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Essays in Political Economy

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by

Dodge Tyler Cahan

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2019

The dissertation of Dodge Tyler Cahan is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California San Diego

2019

DEDICATION

To my parents.

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ABSTRACT OF THE DISSERTATION

Essays in Political Economy

by

Dodge Tyler Cahan

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Professor Julie Cullen, Chair

This dissertation examines three topics in political economy. Chapter 1 studies electoral cycles in public sector employment around US gubernatorial elections. Chapter 2 investigates the well-known fact that the US economy grew faster during Democratic presidencies and to what extent this phenomenon generalizes with respect to state-level elected offices. Chapter 3 studies how the use of best-worst voting rules influences the strategic position-taking behavior of political candidates in a spatial election model.

Chapter 1

Electoral cycles in government employment: Evidence from US gubernatorial elections

Abstract: Incumbents may opportunistically design policies increasing employment before elections or postpone cuts until afterwards. I investigate electoral cycles in public sector employment around US gubernatorial elections. Exploiting staggered gubernatorial election cycles across states, I use both county fixed effects models and a geographic discontinuity design that compares neighboring counties at state borders with a difference in gubernatorial election cycles. Consistent with manipulation, state and local government employment per capita are higher leading up to elections; afterwards, employment abruptly returns to normal. Political and spatial heterogeneities are investigated, including by election competitiveness, term limits, incumbent party affiliation, and ideological alignment between the incumbent and the state legislature or local citizens. Differences across types of government employment, and private sector employment, are also explored.

Keywords: electoral cycles; government employment; state borders; border discontinuity design; gubernatorial elections.

1.1 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 are referred to broadly as "manipulation" (Nordhaus, 1975). Since economic conditions play an important role when voters form their opinions of politicians (Fair, 1981; Erikson, 1989; Klomp and de Haan, 2013), 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 the economy 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; 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. Artificial fluctuations in government employment have the potential to affect the provision of local public services, induce wasteful spending, and crowd out more productive investments.

Anecdotal evidence of manipulation by governors abounds. During his 2014 re-election campaign, Connecticut governor Dannel Malloy dismissed nonpartisan reports of a growing budget deficit; two weeks after winning re-election, he announced a statewide freeze on hiring 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, "sprinkle lists") aimed at providing additional or first time funding for various projects and programs. At the same time, he vetoed $69 million out of $77 billion in the overall budget. The following year, a non-election year, he vetoed a much larger $145 million out of $301 million in supplemental funding and $461 million out of $78 billion in the overall budget. Victims of the cuts included $1.5 million in funding for a storm risk center at Florida State University, and $15 million for a new campus of the University of Central Florida in Orlando. "There is just no consistency this year," said State Senate Majority Leader Bill Galvano of the discrepancy.[2]

I provide empirical evidence that electoral cycles in public sector employment around US gubernatorial elections are more than anecdotal, and their nature is consistent with the manipulation hypothesis. Gubernatorial elections in the US offer several empirical advantages – they are staggered and follow historically fixed schedules, allowing me to exploit differences in election cycles across states. I use both traditional county 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 prevailing economic conditions at a very local level. Moreover, rather than using annual data, which may obfuscate short term effects

[1] For an account, see http://www.courant.com/politics/hc-malloy-hiring-freeze-state-agencies-20141113-story.html (accessed 3/6/17).

[2] See http://www.politico.com/story/2015/06/rick-scott-vetoes-include-election-year-projects-119443 (accessed 3/6/17) or http://www.tampabay.com/news/politics/stateroundup/gov-rick-scott-signs-state-budget-in-private-with-little-notice/2234704 (accessed 3/6/17).

such as differential outcomes before and after elections (Akhmedov and Zhuravskaya, 2004; Labonne, 2016), I use quarterly data that are broad in geographic coverage and span a 27 year period.

In the period leading up to and including the election quarter, state government employment is higher by up to about 7 state government employees per 100 thousand capita. By one or two quarters following the election, employment per capita returns to normal levels. This is precisely what we might expect if incumbent governors attempted to increase state government employment before an election or allowed it to build up by delaying any unpopular employment reducing decisions until afterwards. These findings are confirmed by both the fixed effects and the county-pair approaches, and hold up to a wide range of robustness and placebo tests. Given the average state population is around 6.5 million (similar to that of, say, Indiana), the magnitude of the effect could be substantial relative to other election related costs and, moreover, it is likely to be politically important – in the heated media environment leading up to an election, negative stories can damage a candidate's chances (Ferraz and Finan, 2008; Soroka et al., 2015).

Local government employment also exhibits electoral cycles, increasing by up to about 13 employees per 100 thousand in the quarters before an election and, like state government employment, abruptly returning to normal levels afterwards. This is quite surprising given that local government employment is more removed from the governor's control than state government employment. Even so, the governor may still have the incentive and means to influence local government employment, provided that local officials and politicians are cooperative. With state government employment, no such provision is necessary. Several heterogeneity tests support this explanation – local government employment cycles are weaker in situations where there is likely less local support for the incumbent, such as when the election is close, the incumbent term limited, or local citizens tend to vote against the incumbent's party, while for state government these factors do not matter.

Disaggregating government employment by subcategory, there is evidence that the justice, public order and safety subcategory, which includes law enforcement, corrections and court workers, plays a role in the effects observed. Education, on the other hand, does not appear to exhibit cycles. I also investigate federal government and private sector employment. Federal government, as expected, is not influenced. The results for the private sector are mixed.

Despite the importance of US gubernatorial elections and their 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. 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 have higher local government employment growth during mayoral election years.

Because of mixed early evidence for cycles in developed economies, attention shifted to developing countries and young democracies, where political cycles are believed to be more prevalent (Brender and Drazen, 2005; Shi and Svensson, 2006; Hanusch and Keefer, 2014). 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 Philippine 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, by contrast, I contribute to a small but growing literature on developed economies using modern methods and more comprehensive data, showing that, in fact, electoral cycles often persist despite economic and institutional maturity. The most closely related 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 labor market policies reducing unemployment are pushed before elections (Chortareas et al., 2016; Dahlberg and Mörk, 2011; Tepe and Vanhuysse, 2009; Mechtel and Potrafke, 2013).[3]

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 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

[3]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. US studies focusing on non-employment variables include: Reynolds (2014), who studies tuition levels in public institutions; Sørensen et al. (2001), who find that states accumulate smaller surpluses in election years during economic upturns but not during downturns; and Rose (2006), who examines how business cycles in fiscal variables depend on balanced budget rules.

multiple channels simultaneously or selectively depending on the circumstances (e.g., through timing the use of executive authority, or through influencing the budget well before the election), or when cycles in policy variables that do not affect outcomes are of less consequence.

The heterogeneity tests are suggestive of channels through which manipulation may operate and add to our understanding of the effects of strategic interactions between different tiers of government. The distributive politics literature investigates how higher levels of government funnel funds towards aligned lower level government entities (e.g., Larcinese et al., 2006; Solé-Ollé and Sorribas-Navarro, 2008), but less attention is paid to how local governments behave, or what they do in exchange. When they do touch on this, it is often to see whether turnout and vote shares respond to receipt of funds (Ansolabehere and Snyder, 2006). My results suggest a new way local governments may "earn" such benefits – by influencing local government employment.

While gubernatorial elections are state-level events, by using county-level data, I am also able to investigate spatial heterogeneity in the incidence of electoral cycles within a state – are effects concentrated in, e.g., counties more ideologically aligned with the incumbent? Previous studies largely investigate heterogeneities at the level of the jurisdiction of the elections considered, e.g., across states when studying gubernatorial elections (Rose, 2006; Shi and Svensson, 2006; Foremny and Riedel, 2014). Only a few investigate how the effects at the aggregate level are distributed within those jurisdictions (e.g., Reynolds, 2014).[4]

[4]This is also related to the broad literature on distributive politics, however, most of these studies involve a limited number of elections at the higher level, e.g., presidential elections (Larcinese et al., 2006; Kim et al., 2012). These settings, while conceptually similar, come with many empirical tradeoffs.

Finally, many previous studies use more traditional panel models. Recently, discontinuities at political and administrative boundaries have been exploited as a clean source of identification.[5] To my knowledge, the border discontinuity approach has not previously been applied to political cycles, a setting for which it is quite well-suited.

1.2 Research design

1.2.1 Institutional setting

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. Governors thus have significant influence over the direction of the state budget and policy environment. These powers may also 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 (except New Hampshire and Vermont, where terms are two years). Elections take place in November and every year the governorship is up for election in a subset of the states. Figure 1.1 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. Incumbents cannot chose when

[5] Studies focus on how labor regulations, tax incentives, environmental regulations, and energy prices affect business location decisions, especially of manufacturing firms (Holmes, 1998; Kahn, 2004; Chirinko and Wilson, 2008; Duranton et al., 2011; Kahn and Mansur, 2013; Rohlin et al., 2014). The employment effects of minimum wages (Dube et al., 2010) and spillovers of tax and fiscal policy are also studied (Chirinko and Wilson, 2008; Peltzman, 2016).

elections take place.[6] Rather, these election schedules have been historically fixed for some time, with few changes in the past 50 years.[7] 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.

The governorship is not identical in each state, but US states are certainly more comparable than the national governments considered in cross-country studies (Alesina and Roubini, 1992; Shi and Svensson, 2006; Potrafke, 2010, 2012; Annen and Strickland, 2017). Static state-specific features of the governorship will largely be controlled for by fixed effects. The other immediate concern is other simultaneous elections – presidential, congressional, local. This is discussed in the next section.

[6] In extraordinary circumstances special elections and recall elections are possible. In these cases, the winner finishes the current term – a new election will still be held according to the state's fixed schedule. There were three special election during my sample period: Utah in 2010 (incumbent resigned due to promotion), West Virginia in 2011 (incumbent died), and Oregon in 2016 (incumbent resigned due to scandal). Dropping these elections does not affect inferences. There were two recall elections, California 2003 and Wisconsin 2012, which are dropped because their occurrence was unusual and highly related to economic conditions (recall elections can be triggered by petition when enough signatures are collected in a certain amount of time), though this also does not affect inferences.

[7] 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.

1.2.2 Empirical strategy

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

$$Y_{csqt} = \sum_{k=-3}^{3} \beta_k \cdot (\text{Elec}+k)_{sqt} + \gamma' \cdot X_{cqt} + \alpha_{cq} + \lambda_{qt} + \varepsilon_{csqt}, \quad (1.1)$$

where c, s, q, and t refer to county, state, quarter and year. The variable $(\text{Elec}+k)_{sqt}$ is a dummy variable equal to one if the current quarter is k quarters after (before, if k is negative) an election quarter. To better align with the timing of elections, which almost always take place in November, the definition of a quarter is shifted forward by one month, i.e., the quarters are February-April, May-July, August-October, and November-January.[8]

The dependent variable, Y_{csqt}, is employment per capita in the employment category of interest, in line with the preferred specifications in previous studies (Dahlberg and Mörk, 2011; Labonne, 2016).[9] The coefficients of interest are β_k, interpreted as the difference in employment per capita when there is an election compared to when there is no election. The coefficient estimates reported are multiplied by 100 thousand for a simple per 100 thousand capita interpretation. I cluster the standard errors at the state level to account for

[8]By using a seven quarter window around the election, there is the possibility of overlap in the windows for different states, particularly if their elections are one year apart. The results are not sensitive to dropping states with elections in odd years to eliminate overlap (see Section 1.7.2). I also consider longer election windows in Section 1.7.3.

[9]In Section 1.7.2 I also consider using the quarter-to-quarter growth rate in employment or the log level of employment as the dependent variable. One might also consider a lagged dependent variable on the right hand side. I do not do so because the election cycles are fixed and exogenous. Including lagged dependent variables in a fixed effect model also results in bias (Nickell, 1981), though the bias disappears asymptotically in long panels. In any case, I show in Section 1.7.2 that including a lagged dependent variable does not change inferences.

serial correlation and the fact that the election variable is constant within a state.[10]

The vector X_{cqt}, 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, baseline (first quarter 1990) values of log income per capita, log of population, fraction of private sector employment in goods producing industries as opposed to services industries.[11] This helps control for long term trends affecting different types of counties, for example, the gradual decline experienced by manufacturing industries. Contemporaneous or lagged values are not used because: first, contemporaneous values may also be affected by elections; second, there may be reverse causality if, say, lower employment causes lower income.[12] As an even more saturated specification, 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.

County-quarter fixed effects, α_{cq}, are always included to control for county-specific seasonal trends and time invariant county characteristics. Unless they are subsumed by other fixed effects, year-quarter effects, λ_{qt}, control for common shocks in different time periods.

[10]I focus on the county level because, first, I explore spatial heterogeneity across counties and, second, the subsequently discussed county-pair approach relies on the use of county-level data. By clustering at the state level, I allow arbitrary correlations across counties in a state. County level controls can also improve the precision of the estimates (Foremny and Riedel, 2014). In any case, the results are robust to aggregating to the state level – see Section 1.7.2. I work at the quarterly level rather than the monthly level since it is less noisy and aids in the presentation of the tables, but the results are not sensitive to the aggregation scheme (Section 1.7.2).

[11]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.

[12]Inferences are robust to using contemporaneous values of the control variables – see Section 1.7.2.

The year-quarter fixed effects also help control for simultaneous federal elections – i.e., presidential and House elections. Senate elections do not all occur at once and, in Section 1.7.5, I incorporate them into my analysis. Local elections, e.g., mayoral or city council elections, are another potential omitted variable. First, I believe it is reasonable to assume that these elections tend to be less important than gubernatorial elections, and should not confound the results in a systematic way, though I concede it is possible. Second, cycles are observed in both state and local government employment. While co-timed mayoral elections could contribute to cycles in local government employment, this is less likely for state employment.[13]

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 controls 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 run at the national level (Dube et. al, 2010). The implicit assumption 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 also follow Dube et al. (2010) who, studying the employment effects of minimum wages, implement a geographic discontinuity design that compares employment within pairs of counties that straddle a state border and are generally similar, belonging to the same local market and experiencing similar economic fluctuations.

[13] The mayoral elections dataset of Ferreira and Gyourko (2009), graciously provided by the authors, registers 3464 mayoral elections in 886 cities between 1990 and 2007. Of these mayoral elections, approximately 18% were held in the same year that a gubernatorial election was held in the state, indicating a moderate preference for holding mayoral elections in non-gubernatorial election years in this sample of cities.

The specification takes the following form:

$$Y_{cjsqt} = \sum_{k=-3}^{3} \beta_k \cdot (\text{Elec+}k)_{sqt} + \gamma' \cdot X_{cqt} + \alpha_{cq} + \pi_{jqt} + \varepsilon_{cjsqt}, \tag{1.2}$$

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 mechanical correlation induced by including duplicate counties along a border segment (pair of adjacent states), I cluster the standard errors both at the state level and at the border segment level.[14] County-pair-year-quarter fixed effects, π_{jqt}, control for economic conditions at the very local level, using within county-pair variation in electoral cycles to identify the effect of an election on employment rates.

The county-pair approach offers a clean identification strategy that controls for very local economic conditions, avoiding 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 imagine, and border areas may be subject to different dynamics or spillovers across states. Although private sector firms may shift their activities across borders 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 state specific.[15] There is also a practical issue – focusing on border

[14]Counties with more pairs also implicitly receive unequal weight in the regression. The results are not sensitive to weighting each county by the inverse of the number of pairs in which it appears (Section 1.7.2).

[15]E.g., police officer training is naturally specific to 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

counties reduces sample size. The two methods thus complement each other, and it is comforting that the results are similar using either.

1.2.3 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 first quarter of 2017. 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 the first quarter of 2017, the QCEW recorded, 14.3 million local government employees, 4.6 million state government employees, and 119.8 million private sector employees in the US (the aggregate trends are shown in Figure 1.3 in Appendix 1.6).

The main categories of both state and local 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 state or always local (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, workers in public universities are usually state employees, and for community college workers it varies). Elected officials, members of a legislative body and members of the judiciary are excluded from the QCEW,

up for any lacking basic training, among other paperwork. See https://www.tcole.texas.gov/content/out-state-peace-officers (accessed 3/6/17).

as well as the vast majority of poll workers who help supervise elections.[16] In Section 1.3.3, despite data limitations, I attempt to disaggregate by government employment subcategories and, in Section 1.3.4, I consider federal government employment and a number of private sector industries, including manufacturing and construction.

