)
Elec·Q1	-0.26	-0.33*	-0.22	-0.18	-0.41	-0.49*	-0.57*	-0.57*
	(0.16)	(0.17)	(0.41)	(0.31)	(0.26)	(0.28)	(0.33)	(0.32)
Elec·Q2	-0.29***	-0.20**	-0.29	-0.28	-0.23	-0.19	-0.70**	-0.65**
	(0.09)	(0.09)	(0.28)	(0.26)	(0.21)	(0.17)	(0.30)	(0.28)
Elec·Q3	-0.24*	-0.10	-0.77**	-0.67*	-0.32**	-0.10	-0.88***	-0.75***
	(0.14)	(0.12)	(0.35)	(0.35)	(0.15)	(0.12)	(0.28)	(0.25)
N	170167	173685	170167	173685	84002	85734	84002	85734
R^2	0.85	0.82	0.34	0.35 R^2	0.93	0.91	0.68	0.68
County· quarter	Y	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y				
Pair·year·quarter					Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y	Y	Y

Columns (1)-(4) based on sample of 1759 counties in 49 states; columns (5)-(8) on sample of 504 counties, forming 433 county-pairs, along 72 border segments, in 43 states. Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) are clustered at the state level and, for the county-pair models, additionally at the border segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table A5: Lag specifications.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Local government								
Apr-Mar window									
Elec·Q2	0.18**	0.19**	0.22**	0.18**	0.17*	0.07	0.15	0.19**	0.19**
	(0.09)	(0.09)	(0.08)	(0.09)	(0.09)	(0.09)	(0.10)	(0.09)	(0.09)
Elec·Q3	0.35*	0.34*	0.38*	0.34	0.14	0.42*	0.38*	0.42**	0.37*
	(0.19)	(0.19)	(0.21)	(0.22)	(0.10)	(0.22)	(0.20)	(0.19)	(0.20)
Elec·Q4	0.14***	0.14***	0.16***	0.12**	0.11**	0.15***	0.16***	0.14***	0.16***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)
Elec·Q1	-0.32*	-0.38**	-0.35*	-0.33*	-0.17	-0.35	-0.28	-0.39**	-0.36**
	(0.17)	(0.17)	(0.19)	(0.20)	(0.12)	(0.23)	(0.17)	(0.17)	(0.17)
Oct-Sep window									
Elec·Q4	0.14***	0.14***	0.15***	0.12**	0.11**	0.15***	0.16***	0.14***	0.16***
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)
Elec·Q1	-0.32*	-0.38**	-0.35*	-0.33*	-0.17	-0.35	-0.28	-0.40**	-0.36**
	(0.17)	(0.17)	(0.19)	(0.20)	(0.12)	(0.23)	(0.18)	(0.17)	(0.17)
Elec·Q2	-0.20**	-0.25***	-0.19**	-0.19**	-0.20*	-0.16	-0.16*	-0.20**	-0.23***
	(0.09)	(0.08)	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)	(0.09)	(0.08)
Elec·Q3	-0.13	-0.13	-0.12	-0.21**	-0.00	-0.18	-0.16	-0.14	-0.13
	(0.12)	(0.12)	(0.14)	(0.09)	(0.13)	(0.15)	(0.13)	(0.13)	(0.13)
N	173694	165614	160422	146525	143798	156625	141778	163897	167095
R^2	0.81	0.81	0.82	0.81	0.84	0.83	0.81	0.82	0.81
Omitted division	1	2	3	4	5	6	7	8	9
Counties	1722	1642	1590	1453	1426	1553	1406	1625	1655
States	43	46	45	42	41	45	45	41	44
County·quarter	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y	Y	Y	Y

Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

The census divisions are: (1) CT, MA, ME, NH, RI, VT; (2) NJ, NY, PA; (3) IL, IN, MI, OH, WI; (4) IA, KS, MN, MO, ND, NE, SD; (5) DE, FL, GA, MD, NC, SC, VA, WV; (6) AL, KY, MS, TN; (7) AR, LA, OK, TX; (8) AZ, CO, ID, MT, NM, NV, UT, WY; (9) AK, CA, HI, OR, WA.