Population and income data come from the Bureau of Economic Analysis[17] and the urban/rural dummy comes from a classification by the National Council for Health Statistics. The cross sectional variables used in Section 1.3.3 come from the 2000 census. Data on election outcomes are primarily from David Leip's Atlas on US Presidential elections and sources such as individual state agency websites. My main sample covers 1321 state-years, in 359 of which there were gubernatorial elections.

The main sample for the fixed effects analysis is constructed as follows. Starting from the 3143 county equivalents in the US, I exclude those with missing data for the variables considered and those that were affected by boundary changes and could not be matched across datasets. One complication is that the BLS suppresses employment data for some counties out of confidentiality concerns. I exclude all counties with suppressed data during my sample period.[18] 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 implausible

[16] Those that earn less than $1000 per annum are not covered in the definition of employment for state Unemployment Insurance purposes (Title 26, United States Code §3309(b)(3)(F), 2010). There are multiple additional reasons to believe the results are not driven by poll workers, discussed in detail in Section 1.7.4.

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

[18] The Bureau of Economic Analysis, for statistical purposes, sometimes combines several county equivalents into one statistical area. This occurs predominantly in Virginia, where small independent cities are often combined with a larger neighboring county. Where possible, I aggregated the QCEW employment data for counties within a statistical area so that they could be matched across datasets – thus, a few of my "counties" consist of more than one county equivalent area.

changes remain, though there is no reason to believe these anomalies are related to elections. To consistently lessen the impact of such administrative changes, I exclude counties whose maximum (minimum) change in quarter-to-quarter employment per capita is in the top or bottom 1%.[19]

My final sample consists of 1751 counties, with a combined population of about 236 million in 2016, or about 73% of the total US population of 323.1 million (US Census Bureau). In terms of the workforce, in the first quarter of 2017, my sample includes a total of 10.4, 3.3 and 85.4 million local government, state government, and private sector employees out of the 14.3, 4.6, and 119.8 million recorded by the QCEW, or about 71-73% coverage. Summary statistics are in Table 1.7, and the counties included are shown in Figure 1.4.[20] For the county-pair sample, I start from 1069 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 500 counties, from which 430 county-pairs can be formed at 71 border segments (pairs of adjacent states). Summary statistics are in Table 1.8 and the included counties are shown in Figure 1.5.[21]

[19]There remain some large, though plausible, outliers. In Section 1.7.7, I check the robustness of the results to a more stringent criterion for excluding outliers.

[20]The missing counties are often sparsely populated counties in the West, with the notable exception 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 1.7.7 I check whether the results for local government continue to hold on a larger sample that only requires that counties have local government figures, rather than both local and state.

[21]Out of 71 border segments: 31 have the same election cycle; for 22, the two 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 3 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.

1.3 Results

1.3.1 State government employment

Table 1.1 presents the main results for state government employment using the fixed effects (columns 1-3) and county-pair approaches (columns 4-7). The coefficient estimates, as they are multiplied by 100 thousand, can be interpreted as the additional employees per 100 thousand capita (the average county in my sample has a population of about 120 thousand). The coefficient estimates corresponding to columns (2) and (5) are graphed against the number of quarters to the election in the top panel of Figure 1.2. The specification in 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 and time invariant control variables interacted with linear and quadratic time trends. 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. In this specification, I retain the electoral cycle dummies interacted with linear and quadratic time trends from column (2), since interacting these variables with year-quarter effects would sweep out all of the identifying variation.

All three fixed effects specifications indicate that state government employment per capita is higher in counties that have a gubernatorial election coming up than in counties that do not. In column (1), which essentially compares means net of county-quarter and year-quarter fixed effects, the estimates are large in magnitude and highly statistically significant. When further control variables are incorporated, the estimates decrease somewhat in magnitude, remaining statistically significant at lower levels. After the election, the effects drop off rapidly – by the quarter Elec+2 the effect is no longer statistically significant in any specification. The estimated coefficient (column 2) peaks at about 7 additional state government employees per 100 thousand capita in the quarter of the election.[22]

In columns (4)-(7), the pattern of higher state government employment leading up to elections reappears in the county-pair regressions. Column (4) includes county-quarter and pair-year-quarter fixed effects. Column (5) includes controls interacted with linear and quadratic time trends (except census division time trends as local trends are already captured by the pair-year-quarter effects). Column (6) replaces these time trends with dummy variables interacted with time fixed effects (similar to column 3). Since moving to the county-pair method involves a change in sample, column (7) runs a fixed effects regression on the sample of border counties, using the column (2) specification. The coefficient estimates indicate that employment is higher by up to about 8 employees (column 5) per 100,000 before elections – though the effects do appear to dissipate earlier than using the fixed effects approach.

These patterns of heightened state government employment leading up to elections are consistent with the manipulation hypothesis. Moreover, given the typical county is quite small – the average state has about 63 counties (alternatively, the typical state is similar in population to Indiana, with about 6.5 million residents) – these effects may quickly become economically and politically substantial. One can make a rough back-of-the-envelope calculation of the additional costs implied. In 2016 the average weekly wage

[22]The R^2 values are high because the large baseline differences in employment across different counties, which are explained by the county-quarter fixed effects, dwarf the fluctuations over time for a given county.

of a state government employee in the counties in my sample was $853 in 2016 dollars (QCEW). Assuming any additional employees receive the average wage, the cost based on the coefficient estimates for Elec-2 through Elec+1 is about $260,000 per 100 thousand capita, or $2.60 per person. While this does not seem not enormous in absolute terms, it is large compared to other relevant costs. Comprehensive national data on the costs of holding elections is lacking but, e.g., the 2014 general elections cost Colorado counties about $12.2 million, or $2.30 per capita.[23] There are also fixed costs. Secure electronic voting systems have become a salient issue following the 2016 general elections, but they are expensive and some governments struggle to afford them. In 2018, California governor Jerry Brown proposed $134 million – about $3.40 per capita and half of the amount needed statewide – to upgrade outdated voting equipment.[24] Also in 2018, Pennsylvania governor Tom Wolf told counties that he wanted them to replace their electronic voting systems before 2020. However, counties warned of a lack of funds, estimating a price tag of up to $125 million, or about $10 per capita.[25]

The effects are very short term in nature, consistent with several explanations. The most conventional is that voters are often myopic and take into account only very recent events. What drives this myopia is a matter of debate. Healy and Lenz (2014) find that while voters intend to judge incumbents based on their performance for their whole term, information on cumulative outcomes lacking, so they heuristically use recent conditions

[23]Obtained from the Accountability in Colorado Elections interactive tool from the Colorado Secretary of State: http://www.sos.state.co.us/pubs/elections/ACE/ElectionCostStatistics/atlas.html?indicator2=i0&date2=Gross\%20Cost (accessed 7/7/18).

[24]See, for example, the following article: http://www.latimes.com/politics/la-pol-ca-california-budget-brown-proposed-20180111-htmlstory.html (accessed 7/7/18).

[25]See https://www.usnews.com/news/best-states/pennsylvania/articles/2018-04-12/pennsylvania-asks-counties-to-replace-voting-systems-by-2020 (accessed 7/7/18).

as a substitute. Also, media coverage of the economy may be different before and after elections, in both volume and in tone (Soroka et al., 2015). Wlezien (2015) argues that voters care fairly equally about things that happened long ago and recently, which still leaves an incentive for incumbents to induce short term cycles. Short term effects are also consistent with previous findings (Akhmedov and Zhuravskaya, 2004; Labonne, 2016).

From the perspective of the incumbent governor, there is also an incentive to, rather than just increase employment to win additional votes, avoid negative publicity before elections. With heightened attention from journalists and political opponents, even a small reduction in hiring could be broadcast in a negative light to the public and have electoral repercussions (Ferraz and Finan, 2008). Thus, the incumbent would have an incentive to allow hiring to accumulate excessively during the campaign period with an eye to delaying hard decisions until afterwards, especially if media coverage responds more to changes than to overall levels (Soroka et al., 2015). This explanation is consistent with the anecdotal evidence (see Introduction) and other findings in the literature (Foremny and Riedel, 2014).

Going forward, I choose the specifications in columns (2) and (5) of Tables 1.1 as the preferred specifications – they offer a reasonable balance between controlling for heterogeneous time trends and eliminating too much of the variation – I will often present results for these specifications only to save space. In Section 1.7.2, I show the results are robust to a range of plausible alternative specifications besides the three considered so far.

1.3.2 Local government employment

Theoretically, state government employment is more likely to exhibit electoral cycles than local government employment, e.g., at the municipal or county level, because it is more within the influence of the governor. On the other hand, there may be both incentives and means for the governor to affect local government employment, especially when local governments officials or politicians (mayors, city counselors, county commissioners, etc.) are ideologically aligned or have an incentive to cooperate. Higher level governments have been shown to channel grants towards ideologically aligned lower level governments (Larcinese et al., 2006; Solé-Ollé and Sorribas-Navarro, 2008). Less work has been done on how lower level governments may respond to or "earn" this favoritism (except Ansolabehere and Snyder, 2006, who show that aligned local governments that received more grants had higher turnout in subsequent elections). It could be that another channel through which local governments respond to such benefits is through influencing local government employment.

Table 1.2 provides evidence that cycles in local government employment are observed using both the fixed effects (columns 1-3) and the county-pair approach (columns 4-7), and the coefficient estimates corresponding to columns (2) and (5) are graphed in the bottom panel of Figure 1.2. The results indicate that local government employment per capita is higher in counties that have a gubernatorial election coming up than in counties that do not, peaking at 13 employees per 100,000 capita for the fixed effects specifications and 17 for the county-pair specifications. After the election, the effects again drop off rapidly – the coefficient estimate for the quarter following the election quarter, Elec+1, is already statistically insignificant in all specifications.

The pattern is, as in the case of state government employment, consistent with the manipulation hypothesis. The financial cost is roughly \$3.70 per person, again a substantial amount.[26]

[26]Based on an average weekly wage in 2016 of a local government employee in the counties in my sample of \$728 in 2016 dollars, and the coefficient estimates for Elec-3 through Elec. The somewhat surprising

1.3.3 Heterogeneity

Electoral cycles may vary depending on the institutional or political setting and spatially across counties within a state. Effects may also be driven by certain categories of government employees. Studying heterogeneity may be useful in understanding the mechanisms at play. I restrict attention to the fixed effects specifications, since in the county-pair specification identifying variation may be coming from too few border segments[27] and data limitations are restrictive when disaggregating by subcategory.

Political and institutional heterogeneity

I first include interactions to test for differences across political settings. A complication when working with interactions is that sometimes they are clearly not exogenous to local economic conditions, e.g., elections are more likely to be close if recent economic conditions have been poor, and there are large baseline differences in employment between counties in states with close elections compared to those in states without. To better account for these large baseline differences, I include in such cases the one-year lagged dependent variable to control for recent economic conditions.[28] The results including interaction effects are shown in Table 1.3, while the (equivalent) marginal effects are plotted in Figures 1.6-1.7 (further heterogeneity tests are considered in Section 1.7.1 of the Appendix).

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finding of local government employment cycles is investigated further in the next section, where several heterogeneity analyses provide suggestive evidence in favor of the explanation proposed above.

[27]For example, to estimate separate effects of elections when the incumbent is or is not term limited, I need to 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, drastically reducing statistical power because in the county-pair approach the comparison additionally needs to be across *contiguous* states.

[28]In the main regressions I did not, mainly because the election cycles themselves are fixed and exogenous to local economic conditions (although the main results are robust to the inclusion of lagged dependent variables, see Section 1.7.2).

When an election is competitive, it is possible that incumbents have greater incentives to raise employment, since small changes could be decisive. I split the sample based on whether or not the election is close (having a winning margin of 5% or less). Columns (1)-(2) of Table 1.3 (Figure 1.6) show the results. For state government employment, cycles are present whether the election is close or not. For local government employment, the pre-electoral increase in employment is mainly driven by non-close elections, while for close elections the pre-electoral increase is not significant. In the post-election quarters, there is a negative and significant decrease in local government employment for close elections, but no decrease for non-close elections (and the difference between the effects for close and non-close elections is statistically significant). These findings are consistent with the idea that in both close and non-close elections the governor has similar levels of influence over economic policy at the state level. The governor's influence over local governments, however, is less direct. When elections are close there are likely lower levels of support and political capital at the local level, making it more difficult for the governor to affect local government employment.

Term limits are fixed by law in many states. Incentives for governors to manipulate may be lower when they are not eligible for another term (Rogoff and Sibert, 1988; Besley and Case, 1995), though empirical evidence is mixed (Rose, 2006). For government employment, I find that term limits matter at the local level but not the state level. Columns (3)-(4) of Table 1.3 (Figure 1.6) show that: for state government employment, term limits do not matter and cycles are present in both cases; for local government employment, cycles are driven by elections where the incumbent is not term limited, and are absent when the incumbent is term limited (and the differences between the effects for term limited and non-term limited elections are statistically significant for most quarters). It could be that local government is less manipulable either because the incumbent is a lame-duck governor, unable to offer future benefits in exchange for local support, or because open elections are associated with greater political competition. With state government employment, again, the governor has more direct influence and does not need to rely on local officials.

A county's ideological alignment with the incumbent governor may be important, especially as a determinant of local government employment cycles: politically aligned local officials (proxied by the alignment of the county's voters), may coordinate or cooperate with the governor's office; local officials in unaligned counties, on the other hand, may be less facilitating. I categorize counties as strongly Republican if they are in the top quartile of their state in terms of average Republican vote share, and strongly Democratic if they are in the bottom quartile. I create an alignment variable equal to 1 if the incumbent governor and the county are aligned (e.g., there is a Republican incumbent and the county is classified as strongly Republican), -1 if the incumbent and the county are unaligned (e.g., Republican incumbent and strongly Democratic county), and zero otherwise. I interact this variable with the election dummy variables to test for a differential effect of elections in more aligned counties (columns 5-6 of Table 1.3, Figure 1.7). For state government employment, alignment does not appear to matter. For local government, the differential effect of alignment is positive and statistically significant in the quarters Elec and Elec+1. These findings are consistent with the idea that the incumbent relies on local politician support to affect local government employment, but is able to influence state government employment more directly.

In columns (7) and (8) of Table 1.3 (Figure 1.7), 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 (Alt and Lowry, 1994; Bjørnskov and Potrafke, 2013).[29] The results suggest that this is the case. For both state and local government employment, the pattern of increased employment leading up to elections appears more prominent when the state legislature is aligned with the incumbent, although we cannot in most cases reject that the effect is the same in both cases.

Next, I investigate partisan affiliation of the incumbent governor.[30] According to the partisan theories, left-wing governments prefer to implement more expansionary policies, while right-wing governments are more fiscally conservative (Chappell and Keech, 1986; for a survey, see Potrafke, 2018). At the same time, government employees in the United States tend to lean Democratic, which may make manipulation more important for Democratic incumbents compared to Republicans, who rely less on public sector votes. State government employees, especially, tend to lead Democratic.[31] The results in columns (9)-(10) of Table 1.3 (Figure 1.6) are consistent with these arguments. For state government employment, we observe cycles for both Republicans and Democrats, but the pattern is more pronounced in the lead up to elections under Democrats. For local government employment, cycles are present for both Democratic and Republican incumbents, but

[29]There are, of course, 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.

[30]Elections with independent incumbents are very few and are dropped.

[31]According to a 2010 Gallup poll, 46.1% of unionized state government employees identified as Democratic versus 23.7% as Republican (39.2% vs. 29.5% if non-unionized). For local government employment, the gaps were smaller: 40.6% vs. 26.6% for unionized and 33.7% vs 32.7% for non-unionized workers. See http://news.gallup.com/poll/146786/democrats-lead-ranks-union-state-workers.aspx (accessed 11/13/17).

are more pronounced under Democrats (significantly so for the quarter Elec+1), and the pre-electoral increase in employment is not statistically different from zero for Republicans.

Institutional differences in state budgeting processes may influence cycles. 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. Following Clemens and Miran (2012), I define 19 states as having strict balanced budget rules.[32] Since opportunistic manipulation of public sector employment puts pressure on the state budget, it may be limited in magnitude in states with strict balanced budget laws or, if not, we may observe post-election cuts since any deficit should be eliminated before the end of the budget cycle (Rose, 2006). 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.[33]

The results are shown in columns (11)-(12) of Table 1.3 (Figure 1.6). For state government employment, employment contradictorily increases more in states with strict rules, although the differences do not turn out to be statistically significant. For local

[32]States are scored on the stringency of budget rules in a 1987 Advisory Commission on Intergovernmental Relations (ACIR) report. Strict balanced budget states are those with scores of 7 or more on the ACIR's 1-10 scale and with annual budgetary and legislative cycles (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). The strict balanced budget states are AL, AZ, CO, DE, GA, ID, IA, KS, MS, MO, NJ, NM, OK, RI, SC, SD, TN, UT and WV.

[33]However, Boylan (2008) shows how state governments use overly optimistic revenue forecasts to circumvent balanced budget requirements, and increase spending prior to elections.

government employment, in states with strict balanced budget rules, the pre-electoral increase and post-electoral decrease do not attain statistical significance. The effects for Elec and Elec+1 are significantly lower in states with stringent rules compared to states with lenient rules, consistent with a more urgent need to balance the budget in strict states (Rose, 2006).

Government employment subcategories

It would be desirable to know which employees exactly may be subject to the effects observed previously. Unfortunately, data limitations do not allow for a comprehensive analysis, as the data suppression issue becomes much more severe as we disaggregate to lower level employment categories. I am, however, able to investigate a few particularly important categories in this section, albeit with much reduced sample sizes.[34] I also explain why an alternative explanation, that the results are driven by poll workers, is very unlikely.

I consider public administration and education. Public administration includes a wide range of employees: law enforcement and fire protection, courts, public finance activities, government offices, administration of education, human resources, economic and environmental programs, among others. Education consists largely of schoolteachers at the local level and universities at the state level, while other institutions such as community colleges may be either state or local. Within public administration, I also look at the subcategory of justice, public order and safety employment, which includes law enforcement officers, firefighters, corrections and court workers. Again there are differences between

[34] It is only practical to use the fixed effects approach because, of the remaining counties, even fewer are located at state borders and, of these, still fewer border another county that also does not suffer from data suppression.

the local and state levels, e.g., local law enforcement includes municipal police and county sheriffs' offices, while at the state level it typically includes highway patrol, transit police, various state investigative agencies (criminal, regulatory, revenue enforcement, licensing, etc.), park rangers, departments of homeland security, among others. These are important categories as, in addition to having comparatively good data coverage, they are perhaps among the most salient in terms of public good provision as well as coverage in the media. Regression results are reported in Table 1.4.[35]

For state government employment (columns 1-3), despite the small sample sizes, state public administration shows a clear increase in employment in the period leading up to and including the election quarter (column 1). The coefficient estimates are highly statistically significant for a full five quarters. A fair portion of this effect appears to be coming from the justice, public order and safety subcategory, which shows a similar pattern that also attains high levels of statistical significance (column 3).[36] State education employment does follow the pattern of increased employment leading up to the election, although the coefficient estimates are not statistically significant at conventional levels. One possible explanation is that there may be other offsetting channels of manipulation – e.g., Reynolds (2014) finds electoral cycles in tuition, which could put pressure on university budgets and prevent hiring.

For local government employment (columns 4-6), the results are inconclusive. Local public administration does not appear to be impacted. Local education employment is

[35]Because the number of states represented in the regressions is substantially reduced below conventional levels, I cluster at the state-by-half of sample level.

[36]While the effects appear strong in justice, public order and safety, the aggregate effects are not coming only from this category. Using total state government employment as dependent variable on the smaller sample (result not reported) gives coefficient estimates of up to 17 employees per 100 thousand capita, compared to 4-5 employees per 100 thousand capita for justice, public order and safety.

slightly higher when an election is approaching, but the increase is small and far from statistically significant. For local justice, public order and safety employment, while we do not see a pre-electoral increase in employment, we do see a statistically significant drop in the two quarters following the election, consistent with post-election cuts. The sample sizes for these three regressions are, however, much reduced, retaining 1324, 856 and 370 counties, respectively, out of the 1751 the main sample.[37]

Another important government employment category is poll workers – the effects may simply be due to the workers who supervise polling stations on election day. There are multiple reasons this is unlikely, detailed in Section 1.7.4 in the Appendix. Most importantly: the vast majority of poll workers are excluded from the QCEW dataset. Also, among other reasons, the identification strategy is mostly based on comparing states with and without gubernatorial elections, but *all of which also hold federal elections* and, hence, already hire poll workers. The findings are robust to dropping cases where this is not true.

1.3.4 Federal government and private sector employment

Federal government and private sector employment may behave differently under the manipulation hypothesis than state and local government employment. The governor is unlikely to be able to affect private sector firms and federal government agencies as easily. That said, there may be differences across industries. Construction, for example, may see increased contracts before elections because of new government infrastructure projects timed with the elections in mind or increased issuance of building permits (Garmann,

[37]Indeed, when using aggregate local government employment as the dependent variable, but restricting to the smaller samples, the pre-electoral increases in employment are still present, but only attain statistical significance on the 1324 county sample (result not reported).

2017). Manufacturing firms often play an important role in the local economy, and may be politically connected or sensitive to policy outcomes (Claessens et al, 2008; Innes and Mitra, 2015).

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, 2017). Indeed, Jens (2017) 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 prior to elections, followed by a "rebound" afterwards. Note that these predicted effects would push in the opposite direction to manipulation effects. Uncertainty could also plausibly apply to government employment. Although this seems less likely, the estimated manipulation effects in Section 1.3.1 are likely to be lower bounds.

The results of the fixed effects approach applied to federal government and private sector employment are shown in Table 1.5. Besides aggregate private sector employment, I also investigate manufacturing, construction, retail, and professional and business services.[38] I hypothesize that manufacturing and construction are more likely to be impacted, while retail and professional and business services are diverse categories and seem less so, at least through the manipulation channel.

The results show that federal government employment, as expected, does not appear

[38]Professional and business services broadly include legal, accounting, engineering, administrative, management and other services.

to be influenced by gubernatorial elections. Aggregate private sector employment, however, shows a pattern of higher employment the two quarters prior to an election, by up to 40 employees per county (private sector employment is generally much higher than government employment). It appears that both manufacturing and, to some extent, construction contribute to this pattern (significant at the 10% level). Neither retail nor professional and business services appear to be affected. These results are consistent with political manipulation in certain sectors before elections through, perhaps, more expansionary economic policy such as construction projects or through political connections.

The results of the county-pair regressions are shown in Table 1.6. The results are very similar for federal government and retail employment. Construction and professional and business services display a similar pattern of coefficient estimates, although in the former case they are no longer statistically significant at conventional levels, while in the latter case they now attain statistical significance. The results for aggregate private sector, however, are not consistent with the results of the fixed effects approach, a discrepancy that appears to be driven by manufacturing and, to some extent, professional and business services. In these sectors, manufacturing especially, employment is lower leading up to elections, at high levels of statistical significance, and in the post-electoral period employment quickly returns to normal. This pattern is not consistent with manipulation, but is consistent with the political uncertainty hypothesis, whereby hiring in sensitive industries is lower on the side of the border with greater uncertainty induced by an upcoming election.

A plausible explanation for the discrepancy between the fixed effects and the county-pair results for certain sectors is that employment dynamics in border regions may be very different. Compared to government employment, which is less mobile across state boundaries, private sector employment need not be so constrained and spillovers are an issue. A large literature also shows that firms are more likely to locate on the side of the border with more favorable policies, which may lead to a higher (or lower) than usual density of politically sensitive firms at state borders (Holmes, 1998; Kahn, 2004; Kahn and Mansur, 2013; Rohlin et al., 2014). Thus, while these results are suggestive, and there may be good reasons why the results of both approaches do not agree, and they should perhaps be taken more cautiously than the results for local and state government employment. Future work may look to investigate private sector employment dynamics in more detail.

1.4 Conclusion

Using both fixed effects models and a geographic discontinuity design that zooms in on county-pairs straddling state borders with different election cycles, I find evidence of electoral cycles in government employment. Employment tends to increase leading up to elections, after which it abruptly returns to normal levels. This pattern is consistent with manipulation that could occur through, e.g., delaying a hiring freeze until after an election, or more restrained use of veto powers before elections. The magnitude of the effects is potentially substantial economically and politically. The results are consistent across both approaches and stand up to a range of additional robustness checks. Heterogeneity is explored, and several results support the link to manipulation.

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 an economic giant like the United States, is important, and suggests that electoral cycles are widespread, economic and institutional development notwithstanding.

Future research should investigate further the channels through which governors might influence public sector employment, e.g., which policies are most often timed to the electoral cycle and how this process plays out in the legislative and executive branches. More needs to be said about the impact on the provision of local public services, which would provide a better quantification of the overall effects on a region. Related, while my focus is on relatively short term dynamics, it would also be interesting to assess any long term effects on the size and composition of state and local government, individual occupational or migration decisions, and voting decisions in subsequent elections.

1.5 Acknowledgements

I am grateful to Julie Berry Cullen, Gordon Dahl, Roger Gordon, Seth Hill, and Thad Kousser for helpful comments and suggestions. I also thank Steven Cahan, Mitch Downey, Niklas Potrafke, Fanglin Sun, seminar participants at UCSD and the 2017 Public Choice Society Meetings, where I am grateful for the Ostrom Prize. I am very grateful to Fernando Ferreira for making available the dataset from Ferreira and Gyourko (2009). Finally, I benefited greatly from the many useful comments of the anonymous referees.

Chapter 1, in full, is a reprint of the material as it appears in: Dodge Cahan,

“Electoral cycles in government employment: Evidence from US gubernatorial elections”, *European Economic Review*, vol. 111, 2019. The dissertation author was the sole author of this paper.

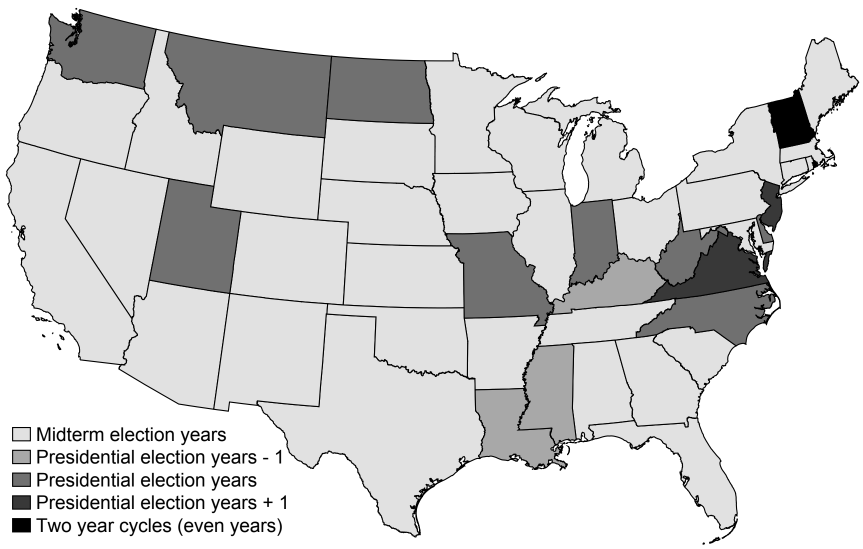


Figure 1.1: Timing of US gubernatorial elections by state.

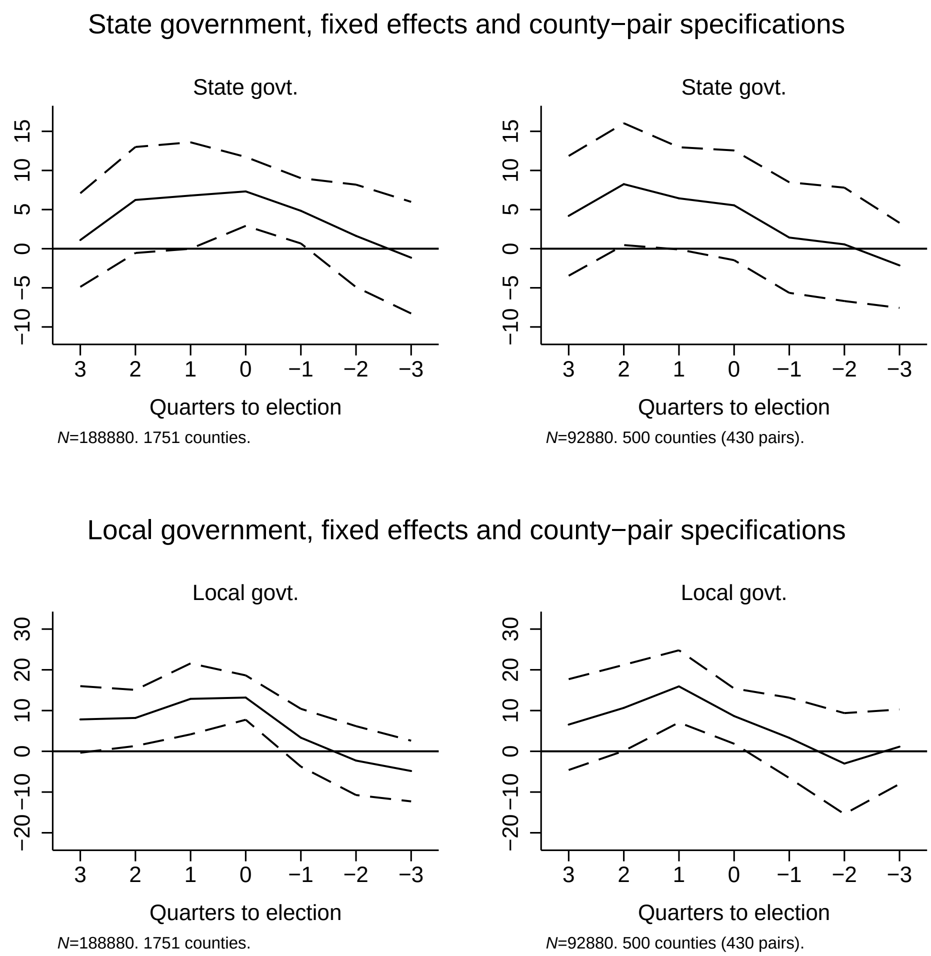


Figure 1.2: Coefficient estimates from main regressions. Each figure corresponds to a separate regression. Corresponds to Tables 1.1-1.2. Dashed lines represent 95% confidence intervals.