Table A6: Results of sequentially excluding each of the nine census divisions for local government employment in the fixed effects approach.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	State government								
Apr-Mar window									
Elec·Q2	0.33	0.25	0.10	0.52	0.12	0.63**	0.24	0.36	0.38
	(0.31)	(0.31)	(0.29)	(0.34)	(0.38)	(0.29)	(0.34)	(0.32)	(0.32)
Elec·Q3	0.61***	0.59***	0.52***	0.64***	0.65***	0.52***	0.57***	0.59***	0.59***
	(0.12)	(0.12)	(0.13)	(0.13)	(0.12)	(0.14)	(0.14)	(0.12)	(0.12)
Elec·Q4	0.09	0.08	0.16	0.20	0.04	-0.04	0.07	0.08	0.04
	(0.15)	(0.15)	(0.15)	(0.17)	(0.15)	(0.16)	(0.15)	(0.16)	(0.15)
Elec·Q1	-0.17	-0.15	-0.06	0.07	-0.26	-0.45	-0.36	-0.22	-0.16
	(0.33)	(0.33)	(0.35)	(0.29)	(0.39)	(0.37)	(0.36)	(0.34)	(0.33)
Oct-Sep window									
Elec·Q4	0.09	0.07	0.15	0.19	0.03	-0.05	0.06	0.07	0.03
	(0.15)	(0.15)	(0.15)	(0.17)	(0.15)	(0.16)	(0.15)	(0.16)	(0.15)
Elec·Q1	-0.18	-0.15	-0.07	0.07	-0.27	-0.45	-0.36	-0.22	-0.16
	(0.33)	(0.33)	(0.35)	(0.29)	(0.39)	(0.37)	(0.36)	(0.34)	(0.33)
Elec·Q2	-0.31	-0.23	-0.39	-0.17	-0.36	-0.31	-0.45	-0.37	-0.27
	(0.26)	(0.27)	(0.28)	(0.21)	(0.33)	(0.31)	(0.29)	(0.27)	(0.27)
Elec·Q3	-0.77**	-0.70**	-0.91***	-0.78**	-0.87**	-0.41*	-0.68*	-0.81**	-0.75**
	(0.34)	(0.34)	(0.32)	(0.37)	(0.42)	(0.24)	(0.37)	(0.34)	(0.35)
N	173694	165614	160422	146525	143798	156625	141778	163897	167095
R^2	0.34	0.34	0.30	0.33	0.36	0.34	0.37	0.33	0.33
Omitted division	1	2	3	4	5	6	7	8	9
Counties	1722	1642	1590	1453	1426	1553	1406	1625	1655
States	43	46	45	42	41	45	45	41	44
County·quarter	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y	Y	Y	Y

Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

The census divisions are: (1) CT, MA, ME, NH, RI, VT; (2) NJ, NY, PA; (3) IL, IN, MI, OH, WI; (4) IA, KS, MN, MO, ND, NE, SD; (5) DE, FL, GA, MD, NC, SC, VA, WV; (6) AL, KY, MS, TN; (7) AR, LA, OK, TX; (8) AZ, CO, ID, MT, NM, NV, UT, WY; (9) AK, CA, HI, OR, WA.

Table A7: Results of sequentially excluding each of the nine census divisions for state government in the fixed effects approach.

	(1)	(2)	(3)	(4)	(5)	(6)
			Local government			
Apr-Mar window						
Elec·Q2	0.11	0.12	0.20**	0.21*	0.19**	0.20**
	(0.07)	(0.10)	(0.08)	(0.12)	(0.09)	(0.08)
Elec·Q3l	0.29	0.46**	0.46**	0.33	0.31	0.22**
	(0.35)	(0.22)	(0.19)	(0.22)	(0.21)	(0.10)
Elec·Q4	0.16***	0.19***	0.16***	0.16***	0.15**	0.06
	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)
Elec·Q1	-0.33*	-0.31	-0.55**	-0.32*	-0.37*	-0.15
	(0.19)	(0.21)	(0.21)	(0.18)	(0.21)	(0.11)
Oct-Sep window						
Elec·Q4	0.16***	0.19***	0.15***	0.16***	0.14**	0.06
	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)
Elec·Q1	-0.33*	-0.31	-0.56**	-0.33*	-0.37*	-0.15
	(0.19)	(0.22)	(0.21)	(0.18)	(0.21)	(0.11)
Elec·Q2	-0.18*	-0.28***	-0.15*	-0.25**	-0.17*	-0.18*
	(0.10)	(0.10)	(0.08)	(0.11)	(0.09)	(0.09)
Elec·Q3	-0.08	-0.16	-0.18	-0.09	-0.08	-0.18
	(0.14)	(0.15)	(0.13)	(0.12)	(0.16)	(0.12)
N	147528	147528	147528	147696	147528	149347
R^2	0.82	0.82	0.82	0.82	0.82	0.83
Omitted time period	1	2	3	4	5	6
County·quarter	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y

Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$.
The sample period is divided into 6 approximately equal time periods (starting in January 1992, due to the choice of the dependent variable).

Table A8: Results of sequentially excluding one of six time periods for local government employment in the fixed effects approach.