Table 1.1: Main results for state government employment using fixed effects and county-pair approaches. See also Figure 1.2. Further specifications considered in Tables 1.12-1.14.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	State government						
Elec-3	4.46	1.10	0.33	3.20	4.20	4.23	6.79**
	(3.84)	(2.98)	(3.37)	(6.33)	(3.79)	(3.83)	(3.31)
Elec-2	9.53**	6.23*	5.25	7.15	8.25**	8.71**	10.70***
	(4.23)	(3.37)	(3.92)	(6.44)	(3.86)	(3.92)	(3.40)
Elec-1	10.05**	6.80*	5.99	5.23	6.44*	6.65*	7.40*
	(4.25)	(3.38)	(4.03)	(5.44)	(3.24)	(3.47)	(3.69)
Elec	10.12***	7.32***	5.72*	4.43	5.55	6.10	6.69**
	(3.32)	(2.19)	(2.98)	(5.40)	(3.47)	(3.66)	(3.09)
Elec+1	6.48***	4.85**	3.69	0.75	1.43	0.55	7.26**
	(2.41)	(2.07)	(2.72)	(4.38)	(3.50)	(3.43)	(3.26)
Elec+2	3.27	1.64	2.09	-0.19	0.56	-0.49	3.66
	(3.35)	(3.26)	(3.03)	(4.71)	(3.59)	(3.45)	(3.54)
Elec+3	0.53	-1.14	0.46	-2.94	-2.13	-3.21	-2.61
	(3.66)	(3.55)	(2.38)	(3.45)	(2.69)	(2.64)	(3.39)
N	188880	188880	188880	92880	92880	92880	41820
R^2	0.97	0.97	0.97	0.97	0.97	0.97	0.95
County·quarter	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	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 1751 counties, 49 states; columns (4)-(7) on 500 counties, 430 county-pairs, 71 border segments, 43 states. Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. "CD" refers to census division. Standard errors (in parentheses) clustered at the state level (columns 1-3) or two ways at the state and border segment level (columns 4-7). * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.2: Main results for local government employment using fixed effects and county-pair approaches. See also Figure 1.2. Further specifications considered in Tables 1.12-1.14.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Local government						
Elec-3	1.64	7.83*	5.30	2.22	6.56	7.41	0.90
	(4.23)	(4.06)	(3.88)	(5.45)	(5.52)	(5.51)	(4.46)
Elec-2	1.96	8.19**	5.61**	6.42	10.66**	11.72**	3.13
	(5.16)	(3.42)	(2.65)	(6.56)	(5.24)	(4.94)	(4.41)
Elec-1	6.57	12.88***	9.24*	11.80**	15.94***	17.13***	13.40**
	(4.32)	(4.33)	(4.74)	(5.66)	(4.39)	(4.09)	(6.48)
Elec	7.22	13.20***	9.95***	5.09	8.65**	9.89***	6.56
	(4.76)	(2.71)	(3.08)	(3.47)	(3.36)	(3.15)	(4.61)
Elec+1	2.02	3.35	1.88	1.28	3.31	4.10	-1.31
	(3.36)	(3.52)	(2.76)	(5.63)	(4.89)	(4.73)	(4.44)
Elec+2	-3.66	-2.27	-5.46	-5.10	-3.00	-1.87	-1.81
	(5.37)	(4.20)	(3.83)	(7.70)	(6.14)	(6.11)	(4.95)
Elec+3	-6.36	-4.83	-4.99	-1.02	1.14	1.59	-5.92
	(4.98)	(3.70)	(3.32)	(5.42)	(4.53)	(4.39)	(5.88)
N	188880	188880	188880	92880	92880	92880	41820
R^2	0.87	0.88	0.88	0.90	0.90	0.90	0.82
County·quarter	Y	Y	Y	Y	Y	Y	Y
Year·quarter	Y	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 1751 counties, 49 states; columns (4)-(7) on 500 counties, 430 county-pairs, 71 border segments, 43 states. Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. "CD" refers to census division. Standard errors (in parentheses) clustered at the state level (columns 1-3) or two ways at the state and border segment level (columns 4-7). * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.3: Heterogeneity tests. See also Figures 1.6 and 1.7 for the marginal effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	X=Close		X=TL		X=Aligned		X=Leg. Aligned		X=Inc. Dem.		X=Strong BB	
	State	Local	State	Local	State	Local	State	Local	State	Local	State	Local
Elec-3	3.40	9.69**	4.88	13.42***	5.36	8.59**	3.12	3.13	0.36	6.96	-1.78	11.54***
	(3.61)	(3.93)	(4.65)	(4.92)	(3.96)	(3.86)	(4.59)	(6.41)	(4.46)	(8.11)	(3.52)	(4.14)
Elec-2	7.32**	8.44*	8.27*	13.99**	8.59**	8.42*	5.25	-0.38	3.94	6.56	3.87	11.37**
	(3.60)	(4.58)	(4.37)	(5.51)	(3.62)	(4.80)	(5.14)	(6.83)	(3.73)	(7.22)	(3.64)	(4.75)
Elec-1	5.36	8.89	6.24	15.90***	6.86*	6.89	3.18	-2.55	2.40	5.46	4.28	14.85**
	(3.60)	(5.47)	(3.79)	(4.72)	(3.43)	(4.57)	(4.95)	(6.21)	(3.12)	(6.79)	(3.31)	(5.78)
Elec	7.62***	13.71***	7.48***	18.79***	8.40***	10.03**	5.30	5.17	3.89*	9.28	5.97***	18.85***
	(2.32)	(4.98)	(2.34)	(5.49)	(2.08)	(4.66)	(3.21)	(7.49)	(2.05)	(6.44)	(2.18)	(4.75)
Elec+1	6.54***	6.45	5.89**	8.21**	6.11**	1.89	4.29	-1.09	6.99**	-3.85	3.77	8.18*
	(2.25)	(4.05)	(2.63)	(4.03)	(2.30)	(4.49)	(3.50)	(7.03)	(2.93)	(5.46)	(2.53)	(4.13)
Elec+2	-1.53	-2.41	-3.38	-5.93	-2.56	-8.89	-5.03	-11.51	-1.29	-8.52	-0.49	1.60
	(3.37)	(5.83)	(3.77)	(6.40)	(3.38)	(6.30)	(4.95)	(7.83)	(3.99)	(5.92)	(4.10)	(5.56)
Elec+3	-5.93	-9.81	-6.20	-15.99**	-6.49*	-15.22**	-6.84	-13.31	-3.35	-17.21**	-3.97	-3.45
	(3.64)	(6.46)	(3.76)	(7.86)	(3.68)	(6.54)	(5.19)	(8.55)	(4.53)	(8.28)	(4.64)	(4.43)
X·Elec-3	9.82	-4.60	1.65	-17.43*	1.41	2.00	4.53	11.02	9.54*	2.77	7.93	-9.97
	(5.88)	(11.27)	(5.18)	(9.39)	(1.68)	(3.95)	(4.70)	(7.47)	(5.15)	(10.83)	(6.89)	(7.65)
X·Elec-2	6.44	1.09	1.23	-19.33*	1.47	3.05	6.73	17.66	8.78*	3.68	6.41	-8.54
	(5.24)	(12.22)	(4.46)	(10.34)	(1.66)	(3.81)	(5.20)	(12.42)	(4.82)	(10.37)	(6.94)	(7.87)
X·Elec-1	7.52	-8.98	2.04	-30.09**	0.70	4.63	7.39	18.86**	8.49*	2.51	6.79	-5.39
	(4.93)	(10.27)	(4.06)	(11.66)	(1.66)	(4.46)	(5.09)	(8.01)	(4.61)	(7.97)	(6.43)	(9.46)
X·Elec	3.88	-18.19**	3.04	-28.99***	0.49	10.04***	6.24	9.75	8.51**	1.23	3.85	-15.98*
	(4.66)	(8.85)	(4.35)	(10.24)	(1.70)	(3.49)	(4.65)	(8.79)	(3.79)	(7.17)	(5.50)	(8.93)
X·Elec+1	-2.12	-22.21***	0.80	-21.94**	-1.99	9.08**	3.88	6.32	-0.80	12.51**	2.47	-13.05**
	(6.28)	(5.95)	(4.62)	(9.86)	(2.23)	(3.97)	(4.74)	(7.56)	(4.57)	(5.75)	(4.61)	(6.31)
X·Elec+2	-4.95	-31.45***	2.82	-10.79	-2.68	5.34	5.26	5.53	-1.62	0.54	5.62	-10.40
	(6.93)	(11.41)	(4.47)	(9.30)	(2.25)	(3.73)	(5.49)	(9.45)	(4.57)	(6.31)	(4.57)	(9.90)
X·Elec+3	-2.72	-26.18*	-0.85	1.09	-2.65	5.53	0.74	-4.15	-5.46	5.54	7.55	-3.55
	(7.00)	(13.50)	(5.02)	(9.03)	(2.43)	(3.54)	(5.13)	(10.42)	(5.29)	(8.16)	(4.57)	(9.08)
N	181876	181876	181876	181876	181876	181876	181876	181876	180865	180865	188880	188880
R^2	0.99	0.98	0.99	0.98	0.99	0.98	0.99	0.98	0.99	0.98	0.97	0.88

1751 counties, 49 states. Dependent variable is employment per capita; coefficients, standard errors multiplied by 100 thousand. Preferred specification (column 2, Table 1.1) plus lagged dependent variable except columns (7)-(10). Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

1.6 Appendix A

Table 1.4: Fixed effects results for state and local government employment, disaggregated by employment subsectors.

	(1)	(2)	(3)	(4)	(5)	(6)
	State government			Local government		
	Pub. ad.	Edu.	JOS	Pub. ad.	Edu.	JOS
Elec-3	4.05*	0.88	3.67**	-2.11	0.44	-0.61
	(2.11)	(6.75)	(1.79)	(2.76)	(4.17)	(1.00)
Elec-2	7.60***	2.77	5.34***	-0.73	3.42	-0.70
	(2.13)	(5.90)	(1.95)	(2.94)	(5.13)	(0.96)
Elec-1	7.49***	5.28	5.59***	0.90	1.57	0.33
	(2.10)	(6.68)	(1.90)	(2.59)	(4.88)	(0.84)
Elec	7.64***	7.61	4.05***	0.91	6.97	0.12
	(1.78)	(4.90)	(1.51)	(2.50)	(4.66)	(0.91)
Elec+1	5.04***	2.92	1.49	-0.96	4.28	-1.27*
	(1.69)	(6.24)	(1.51)	(1.16)	(3.28)	(0.66)
Elec+2	1.20	-1.33	0.70	-2.84*	3.28	-1.74**
	(2.10)	(5.73)	(1.43)	(1.54)	(3.57)	(0.82)
Elec+3	-2.97	0.15	-0.22	-2.22	0.82	-1.04
	(2.30)	(6.02)	(1.67)	(1.49)	(3.25)	(1.22)
N	77784	14952	20004	142768	92248	39876
R^2	0.98	0.94	0.97	0.87	0.93	0.92
County·quarter	Y	Y	Y	Y	Y	Y
Year·quarter	Y	Y	Y	Y	Y	Y
Controls·trends	Y	Y	Y	Y	Y	Y
Counties	721	139	186	1324	856	370
States	46	34	39	47	41	31

Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. "JOS" refers to justice, public order and safety. All specifications include county-quarter, year-quarter fixed effects and controls interacted with time trends (column 2, Table 1.1). Standard errors (in parentheses) clustered at the state-by-half of sample level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.5: Fixed effects results disaggregated by other employment subsectors.

	(1) Fed.	(2) Priv.	(3) Manu.	(4) Const.	(5) Retail	(6) Prof./bus.
Elec-3	1.73	31.49	18.26	9.31*	3.03	-4.15
	(1.77)	(21.46)	(14.55)	(4.90)	(4.41)	(5.35)
Elec-2	0.78	43.27**	22.76*	8.09*	6.26	-5.26
	(2.39)	(21.43)	(11.69)	(4.48)	(4.20)	(4.52)
Elec-1	0.40	36.72*	16.57*	4.80	5.90	-6.86
	(1.93)	(20.43)	(9.86)	(5.26)	(3.98)	(4.15)
Elec	-0.08	15.38	9.35	2.58	3.87	-7.23
	(1.52)	(24.38)	(10.59)	(5.61)	(4.12)	(5.19)
Elec+1	-1.32	13.78	-0.06	9.35	0.05	-2.39
	(1.95)	(19.00)	(16.00)	(6.71)	(2.73)	(5.04)
Elec+2	-0.48	13.26	-2.39	8.52	4.00	-1.31
	(1.76)	(23.32)	(22.36)	(5.67)	(2.56)	(4.67)
Elec+3	-0.58	5.69	-7.59	7.56	3.54	-1.35
	(1.26)	(24.20)	(20.57)	(5.75)	(3.01)	(4.60)
N	186828	188880	160912	145904	185964	161668
R^2	0.96	0.95	0.92	0.80	0.93	0.93
Counties	1732	1751	1492	1353	1724	1499
States	49	49	48	48	49	49

Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. All specifications include county-quarter, year-quarter fixed effects and controls interacted with time trends (column 2, Table 1.1). Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.6: County-pair effects results disaggregated by other employment subsectors.

	(1) Fed.	(2) Priv.	(3) Manu.	(4) Const.	(5) Retail	(6) Prof./bus.
Elec-3	0.65	-45.46*	-40.03***	7.74	7.62	-16.29**
	(2.12)	(26.11)	(9.79)	(7.37)	(5.84)	(8.02)
Elec-2	0.64	-11.08	-40.25***	9.00	9.41	-8.72
	(2.17)	(28.40)	(13.74)	(8.86)	(6.01)	(6.83)
Elec-1	1.95	-11.76	-36.63***	3.91	7.13	-14.32**
	(1.54)	(29.37)	(13.38)	(9.36)	(6.32)	(6.21)
Elec	0.03	-43.41*	-39.76***	-1.00	-1.99	-17.24**
	(1.62)	(22.97)	(10.91)	(4.67)	(6.20)	(6.47)
Elec+1	-2.29	23.14	-15.95	7.93	3.47	-2.01
	(2.40)	(29.02)	(14.33)	(9.18)	(7.31)	(10.46)
Elec+2	-0.69	50.74*	-9.35	5.84	9.94	2.23
	(2.25)	(29.20)	(15.39)	(10.44)	(7.05)	(12.23)
Elec+3	0.45	40.05*	-2.26	5.49	11.87	-5.92
	(1.85)	(21.37)	(12.19)	(9.80)	(7.55)	(12.39)
N	90720	92880	68688	53568	90072	70632
R^2	0.96	0.98	0.97	0.93	0.97	0.97
Counties	489	500	387	321	487	408
County-pairs	420	430	318	248	417	327
Border segments	70	71	67	60	71	66
States	43	43	42	42	43	43

Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. All specifications include county-quarter, pair-year-quarter fixed effects and controls interacted with time trends (column 5, Table 1.1). Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

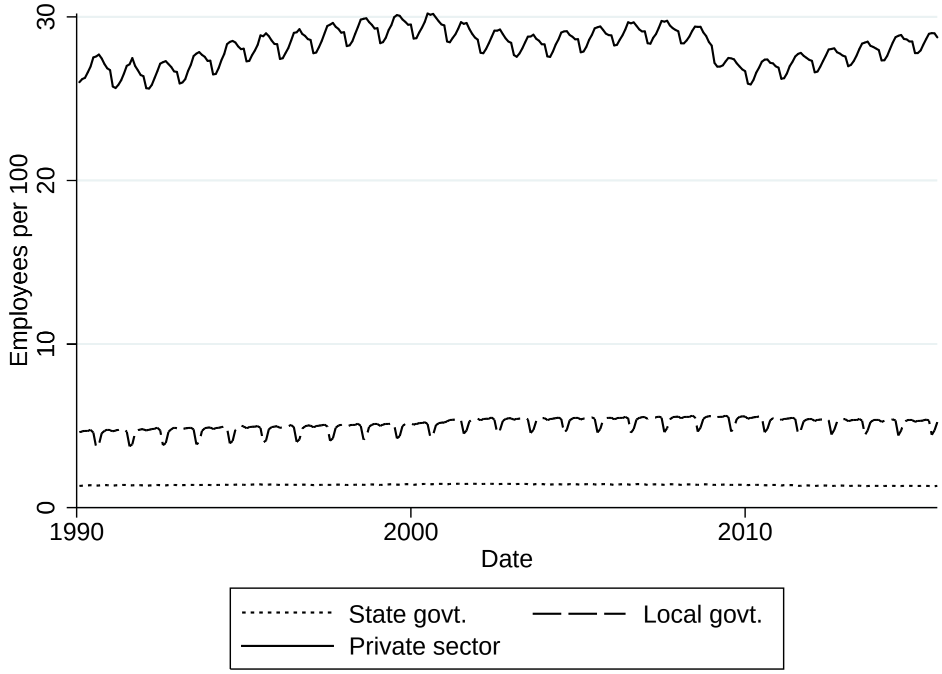


Figure 1.3: Trends in state government, local government and private sector employment.

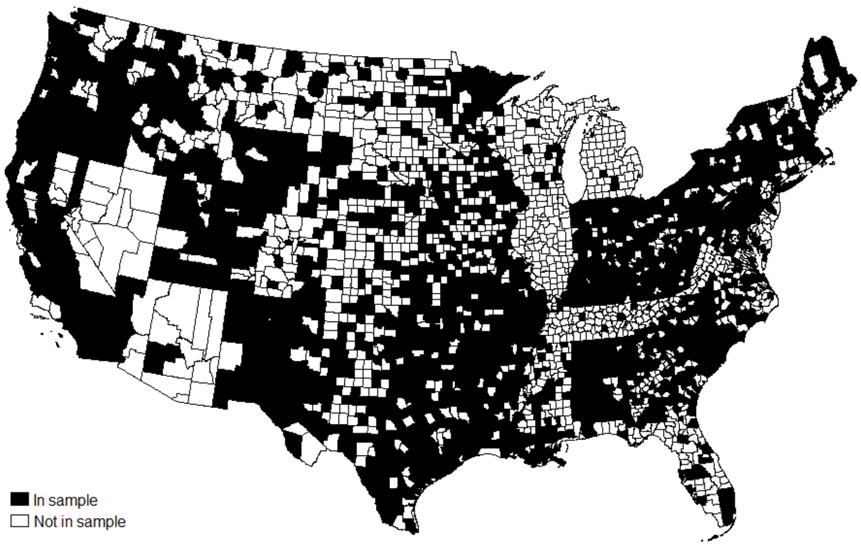


Figure 1.4: Main sample for the fixed effects specifications.

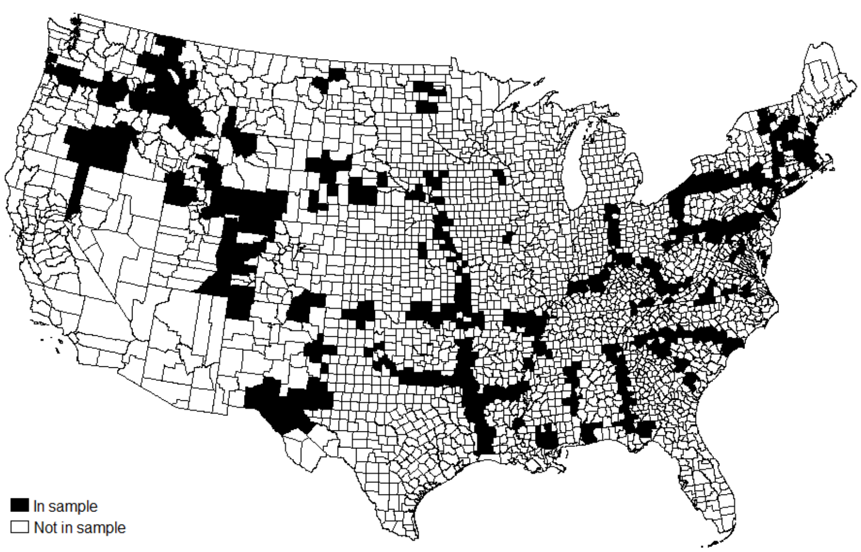


Figure 1.5: Main sample for the county-pair specifications.

1.7 Appendix B

1.7.1 Further heterogeneity results

Another potential source of heterogeneity is whether there is turnover in the party of the governor (columns 1-2 of Table 1.10). For state government, employment cycles do not depend on turnover; for local government, they are absent when the incumbent loses. The mechanisms may be similar to the case of close and term-limited elections – when the incumbent is less popular, support from local citizens and officials is likely to be less. State government, on the other hand, is more directly influenced by the incumbent. These results also suggest that 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.

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. However, it is not obvious what effect this should have. On one hand, coattail effects may reduce the incentive to manipulate for individual governors; on the other, governors likely still want to influence the presidential vote since low turnout there will imply low turnout for the gubernatorial election. The results in columns (3)-(4) of Table 1.10) show that for state government employment it does not matter. For local government employment, the pattern appears more driven by non-presidential election years, although the differences are not significant.

States with a higher than median unemployment rate may experience larger cycles. When the unemployment rate is high, electoral cycles in government employment may be different: e.g., there is a larger pool of workers to hire from; employment may be a more salient political issue, making separation more risky. In columns (5) and (6) of Table 1.10, the coefficient estimates for the interaction terms are all positive, suggesting that, indeed, the increase in employment leading up to elections appears more pronounced when the unemployment rate is high. However, for the most part the differences are not statistically significant. The exception is the post-electoral period for local government employment, where 2 to 3 quarters after an election, high unemployment states have significantly higher local government employment. That is, the post-election drop-off in employment is substantially less abrupt, consistent with the reasoning above.

I also investigate how the employment cycles observed depend on other county characteristics. To do so, I usually take a cross sectional variable such as income per capita and divide the sample into high and low income counties, based on the median within each state. Using the within state distribution of income, rather than the overall distribution, helps guards against alternative explanations of the following kind: cycles are larger in high income counties, but coastal states tend to have more high income counties, and cycles are larger in coastal states for other reasons.

First, in columns (7)-(8) of Table 1.10, I test whether the effects are driven more by counties of a particular long-term partisan leaning. In line with the partisan theories mentioned above, it may be that left-wing constituents are more swayed by manipulation than right-wing constituents. My measure of partisanship is the long-term (1984-2016) average vote share for the Republican party in presidential elections. The results suggest that local long-term partisanship largely does not matter – more or less Republican leaning parts of a state appear to experience similar effects.

The remainder of Table 1.10 and Table 1.11 investigate a range of additional socio-economic and demographic variables: urbanicity, population density, income, educational attainment, age structure, racial fractionalization, fraction of employment in goods producing compared to services, whether the state has above median state or local government employment.[39] Income and age structure do not appear to influence the magnitude of the electoral cycles, but cycles do tend to be more pronounced in more urban, more densely populated, and more educated counties. There are some quantitative differences in the magnitude of the cycles for counties with different racial compositions, different mixes of goods and services industries, and states with different levels of state and local government employment. Qualitatively however, in most cases the cycles are still generally present. Thus, gubernatorial elections appear to induce employment effects that are fairly generalized geographically.

1.7.2 Alternative specifications

One potential concern with my main specifications is serial correlation in the dependent variable over time. One reason I do not use lagged dependent variables on the right-hand side in my main specifications is because it may soak up 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 could affect the current employment rate through the lagged employment rate rather than through the election dummies. The second reason for not including lagged dependent variables is that

[39]Income and goods producing fraction are at baseline 1990, from the Bureau of Economic Analysis. The urban dummy is from the National Council for Health Statistics and the remaining variables are from the 2000 census. Racial fractionalization is calculated as a Herfindahl index of ethnic shares.

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 108 quarters, these asymptotics should come into play. In Tables 1.12 and 1.13 (columns 1 and 8) I consider specifications that include lags – the results are largely consistent with the main findings.

In columns (2) and (9) of Tables 1.12-1.13, I consider specifications with the log one-quarter growth rate as the dependent variable. In this case, a bump in employment prior to elections is likely to manifest itself as a positive growth rate before the election and a negative growth rate afterwards. The results confirm this. At some point prior to the elections we see positive coefficients. Only for local government employment in the county-pair approach is statistical significance not attained, although the sum of the coefficients in the pre-electoral period is statistically significant (e.g., the sum Elec-3 through Elec is statistically significant at the 10% level and the sum of Elec-3 through Elec-1 at the 1% level). In the quarters following elections, there are negative and significant coefficients, except in the case of local government employment in the fixed effects specifications, in which case, again, the sum of coefficients is significant (the sum of Elec through Elec+3 at the 10% level and Elec+1 through Elec+3 at the 5% level). Columns (3) and (10) of Tables 1.12-1.13 use the log of the level of employment as dependent variable, rather than employment per capita, in which case the coefficient estimates are interpreted as percentage increases in employment. Results are consistent with the main findings.

In the specifications considered prior to this point, I avoided including contemporaneous values of the control variables out of endogeneity concerns. Columns (4) and (11) of Tables 1.12-1.13 show that inferences do not change when the contemporaneous values are used instead of baseline values interacted with linear and quadratic time trends.

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. Most specifications include 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 may still be invalid. I therefore run specifications that drop counties in states that have elections in off years (i.e., odd numbered years), thus only comparing counties in states that have gubernatorial elections two years apart. These results are shown in columns (5) and (12) of Tables 1.12-1.13. Even excluding these counties, the signature pattern of increased pre-electoral employment persists. Only in the county-pair specification for local government employment is the pattern somewhat attenuated, only significant at the 10% level.

Another issue is that, in the county-pair analysis, I 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 may implicitly receive greater weight in the regressions. Weighting each county by the inverse of the number of pairs does not affect the results (columns 6 and 13 of Table 1.13).

Columns (7) and (14) of Table 1.12-1.13 show that the results are not sensitive to the fact that I have aggregated to the quarterly level – keeping the underlying data in monthly form, inferences are largely unchanged. Similarly, columns (1)-(3) and (8)-(10) of Table 1.14 show the results when I aggregate to the state-quarter, county-year, and state-year levels. The findings are consistent with the main results. Columns (4)-(7) and (11)-(14) show the results when clustering the standard errors at different levels – at the county level or two ways at the state and year level. Inferences are similar to before.

1.7.3 Election window

Because elections are periodic, widening the election window means election windows of different states will increasingly overlap. The idenfitication strategy is based on comparison of states with and without elections. However, at some point the control states will also hold elections themselves. Also, the longer the window, the less identifying variation to identify each coefficient estimate – less states are eligible as controls, reducing the possible cross-state comparisons. This is especially problematic for the county-pair approach where states need to be adjacent. In my data, most states have elections two years apart. Thus, with my preferred window there is no overlap for the majority of observations.[40] In some cases, there is overlap – some states have elections in odd numbered years. However, the results are robust to dropping these states (columns 5 and 12 of Tables 1.12 and 1.13). That is, restricting to states that have elections two years apart, and thus no overlap.

Despite these concerns, the results of extending the window by one and two leads/lags are reported in Table 1.15 for the fixed effects approach. For state government employment, none of the additional leads or lags are statistically significant. For local government, only Elec+4 attains statistical significance. The coefficient estimate is negative and small compared to the pre-electoral effect. This could be the result of post-election cuts

[40]One state's Elec+3 is two quarters before the other state's Elec-3. By widening the window, however, the first state's Elec+4 would be the same as the second state's Elec-4, and election windows begin to overlap.

in local government employment, possibly delayed due to electoral concerns.

1.7.4 Poll workers

An alternative explanation of the findings is that we may simply observe 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, as previously noted, services performed by election workers are excluded from unemployment insurance coverage if the worker makes less than $1,000 per year.[41] Given prevailing compensation rates, the vast majority of workers should fall below this threshold.[42] Moreover, most of these poll workers are hired by local governments, in particular county governments, rather than state governments.[43] In the paper, cycles show up for both local and state government. Thus, even if the local government effects are due to election workers, this is much less likely for state government employment. 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 commission, consisting of members elected or appointed to a fixed, continuous, term. These positions would be classified as local government employment. Similarly at the state level, usually election administrators are permanent employees. Even if such permanent officials work more around elections, this

[41]Title 26, United States Code §3309(b)(3)(F) (2010), which regulates under the Federal Unemployment Tax Act (FUTA) coverage of state Unemployment Insurance laws.

[42]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/, accessed 3/6/17).

[43]National Conference of State Legislatures, http://www.ncsl.org/research/elections-and-campaigns/election-administration-at-state-and-local-levels.aspx (accessed 3/6/17).

would show up in their compensation rather than in the number of employees. Additionally, the QCEW data excludes elected officials, members of a legislative body and members of the judiciary.

With state government election officials it is also likely that any effects driven by election administrators would be concentrated geographically in the state capital (local government employees on the ground would report up to the state officials). As an additional robustness check, I run a regression in which the counties containing the state capitals (and hence the majority of the state level administrative offices) are excluded. The results are in columns 6 and 13 of Table 1.12 and show that the effect is not driven by state capitals.

It is also important to note that the identification strategy makes it very unlikely that the effects are due to election workers. Despite staggered gubernatorial election cycles, all federal and state level offices up for election generally appear on a single ballot. Thus, gubernatorial elections that coincide with federal elections (which occurs in any even numbered year) are unlikely to entail significantly more election workers over and above those already manning federal elections. That is, both treatment and control states experience federal elections, and thus already have to hire poll workers, but treatment states additionally have gubernatorial elections listed on the ballot. The exception would be states that have elections in off-years, but the results are robust to excluding these states (see columns 5 and 12 of Tables 1.12-1.13).

In Section 1.3.3 I found evidence that at least some of the effects are coming from employment in justice, public order and safety, which would not include election workers. Finally, election workers are typically only compensated for work on election day and, in many cases, a training session usually held a few weeks before the election. However, the observed effects are often consistent with increased employment over a longer period of time.

1.7.5 Senate elections

One of the principal threats to identification is simultaneous 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. 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 solely by local elections. Additionally, I am 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 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 1.8. There were 467 Senate elections during my period of study.

Senate election variables are included in the model, defined analogously as in the case of gubernatorial elections.[44] The results are displayed in Table 1.16. The coefficient

[44]Special elections in the Senate are common, unlike for gubernatorial elections. Special elections are

estimates for the gubernatorial election variables are largely very similar to those obtained previously. Senate elections do not appear to matter in three of the four cases. The exception is the fixed effects approach for local government employment, where the coefficient estimates for the Senate are large, negative and significant before elections. It is not clear why, but it is not consistent with manipulation. This anomaly aside, it is not surprising that Senate elections do not appear to induce manipulation consistent effects since a senator's work is federal in nature and does not entail direct authority over state policy levers.

1.7.6 Placebo and jackknife tests

I implement two placebo tests. In the first,"Test 1," I randomize whether each state-year is a gubernatorial election year, with the restriction that no elections are held in consecutive years, so that the probability of a given state-year is assigned an election is approximately 0.25. Using these fake election dates, I then run the preferred specification. Since fake elections are randomly assigned, they should affect employment. Similarly, in "Test 2," I randomly assigns each state one of the four possible four-year election cycles.

Table 1.9 shows the results of 1000 iterations. The first row shows how often the maximum t-statistic corresponding to any of the seven election quarters using the placebo election dates exceeds in absolute value the maximum t-statistic using the real election dates. The second row shows the frequency with which the maximum placebo t-statistic exceeds in absolute value the second largest t-statistic using the real election dates. These numbers are informative about how unlikely it would be to observe findings more extreme than my

often closely related to economic conditions – e.g., senators retire when they do not expect to win re-election – creating a more serious endogeneity problem. I therefore include the lagged dependent variable to control for prevailing economic conditions, as when I had endogenous interactions in Section 1.3.3.

main results if elections had no effect on employment. These are also very conservative tests, since I only require one large t-statistic to be observed, whereas in the main results there are usually multiple large t-statistics. Additionally, no restriction is imposed on which election variable should be significant or the sign. That is, placebo effects need not be consistent with manipulation to count as "more extreme." The results suggest that similar results under the null that elections do not affect employment are very unlikely, especially for the first test (for the second, there remains a decent probability that the correct election cycle is assigned).

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 1.17 and 1.18 show the results.[45] In each case, the usual pattern appears, and remains pronounced in most cases. Similarly, I divide my sample period into six consecutive periods of 18 quarters in length and sequentially exclude each one. The results are displayed in Table 1.19 and are largely unchanged. The estimated coefficient for the election quarter is highly statistically significant in all subsamples, with some variation in the timing of when the effects kick in or drop off.

[45] 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. In any case, running the jackknife analyses using the county-pair sample generally shows the usual pattern of positive coefficient estimates leading up to elections, though sometimes they do not attain statistical significance (results not reported).