	(1)	(2)	(3)	(4)	(5)	(6)
			State government			
Apr-Mar window						
Elec·Q2	0.09	0.33	0.55*	0.11	0.50	0.35
	(0.38)	(0.34)	(0.29)	(0.32)	(0.31)	(0.33)
Elec·Q3	0.37**	0.69***	0.60***	0.53***	0.64***	0.69***
	(0.16)	(0.14)	(0.16)	(0.13)	(0.15)	(0.15)
Elec·Q4	0.14	0.15	0.11	-0.03	0.16	-0.05
	(0.19)	(0.15)	(0.15)	(0.16)	(0.16)	(0.16)
Elec·Q1	0.03	-0.36	-0.04	-0.13	-0.24	-0.39
	(0.40)	(0.33)	(0.31)	(0.32)	(0.36)	(0.39)
Oct-Sep window						
Elec·Q4	0.14	0.15	0.10	-0.03	0.15	-0.06
	(0.19)	(0.15)	(0.15)	(0.16)	(0.16)	(0.16)
Elec·Q1	0.03	-0.36	-0.05	-0.14	-0.25	-0.40
	(0.40)	(0.33)	(0.31)	(0.32)	(0.37)	(0.39)
Elec·Q2	-0.23	-0.51*	-0.17	-0.24	-0.36	-0.41
	(0.32)	(0.30)	(0.22)	(0.26)	(0.31)	(0.35)
Elec·Q3	-0.70**	-0.96**	-0.53	-0.71*	-0.76**	-0.86**
	(0.29)	(0.38)	(0.41)	(0.41)	(0.30)	(0.39)
N	147528	147528	147528	147696	147528	149347
R^2	0.34	0.34	0.33	0.34	0.35	0.36
Omitted time period	1	2	3	4	5	
County·quarter	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y

Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$.
The sample period is divided into 6 approximately equal time periods (starting in January 1992, due to the choice of the dependent variable).

Table A9: Results of sequentially excluding one of six time periods for state government employment in the fixed effects approach.

	(1)	(2)	(3)	(4)	(5)	(6)
	Local		Local	State	Local	State
Apr-Mar window						
Elec·Q2	0.16**	0.26**	0.16*	0.47*	0.29***	0.55**
	(0.08)	(0.11)	(0.09)	(0.25)	(0.10)	(0.26)
Elec·Q3	0.36**	0.46**	0.40**	0.62***	0.69***	0.69***
	(0.17)	(0.18)	(0.20)	(0.12)	(0.23)	(0.20)
Elec·Q4	0.18***	-0.03	0.14***	0.09	-0.06	-0.00
	(0.05)	(0.11)	(0.05)	(0.14)	(0.12)	(0.15)
Elec·Q1	-0.24	-0.11	-0.40**	-0.14	-0.71**	-0.41*
	(0.16)	(0.36)	(0.17)	(0.28)	(0.27)	(0.24)
Oct-Sep window						
Elec·Q4	0.18***	-0.03	0.14***	0.08	-0.06	-0.01
	(0.05)	(0.11)	(0.05)	(0.14)	(0.12)	(0.15)
Elec·Q1	-0.24	-0.11	-0.40**	-0.14	-0.71**	-0.42*
	(0.16)	(0.36)	(0.17)	(0.28)	(0.27)	(0.24)
Elec·Q2	-0.15*	-0.01	-0.20**	-0.27	-0.30**	-0.58**
	(0.08)	(0.27)	(0.08)	(0.24)	(0.13)	(0.29)
Elec·Q3	-0.18	-0.17	-0.10	-0.75**	-0.13	-0.89***
	(0.12)	(0.12)	(0.12)	(0.32)	(0.12)	(0.25)
N	212006	117160	153372	153372	68478	68478
R^2	0.81	0.90	0.83	0.39	0.92	0.67
County· quarter	Y	Y	Y	Y	Y	Y
Year·quarter	Y		Y	Y		
Pair·year·quarter		Y			Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y
Larger sample	Y	Y				
Dropped outliers			Y	Y	Y	Y

Column (1) based on sample of 2102 counties in 50 states; column (2) on 643 counties, forming 580 county-pairs, along 80 border segments, in 45 states; columns (3)-(4) based on sample of 1520 counties in 49 states; columns (5)-(6) on sample of 417 counties, forming 339 county-pairs, along 73 border segments, in 45 states. Dependent variable is the two quarter log growth rate. Coefficients and standard errors multiplied by 100. Standard errors (in parentheses) are clustered at the state level for the fixed effects models, and additionally at the border segment level for county-pair models. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table A10: Results for local government on larger sample that does not require state government not be suppressed (columns 1-2), and robustness to outliers (columns 3-6).

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