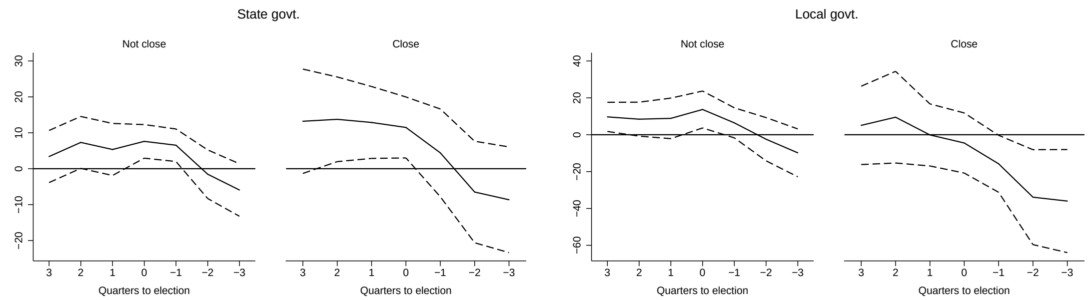
1.7.7 Alternative samples

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 quarter-to-quarter changes remain. In multiple instances, the number of employees per capita more than doubled or halved from one quarter to the next. 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 for which employment per capita in either state or local government more than doubled or halved. This leaves 1223 counties for the fixed effects sample and 450 counties (381 pairs) for the county-pair sample. The results are shown in columns (1)-(4) of Table 1.20. The usual pattern persists and, in fact, becomes even more pronounced for state government employment. These results suggest that outliers are not driving the results – if anything, they tend to add noise and work against finding effects.

I previously restricted to counties that do not have suppressed data for both state and local 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 local government employment only and relax the requirement that state data not be suppressed. Doing so allows me to increase the number of counties from 1751 to 2085 in the fixed effects sample, and from 500 to 637 (430 to 575 pairs) in the county-pair sample. The expanded samples are shown in Figures 1.9 and 1.10. In particular, I recover a substantial number of the counties in Illinois and Michigan, which were largely missing from the main sample. Columns (5)-(6) of Table 1.20 show the results. The coefficient estimates follow the usual pattern observed, are similar in magnitude, and highly statistically significant.

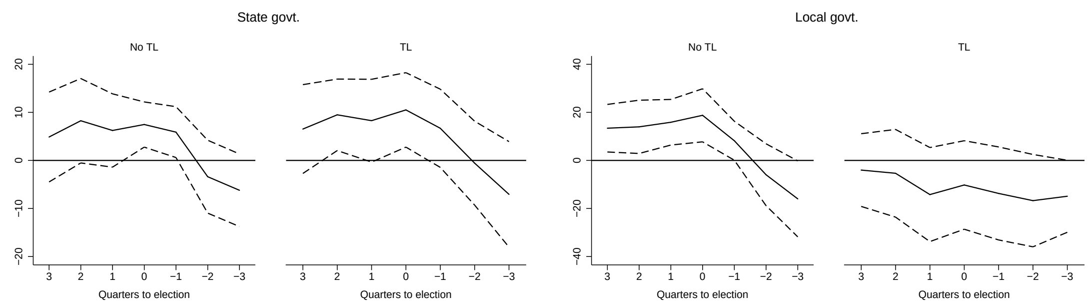
Figure 1.6: Coefficient estimates for heterogeneity by political/institutional factors. Each pair of figures corresponds to one regression: the two figures graph the marginal effects for the two types of counties. Corresponds to Table 1.3. Dashed lines represent 95% confidence intervals.

Close elections



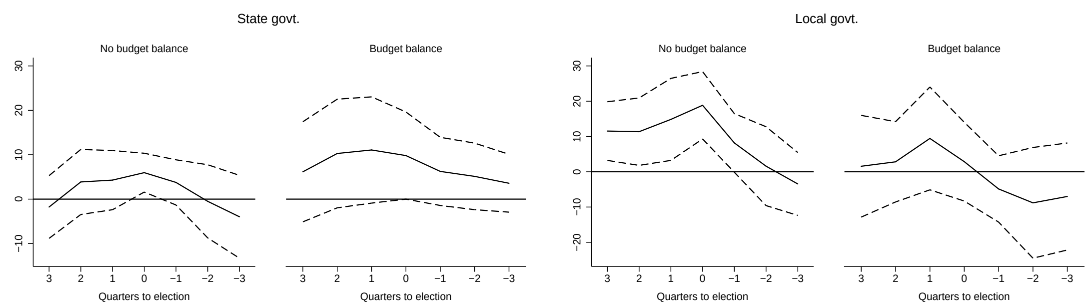
N=181876, 1751 counties. Lagged dependent variable.

Term limited incumbent



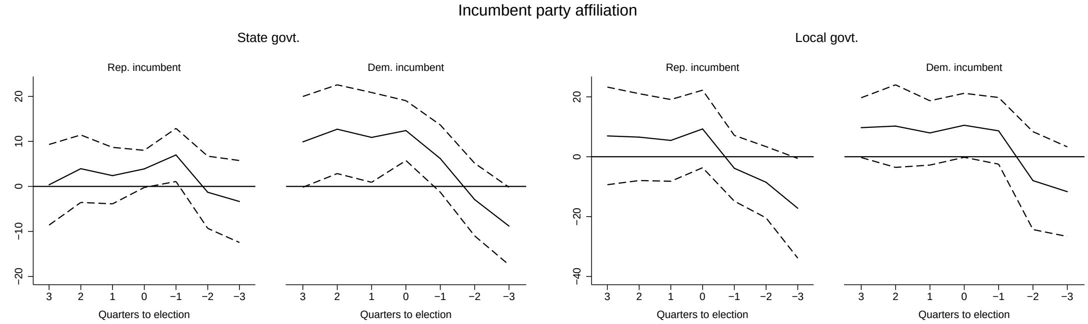
N=181876, 1751 counties. Lagged dependent variable.

Budget balance requirements

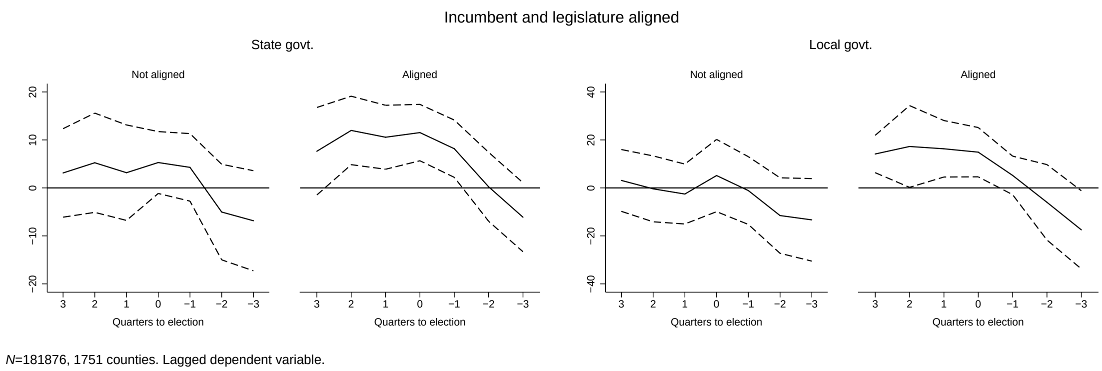


N=188880, 1751 counties.

Incumbent party affiliation



N=181876, 1751 counties. Lagged dependent variable.



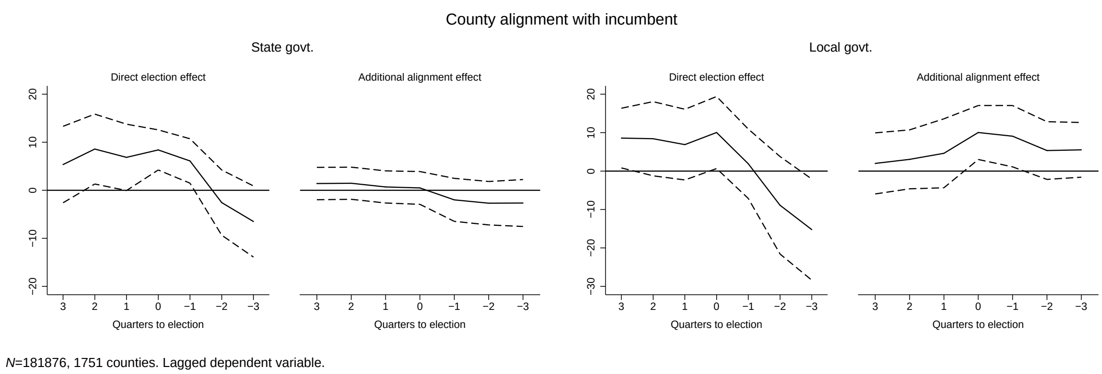


Figure 1.7: Coefficient estimates for further heterogeneity tests by political/institutional factors. Each pair of figures corresponds to one regression: the two figures graph the marginal effects for the two types of counties. Corresponds to Table 1.3. Dashed lines represent 95% confidence intervals.

Table 1.7: Summary statistics for the sample of all counties used in the fixed effects regressions.

	Mean	s.d.	Min	Median	Max
Employment					
State government	1774	5689	8	299	115526
Local government	5135	15135	153	1649	475959
Private sector	42861	138724	172	9355	3807722
Employment per capita					
State government	0.013	0.017	0.000	0.008	0.301
Local government	0.050	0.016	0.005	0.046	0.351
Private sector	0.280	0.107	0.040	0.270	1.213
Political variables					
Elec window (7 qtrs)	0.453	0.498	0.000	0.000	1.000
Senate elec window (7 qtrs)	0.610	0.488	0.000	1.000	1.000
Presidential cycle dummy	0.200	0.400	0.000	0.000	1.000
Off-years cycle dummy	0.135	0.342	0.000	0.000	1.000
Close	0.087	0.282	0.000	0.000	1.000
Incumbent term limited	0.117	0.321	0.000	0.000	1.000
Turnover	0.155	0.362	0.000	0.000	1.000
Strict balanced budget	0.380	0.485	0.000	0.000	1.000
Presidential elec. window	0.481	0.500	0.000	0.000	1.000
Dem. incumbent	0.212	0.409	0.000	0.000	1.000
Governor and legislature aligned	0.221	0.415	0.000	0.000	1.000
Aligned	-0.001	0.465	-1.000	0.000	1.000
Pop.	119634	356095	2204	35461	10294383
Income per cap.	33769	8388	8034	32375	132004
Goods fraction	0.310	0.130	0.022	0.295	0.793
Cross sectional variables					
Urban	0.297	0.457	0.000	0.000	1.000
Avg. Rep. vote share	0.572	0.107	0.174	0.578	0.891
Baseline pop. (1990)	102962	309553	2905	31565	8878157
Baseline income per cap. (1990)	27979	5909	10214	27403	62939
Baseline goods fraction (1990)	0.360	0.140	0.047	0.346	0.788
Baseline local govt. (1990)	0.046	0.012	0.006	0.044	0.112
Baseline state govt. (1990)	0.013	0.017	0.000	0.007	0.286
Pop. density (2000)	270	1457	1	56	34915
Share with bachelor's or up (2000)	0.164	0.072	0.049	0.145	0.602
Racial fractionalization (2000)	0.218	0.170	0.009	0.166	0.723
Share aged 65 up (2000)	0.143	0.035	0.039	0.141	0.315

Based on main sample of 1751 counties in 49 states, 188880 observations.

Table 1.8: Summary statistics for the sample of all counties used in the county-pair regressions.

	Mean	s.d.	Min	Median	Max
Employment					
State government	1391	3499	9	358	67032
Local government	4592	8137	160	1860	82439
Private sector	39307	82160	244	10301	806851
Employment per capita					
State government	0.012	0.013	0.000	0.008	0.285
Local government	0.048	0.017	0.005	0.045	0.348
Private sector	0.284	0.106	0.052	0.274	1.213
Political and other variables					
Elec window (7 qtrs)	0.457	0.498	0.000	0.000	1.000
Senate elec window (7 qtrs)	0.610	0.488	0.000	1.000	1.000
Presidential cycle dummy	0.202	0.402	0.000	0.000	1.000
Off-years cycle dummy	0.143	0.350	0.000	0.000	1.000
Close	0.091	0.287	0.000	0.000	1.000
Incumbent term limited	0.127	0.332	0.000	0.000	1.000
Turnover	0.163	0.369	0.000	0.000	1.000
Strict balanced budget	0.403	0.491	0.000	0.000	1.000
Presidential elec. window	0.481	0.500	0.000	0.000	1.000
Dem. incumbent	0.231	0.422	0.000	0.000	1.000
Governor and legislature aligned	0.223	0.416	0.000	0.000	1.000
Aligned	0.003	0.473	-1.000	0.000	1.000
Pop.	108881	196193	2204	40186	1604836
Income per cap.	33808	8718	8143	32132	132004
Goods fraction	0.313	0.131	0.035	0.297	0.793
Cross sectional variables					
Urban	0.286	0.452	0.000	0.000	1.000
Avg. Rep. vote share	0.570	0.107	0.174	0.574	0.882
Baseline pop. (1990)	91403	181245	2905	33863	1584293
Baseline income per cap. (1990)	28008	6212	16117	27138	62620
Baseline goods fraction (1990)	0.370	0.144	0.047	0.357	0.719
Baseline local govt. (1990)	0.045	0.012	0.006	0.042	0.096
Baseline state govt. (1990)	0.012	0.015	0.001	0.007	0.182
Pop. density (2000)	294	1618	1	56	31707
Share with bachelor's or up (2000)	0.164	0.075	0.055	0.145	0.543
Racial fractionalization (2000)	0.215	0.166	0.012	0.154	0.715
Share aged 65 up (2000)	0.143	0.032	0.049	0.142	0.259

Based on sample of 500 counties, forming 430 county-pairs, along 71 border segments, in 43 states, 92880 observations.

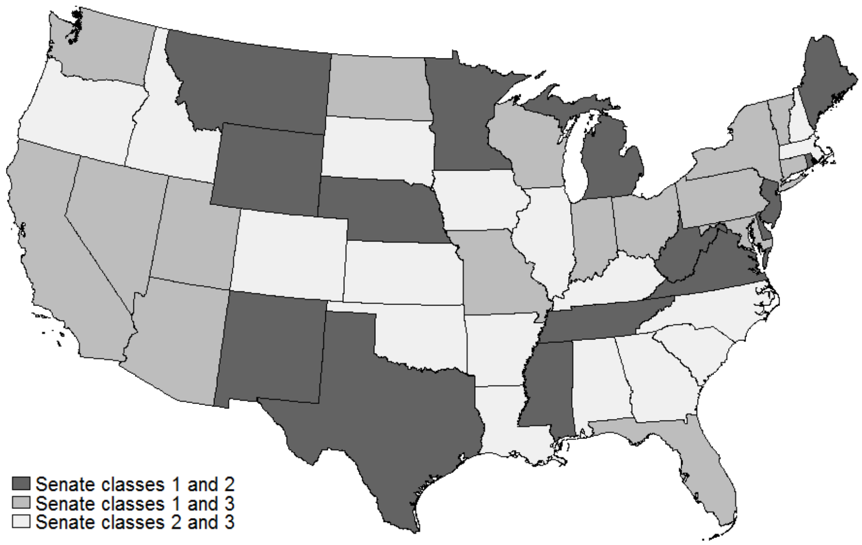


Figure 1.8: Distribution of US Senate election classes.

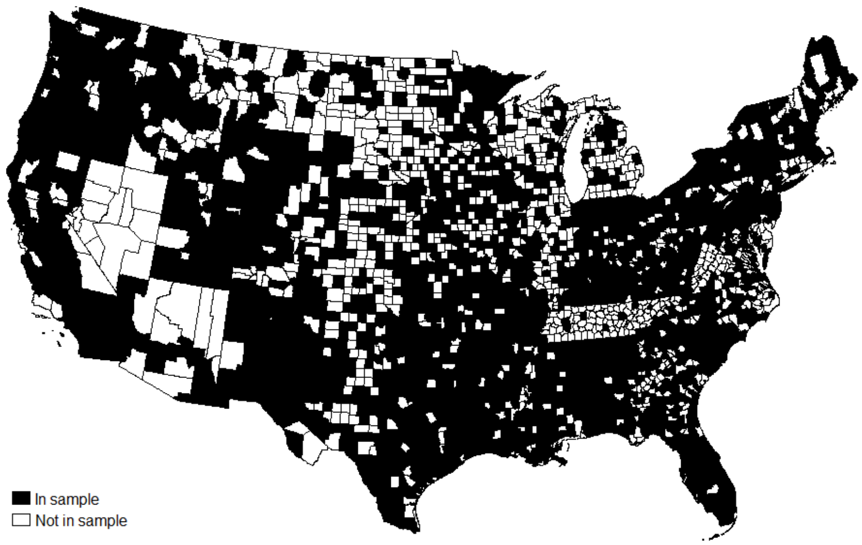


Figure 1.9: Alternative sample for the fixed effects specifications, based on all counties for which local government employment is available.

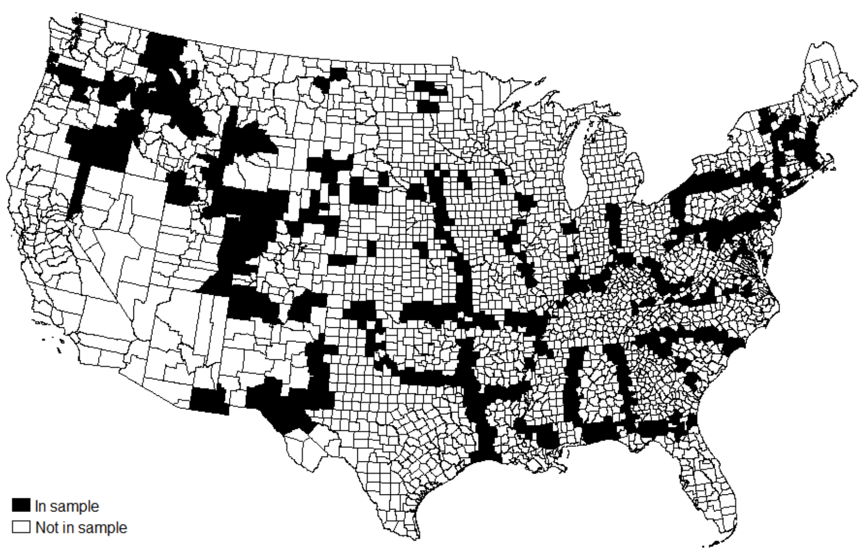


Figure 1.10: Alternative sample for the county-pairs specifications, based on all counties for which local government employment is available.

Table 1.9: Placebo tests.

	# of times observed							
	State govt.				Local govt.			
	Test 1		Test 2		Test 1		Test 2	
Regression outcome	FE	BP	FE	BP	FE	BP	FE	BP
Placebo t-stat exceeds largest actual t-stat	0	19	6	190	0	2	10	170
Placebo t-stat exceeds second largest actual t-stat	40	120	62	246	20	96	62	216

Based on 1000 iterations, using preferred specifications. Test 1 assigns each state-year a gubernatorial election such that no elections are in consecutive years and the probability of an election is approximately 0.25. Test 2 assigns each state a four year electoral cycle with probability 0.25 each. "Placebo t-stat exceeds actual (second) largest t-stat " means that the largest t-statistic (in absolute value) from the placebo regression exceeded the (second) largest t-stat in the true regression. For the fixed effects approach, the relevant t-stats are 3.34 and 2.34 for state government, 4.87 and 2.97 for local government; for the county-pair approach they 2.14 and 1.99 for state, 3.63 and 2.57 for local.

Table 1.10: Further heterogeneity tests.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	X=Turnover		X=Pres. elec.		X=High unemp.		X=Rep. leaning		X=Urban		X=High dens.	
	State	Local	State	Local	State	Local	State	Local	State	Local	State	Local
Elec-3	6.06	15.06***	-1.48	5.64	8.00	12.20*	2.90	7.78*	0.69	7.88*	-2.84	5.32
	(4.18)	(5.04)	(5.87)	(7.04)	(5.38)	(7.25)	(3.71)	(4.60)	(3.04)	(4.15)	(3.36)	(5.13)
Elec-2	8.19*	17.38**	6.69	7.27	10.53**	7.41	9.28**	6.59*	5.85*	8.28**	2.35	2.52
	(4.09)	(7.49)	(5.75)	(8.47)	(4.94)	(8.14)	(3.92)	(3.55)	(3.41)	(3.48)	(3.85)	(4.36)
Elec-1	5.22	16.92***	7.81	20.13**	8.19*	3.77	9.83**	11.35**	6.43*	12.75***	3.41	6.24
	(3.53)	(4.91)	(5.62)	(9.44)	(4.25)	(6.96)	(3.92)	(4.72)	(3.41)	(4.41)	(3.83)	(5.64)
Elec	5.97**	17.19***	6.86**	17.28***	4.90*	7.97	9.33***	11.53***	6.86***	13.21***	4.96*	11.24***
	(2.74)	(6.31)	(2.85)	(2.91)	(2.52)	(7.70)	(2.85)	(3.70)	(2.24)	(2.76)	(2.85)	(3.52)
Elec+1	6.12**	5.02	2.51	-0.98	2.80	-5.90	6.71**	3.17	4.35**	2.71	1.85	-1.67
	(2.83)	(4.00)	(6.43)	(8.35)	(3.22)	(7.81)	(2.87)	(4.21)	(2.11)	(3.59)	(2.96)	(4.00)
Elec+2	0.14	-8.45	4.41	2.72	-3.73	-23.09***	1.89	-3.85	1.14	-3.24	-0.87	-9.12*
	(3.28)	(6.23)	(5.97)	(12.28)	(3.71)	(8.56)	(3.74)	(5.26)	(3.29)	(4.29)	(3.66)	(5.41)
Elec+3	-2.27	-18.49**	3.41	1.05	-7.76*	-30.97***	-0.25	-4.09	-1.57	-5.49	-4.05	-9.54*
	(3.87)	(7.35)	(5.98)	(9.86)	(4.41)	(11.21)	(4.31)	(5.13)	(3.57)	(3.75)	(3.65)	(4.93)
X·Elec-3	-1.95	-18.46**	6.06	4.36	-5.02	-6.94	-3.60	0.10	17.74**	0.93	7.97**	5.06
	(4.92)	(8.94)	(10.52)	(12.87)	(5.70)	(9.10)	(3.98)	(7.88)	(7.82)	(17.20)	(3.42)	(4.53)
X·Elec-2	0.92	-25.25*	-0.31	4.09	-3.65	1.38	-6.05	3.21	16.54**	0.76	7.85**	11.46**
	(5.09)	(13.79)	(9.72)	(18.49)	(5.58)	(11.37)	(3.63)	(7.71)	(7.69)	(17.06)	(3.11)	(4.61)
X·Elec-1	4.27	-27.95***	-1.18	-17.22	-2.52	5.21	-6.03*	3.02	15.57*	7.40	6.84**	13.41***
	(5.72)	(10.27)	(10.23)	(18.84)	(4.99)	(7.69)	(3.38)	(6.88)	(8.14)	(17.00)	(3.35)	(4.59)
X·Elec	6.76	-19.93*	1.25	-10.95	6.41	3.79	-3.99	3.31	17.87**	-0.54	4.79	3.95
	(5.65)	(10.17)	(4.46)	(7.84)	(4.05)	(9.44)	(2.86)	(6.57)	(7.65)	(11.06)	(2.87)	(3.71)
X·Elec+1	-0.10	-10.15	4.01	8.14	5.59	13.44	-3.70	0.36	17.38***	24.54	6.07*	10.16**
	(5.08)	(7.55)	(9.94)	(13.29)	(4.00)	(8.77)	(3.09)	(4.65)	(6.01)	(15.78)	(3.21)	(4.21)
X·Elec+2	-7.89	-2.57	-5.44	-10.22	1.86	25.44**	-0.47	3.13	17.78***	37.08**	5.09	13.85***
	(5.81)	(7.15)	(10.77)	(21.00)	(4.98)	(10.09)	(2.45)	(5.84)	(5.33)	(14.62)	(3.73)	(4.89)
X·Elec+3	-12.16*	8.22	-8.88	-9.95	2.12	28.51**	-1.76	-1.48	14.72**	24.48	5.87	9.52*
	(6.68)	(8.09)	(11.36)	(16.77)	(5.76)	(12.24)	(2.72)	(5.19)	(6.16)	(15.11)	(4.28)	(5.36)
N	181876	181876	188880	188880	181876	181876	188880	188880	188880	188880	188880	188880
R^2	0.99	0.98	0.97	0.88	0.99	0.98	0.97	0.88	0.97	0.88	0.97	0.88

1751 counties, 49 states. Dependent variable is employment per capita; coefficients, standard errors multiplied by 100 thousand. Preferred specification (column 2, Table 1.1) plus lagged dependent variable in columns (1)-(2) and (5)-(8). Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.11: Further heterogeneity tests.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	X=High inc.		X=High edu.		X=High age		X=High frac.		X=High goods		X=High st. gov.		X=High loc. gov.	
	State	Local	State	Local	State	Local	State	Local	State	Local	State	Local	State	Local
Elec-3	0.57	8.79**	-0.57	4.31	1.53	6.65	1.96	8.12*	3.30	9.36**	3.54	4.65	9.72*	7.57*
	(3.74)	(4.36)	(3.40)	(5.04)	(2.83)	(5.04)	(3.30)	(4.10)	(3.60)	(4.11)	(4.65)	(6.81)	(4.88)	(4.41)
Elec-2	6.07	8.91***	3.78	4.05	5.41*	7.20	7.40**	10.90***	9.64**	9.24**	8.14*	0.85	13.07***	16.82**
	(3.86)	(3.26)	(3.82)	(3.70)	(3.22)	(4.53)	(3.32)	(3.60)	(3.88)	(4.02)	(4.73)	(6.55)	(4.39)	(8.26)
Elec-1	7.01*	11.67**	3.67	8.00	6.05*	12.39**	8.07**	15.86***	10.38**	13.41***	7.74**	4.57	11.65***	5.84
	(3.77)	(4.42)	(4.03)	(5.08)	(3.24)	(5.76)	(3.46)	(3.95)	(4.14)	(4.53)	(3.80)	(6.55)	(4.30)	(5.06)
Elec	6.70**	14.22***	4.55	11.34***	5.99***	14.51***	8.75***	13.50***	9.99***	13.01***	7.82**	6.57	14.47***	10.44*
	(2.70)	(3.18)	(2.83)	(3.27)	(2.06)	(3.65)	(2.35)	(2.71)	(2.96)	(2.73)	(3.42)	(6.47)	(2.82)	(5.91)
Elec+1	4.36	1.73	1.37	1.44	2.76	1.29	5.72**	6.05	8.49***	3.10	4.22	-0.21	7.64**	4.17
	(3.05)	(4.20)	(2.85)	(4.68)	(2.21)	(4.07)	(2.61)	(4.01)	(2.37)	(3.35)	(4.17)	(4.99)	(2.93)	(5.75)
Elec+2	2.44	-4.06	-0.95	-5.44	0.93	-4.25	1.78	4.38	4.37	-2.24	0.75	-9.92	-2.14	-10.24
	(3.84)	(5.28)	(3.62)	(5.54)	(3.12)	(4.47)	(3.61)	(4.43)	(3.20)	(4.16)	(4.06)	(7.41)	(4.09)	(12.56)
Elec+3	-0.12	-7.56	-4.70	-7.13	-1.74	-6.89	-1.52	1.45	1.09	-4.67	-3.58	-18.87***	-7.19	-8.18
	(3.78)	(4.66)	(3.54)	(4.48)	(3.10)	(4.75)	(4.25)	(3.61)	(3.79)	(4.07)	(3.65)	(6.90)	(5.10)	(9.16)
X·Elec-3	1.07	-1.94	3.41	7.18	-0.84	2.34	-1.74	-0.59	-4.45	-3.10	3.59	7.49	-8.79	1.12
	(3.95)	(4.96)	(3.17)	(4.75)	(3.33)	(4.83)	(3.21)	(4.46)	(3.23)	(4.63)	(6.44)	(9.64)	(5.54)	(7.68)
X·Elec-2	0.33	-1.45	5.01	8.45*	1.62	1.97	-2.37	-5.46	-6.90**	-2.11	0.90	14.94	-8.98*	-17.01
	(3.55)	(4.32)	(3.21)	(5.02)	(3.42)	(5.39)	(3.10)	(5.02)	(3.14)	(4.04)	(6.35)	(11.14)	(5.27)	(11.70)
X·Elec-1	-0.43	2.43	6.38*	9.95**	1.49	0.97	-2.57	-6.02	-7.25**	-1.07	-1.74	3.83	-9.52*	0.80
	(3.61)	(3.97)	(3.35)	(4.94)	(3.14)	(4.89)	(3.44)	(4.18)	(3.32)	(4.40)	(5.98)	(10.98)	(5.10)	(6.70)
X·Elec	1.26	-2.08	5.65*	3.78	2.64	-2.60	-2.89	-0.62	-5.39*	0.38	1.02	6.08	-12.15**	-1.53
	(3.05)	(3.89)	(2.94)	(4.45)	(2.49)	(3.76)	(2.81)	(3.30)	(2.88)	(3.01)	(5.03)	(11.65)	(4.72)	(7.73)
X·Elec+1	0.98	3.28	7.09**	3.89	4.13	4.07	-1.76	-5.46	-7.38**	0.51	2.33	-0.47	-4.50	-9.92
	(3.42)	(3.49)	(3.13)	(4.86)	(3.17)	(3.54)	(3.39)	(4.01)	(3.03)	(3.13)	(5.74)	(11.99)	(5.03)	(7.23)
X·Elec+2	-1.60	3.62	5.29	6.46	1.42	3.92	-0.28	-13.43***	-5.51*	-0.07	-5.24	-1.47	0.70	-1.69
	(3.73)	(3.79)	(3.48)	(4.66)	(3.90)	(4.48)	(3.01)	(4.14)	(3.14)	(3.09)	(6.48)	(14.02)	(6.77)	(14.51)
X·Elec+3	-2.07	5.51	7.24*	4.68	1.17	4.06	0.75	-12.70***	-4.53	-0.34	-5.81	6.25	1.86	-15.37
	(4.02)	(4.10)	(3.76)	(3.79)	(4.46)	(5.46)	(3.43)	(4.56)	(3.08)	(3.48)	(7.30)	(11.99)	(7.60)	(9.48)
N	188880	188880	188880	188880	188880	188880	188880	188880	188880	188880	178069	178069	178069	178069
R^2	0.97	0.88	0.97	0.88	0.97	0.88	0.97	0.88	0.97	0.88	0.99	0.98	0.99	0.98

1751 counties, 49 states. Dependent variable is employment per capita; coefficients, standard errors multiplied by 100 thousand. Preferred specification (column 2, Table 1.1) plus lagged dependent variable in columns (11)-(14). Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.12: Alternative specifications for the fixed effects approach.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	State government							Local government						
1-yr lagged d.v.	0.88^{***}							0.91^{***}						
	(0.01)							(0.03)						
Elec-3	5.36	0.05	0.25	1.30	3.73	0.99	1.15	8.58^{**}	0.13^{***}	0.15^{*}	7.47^{*}	8.65	9.97^{**}	7.81^{*}
	(3.96)	(0.21)	(0.32)	(2.87)	(3.50)	(2.88)	(2.97)	(3.86)	(0.04)	(0.08)	(4.36)	(6.28)	(4.17)	(4.05)
Elec-2	8.59^{**}	0.35^{***}	0.61^{*}	6.47^{*}	9.25^{**}	5.77^{*}	6.20^{*}	8.41^{*}	0.03	0.18^{*}	7.76^{**}	6.75	10.50^{***}	8.02^{**}
	(3.62)	(0.12)	(0.36)	(3.25)	(3.93)	(3.27)	(3.35)	(4.79)	(0.08)	(0.10)	(3.82)	(4.53)	(3.31)	(3.42)
Elec-1	6.86^{*}	0.03	0.64^{*}	7.05^{**}	10.28^{***}	6.18^{*}	7.18^{**}	6.88	0.09	0.27^{***}	12.37^{***}	15.77^{**}	14.99^{***}	14.67^{***}
	(3.43)	(0.08)	(0.34)	(3.28)	(3.73)	(3.31)	(2.89)	(4.57)	(0.10)	(0.08)	(4.37)	(6.86)	(3.95)	(4.78)
Elec	8.40^{***}	-0.05	0.66^{***}	7.55^{***}	8.57^{***}	6.29^{***}	7.13^{***}	10.01^{**}	-0.03	0.25^{***}	12.60^{***}	11.45^{***}	15.23^{***}	12.21^{***}
	(2.08)	(0.17)	(0.19)	(2.12)	(3.01)	(2.10)	(2.06)	(4.66)	(0.07)	(0.07)	(3.03)	(3.51)	(2.87)	(2.76)
Elec+1	6.11^{**}	-0.09	0.40^{**}	5.37^{**}	4.60^{*}	4.04^{*}	4.88^{**}	1.87	-0.07	0.13	3.08	0.22	4.01	3.33
	(2.30)	(0.15)	(0.20)	(2.02)	(2.59)	(2.09)	(2.07)	(4.49)	(0.06)	(0.08)	(3.69)	(3.53)	(3.73)	(3.53)
Elec+2	-2.55	-0.27	0.14	2.10	4.21	2.02	1.51	-8.90	-0.08	0.05	-2.44	-4.68	-1.73	-2.70
	(3.38)	(0.22)	(0.32)	(3.17)	(2.84)	(2.89)	(3.25)	(6.30)	(0.08)	(0.10)	(4.26)	(5.25)	(4.42)	(4.21)
Elec+3	-6.49^{*}	-0.34^{***}	-0.21	-0.78	1.91	-0.33	-0.49	-15.23^{**}	-0.04	-0.01	-4.90	-5.15	-4.19	-3.72
	(3.68)	(0.12)	(0.33)	(3.46)	(2.52)	(3.03)	(2.40)	(6.54)	(0.07)	(0.09)	(3.74)	(5.34)	(3.92)	(3.67)
N	181876	187129	188880	188880	163392	184676	568391	181876	187129	188880	188880	163392	184676	568391
R^2	0.99	0.41	0.95	0.97	0.97	0.95	0.97	0.98	0.76	0.99	0.88	0.88	0.88	0.86
Controls·trends	Y	Y	Y		Y	Y	Y	Y	Y	Y		Y	Y	Y
Log growth rate		Y							Y					
Log level			Y							Y				
Contemp. controls				Y							Y			
Drop odd years					Y							Y		
Drop capitals						Y							Y	
Monthly							Y							Y

Based on sample of 1751 counties in 49 states. Dependent variable is employment per capita (in columns 2 and 9, the log growth rate); except for lagged dependent variable, coefficients and standard errors multiplied by 100 thousand (by 100 in columns 2 and 9). All specifications include county-quarter and year-quarter fixed effects. Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.13: Alternative specifications for the county-pair approach.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	State government							Local government						
1-yr lagged d. v.	0.85***							0.98***						
	(0.03)							(0.03)						
Elec-3	2.45	0.10	0.77**	3.79	10.79**	5.91	4.22	10.22	0.16	0.14	9.07	2.41	6.57	6.51
	(4.99)	(0.38)	(0.33)	(3.73)	(4.36)	(3.64)	(3.79)	(6.68)	(0.10)	(0.13)	(6.31)	(7.43)	(5.34)	(5.49)
Elec-2	8.12**	0.47***	1.26***	7.50*	12.45***	9.32**	8.27**	19.26**	0.09	0.23*	12.14**	6.44	12.30**	10.61**
	(3.70)	(0.15)	(0.33)	(3.75)	(4.44)	(3.76)	(3.85)	(7.87)	(0.12)	(0.12)	(5.65)	(5.95)	(5.47)	(5.24)
Elec-1	3.55	-0.22	1.06***	5.57*	9.35**	7.08**	7.33**	11.28**	0.11	0.36***	17.04***	13.40*	17.94***	15.78***
	(3.38)	(0.15)	(0.31)	(3.14)	(4.20)	(3.10)	(3.53)	(4.97)	(0.09)	(0.11)	(4.37)	(7.01)	(4.68)	(4.55)
Elec	3.04	-0.20	1.03***	5.34	9.67*	6.19*	5.85	7.02	-0.16*	0.23**	10.46**	4.26	8.96***	8.07**
	(3.70)	(0.17)	(0.25)	(3.46)	(5.18)	(3.38)	(3.50)	(4.47)	(0.09)	(0.09)	(4.01)	(5.26)	(3.26)	(3.20)
Elec+1	-3.06	-0.28	0.39	0.46	-2.39	2.28	1.45	3.65	0.03	0.18	5.66	3.29	0.42	3.23
	(3.44)	(0.38)	(0.33)	(3.33)	(4.86)	(3.24)	(3.50)	(5.55)	(0.16)	(0.16)	(5.12)	(5.73)	(5.20)	(4.88)
Elec+2	-6.45*	-0.11	0.27	-0.97	0.28	1.12	0.58	-14.49*	-0.12	0.05	-2.27	-1.89	-5.25	-3.08
	(3.59)	(0.16)	(0.35)	(3.47)	(4.63)	(3.19)	(3.58)	(7.74)	(0.12)	(0.18)	(5.95)	(6.70)	(6.50)	(6.14)
Elec+3	-7.81*	-0.59***	-0.33	-3.80	-1.74	-2.06	-2.21	-15.88**	0.07	0.11	1.43	2.43	-0.07	-0.59
	(4.05)	(0.18)	(0.32)	(2.69)	(3.57)	(2.56)	(1.92)	(6.88)	(0.11)	(0.14)	(4.71)	(5.24)	(5.16)	(3.76)
N	89440	92020	92880	92880	68688	92880	279500	89440	92020	92880	92880	68688	92880	279500
R^2	0.99	0.69	0.99	0.98	0.97	0.98	0.97	0.99	0.87	1.00	0.90	0.89	0.92	0.89
Controls·trends	Y	Y	Y		Y	Y	Y	Y	Y	Y		Y	Y	Y
Log growth rate		Y							Y					
Log level			Y							Y				
Contemp. controls				Y							Y			
Drop odd years					Y							Y		
Weight by pairs						Y							Y	
Monthly							Y							Y

Based on sample of 500 counties, 430 county-pairs, 71 border segments, 43 states. Dependent variable is employment per capita (in columns (2) and (8), the log growth rate); except for lagged dependent variable, coefficients and standard errors multiplied by 100 thousand (by 100 in columns 2 and 8). All specifications include county-quarter and pair-year-quarter fixed effects. Standard errors (in parentheses) two way clustered at the state and border segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.14: Alternative levels of aggregation and clustering.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	State government							Local government						
Pre Elec Yr		3.88	8.28						0.38	-0.58				
		(3.45)	(6.66)						(5.11)	(4.47)				
Elec Yr		6.65*	11.65**						10.39**	8.08**				
		(3.62)	(5.76)						(4.29)	(3.89)				
Post Elec Yr		2.94	9.13						-1.85	3.14				
		(3.71)	(5.96)						(2.41)	(3.53)				
Elec-3	4.24			1.10	1.10	4.20	4.20**	4.07			7.83***	7.83**	6.56	6.56
	(4.45)			(1.55)	(1.43)	(3.84)	(1.77)	(3.81)			(2.19)	(3.66)	(5.23)	(9.05)
Elec-2	9.13**			6.23***	6.23***	8.25**	8.25***	4.34			8.19***	8.19	10.66*	10.66
	(4.36)			(1.55)	(1.19)	(3.83)	(0.99)	(3.50)			(2.30)	(5.90)	(5.54)	(10.36)
Elec-1	11.59**			6.80***	6.80***	6.44*	6.44***	13.02***			12.88***	12.88***	15.94***	15.94***
	(4.42)			(1.54)	(1.03)	(3.30)	(1.31)	(3.82)			(2.30)	(1.77)	(4.51)	(3.13)
Elec	9.78**			7.32***	7.32***	5.55*	5.55***	11.06***			13.20***	13.20***	8.65**	8.65
	(4.81)			(1.30)	(1.42)	(3.30)	(1.36)	(3.15)			(1.81)	(4.22)	(3.49)	(6.85)
Elec+1	5.80			4.85***	4.85**	1.43	1.43	6.92			3.35*	3.35	3.31	3.31
	(5.26)			(1.38)	(1.84)	(3.49)	(1.56)	(4.72)			(1.88)	(6.08)	(4.78)	(8.25)
Elec+2	5.90			1.64	1.64	0.56	0.56	2.14			-2.27	-2.27	-3.00	-3.00
	(3.74)			(1.53)	(1.84)	(3.62)	(0.74)	(4.68)			(2.21)	(8.95)	(6.59)	(9.18)
Elec+3	5.29			-1.14	-1.14	-2.13	-2.13	1.17			-4.83**	-4.83	1.14	1.14
	(4.05)			(1.63)	(1.81)	(2.71)	(2.17)	(3.83)			(2.14)	(4.01)	(4.71)	(4.46)
N	5284	47220	1321	188880	188880	92880	92880	5284	47220	1321	188880	188880	92880	92880
R^2	0.99	0.97	0.99	0.97	0.97	0.97	0.97	0.97	0.88	0.97	0.88	0.88	0.90	0.90
State-quarter level	Y							Y						
County-year level		Y							Y					
State-year level			Y							Y				
Cluster county (FE)				Y							Y			
Cluster year (FE)					Y							Y		
Cluster county (BP)						Y							Y	
Cluster year (BP)							Y							Y

Fixed effects regressions based on sample of 1751 counties in 49 states; county-pair based on sample of 500 counties, 430 county-pairs, 71 border segments, 43 states. Dependent variable is employment per capita, coefficients and standard errors multiplied by 100 thousand. See text for details on aggregation and clustering. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.15: Alternative specifications with a longer window.

	(1)	(2)	(3)	(4)	(5)	(6)
	State govt.			Local govt.		
Elec-5			4.46			3.16
			(3.82)			(7.35)
Elec-4		3.48	3.50		-0.20	-0.18
		(3.67)	(3.68)		(4.55)	(4.58)
Elec-3	1.10	1.10	-0.32	7.83*	7.82*	5.32
	(2.98)	(2.98)	(3.43)	(4.06)	(4.06)	(3.78)
Elec-2	6.23*	6.23*	6.23*	8.19**	8.19**	8.19**
	(3.37)	(3.37)	(3.37)	(3.42)	(3.42)	(3.42)
Elec-1	6.80*	6.80*	8.47**	12.88***	12.87***	14.06**
	(3.38)	(3.39)	(4.15)	(4.33)	(4.33)	(6.20)
Elec	7.32***	7.86**	7.87**	13.20***	11.66***	11.67***
	(2.19)	(3.19)	(3.19)	(2.71)	(3.29)	(3.30)
Elec+1	4.85**	4.84**	3.93	3.35	3.34	1.73
	(2.07)	(2.08)	(2.69)	(3.52)	(3.53)	(4.47)
Elec+2	1.64	1.64	1.64	-2.27	-2.28	-2.28
	(3.26)	(3.26)	(3.26)	(4.20)	(4.21)	(4.21)
Elec+3	-1.14	-1.15	1.54	-4.83	-4.85	-2.94
	(3.55)	(3.55)	(5.03)	(3.70)	(3.71)	(3.65)
Elec+4		-1.13	-1.12		-6.21**	-6.20**
		(3.76)	(3.76)		(2.59)	(2.59)
Elec+5			-2.30			-4.05
			(2.73)			(4.54)
N	188880	188880	188880	188880	188880	188880
R^2	0.97	0.97	0.97	0.88	0.88	0.88
Fixed effects	Y	Y	Y	Y	Y	Y
Preferred specification	Y			Y		

Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. Columns 1 and 3 use preferred specifications (columns 2 and 5 of Table 1.1 in the text). Standard errors (in parentheses) clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 1.16: Fixed effects and county-pair results for Senate elections.

	(1)	(2)	(3)	(4)
	State govt.		Local govt.	
Elec-3	5.35	2.40	8.49**	10.40
	(3.95)	(5.00)	(3.93)	(6.67)
Elec-2	8.58**	7.98**	8.25*	19.19**
	(3.62)	(3.73)	(4.75)	(7.86)
Elec-1	6.83*	3.45	6.78	11.34**
	(3.43)	(3.38)	(4.56)	(4.93)
Elec	8.38***	3.00	9.99**	7.26
	(2.07)	(3.70)	(4.68)	(4.44)
Elec+1	6.09**	-3.05	1.69	3.27
	(2.30)	(3.48)	(4.53)	(5.51)
Elec+2	-2.57	-6.50*	-9.04	-14.64*
	(3.38)	(3.65)	(6.33)	(7.71)
Elec+3	-6.50*	-7.87*	-15.24**	-16.04**
	(3.68)	(4.07)	(6.55)	(6.88)
SenElec-3	-2.10	-3.83	-14.81**	10.13
	(2.65)	(5.53)	(5.78)	(9.96)
SenElec-2	-1.16	-8.59	-21.63***	-3.40
	(2.68)	(6.39)	(5.22)	(10.66)
SenElec-1	-2.85	-5.81	-11.18*	3.20
	(2.67)	(6.25)	(5.58)	(10.85)
SenElec	-3.28	-3.48	-6.09	12.84
	(2.67)	(4.80)	(5.87)	(8.94)
SenElec+1	-0.63	1.88	-6.77	-23.47
	(3.21)	(4.72)	(6.78)	(14.89)
SenElec+2	-0.14	-1.24	-0.73	-8.98
	(3.44)	(4.58)	(5.95)	(12.36)
SenElec+3	0.56	-2.51	7.04	-9.46
	(3.16)	(7.21)	(6.10)	(10.81)
N	181876	89440	181876	89440
R^2	0.99	0.99	0.98	0.99
Fixed effects	Y		Y	
County-pair		Y		Y

Columns (1)-(2) based on sample of 1751 counties in 49 states; columns (3)-(4) based on sample of 500 counties, forming 430 county-pairs, along 71 border segments, in 43 states. Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. Preferred specifications (Table 1.1, columns 2 and 5). 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 1.17: Results of sequentially excluding each of the nine census divisions for state government employment in the fixed effects approach.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					State government				
Elec-3	0.85	0.35	-0.01	1.45	-0.20	2.81	2.32	2.52	0.20
	(3.00)	(2.99)	(3.27)	(3.45)	(3.37)	(3.29)	(3.19)	(2.71)	(3.08)
Elec-2	6.06*	5.47	4.84	7.31*	4.70	7.85**	7.78**	7.07**	5.12
	(3.39)	(3.38)	(3.71)	(3.89)	(3.47)	(3.62)	(3.70)	(3.38)	(3.46)
Elec-1	6.58*	6.10*	6.02	7.36*	5.50	8.77**	8.20**	7.19**	5.62
	(3.40)	(3.45)	(3.89)	(3.88)	(3.41)	(3.44)	(3.78)	(3.48)	(3.46)
Elec	7.19***	6.57***	7.52***	8.61***	6.48***	7.13**	8.26***	7.79***	6.42***
	(2.19)	(2.25)	(2.46)	(2.43)	(1.63)	(2.73)	(2.50)	(2.17)	(2.17)
Elec+1	4.78**	4.16*	4.74*	6.45**	3.58*	4.83**	5.15**	5.65***	4.45*
	(2.10)	(2.12)	(2.40)	(2.41)	(1.84)	(2.29)	(2.34)	(2.02)	(2.24)
Elec+2	1.41	1.52	0.59	3.26	-0.44	4.20*	1.74	1.88	1.20
	(3.29)	(3.34)	(3.53)	(3.80)	(3.46)	(2.38)	(3.65)	(3.50)	(3.51)
Elec+3	-1.46	-0.94	-2.21	-0.94	-3.08	2.06	-0.70	-1.13	-1.21
	(3.59)	(3.63)	(3.86)	(4.19)	(4.04)	(1.96)	(3.98)	(3.84)	(3.84)
N	184884	176352	170796	156264	153024	166956	150864	174300	177600
R^2	0.97	0.97	0.97	0.97	0.97	0.96	0.97	0.97	0.97
Omitted division	1	2	3	4	5	6	7	8	9
Counties	1714	1635	1583	1449	1419	1548	1399	1616	1645
States	43	46	45	42	41	45	45	41	44

Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. All specifications include county-quarter, year-quarter fixed effects and controls interacted with time trends (column 2, Table 1.1). 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 1.18: 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)
					Local government				
Elec-3	7.69*	8.04*	8.50*	10.34**	5.12	7.53	5.89	8.47**	8.68**
	(4.12)	(4.25)	(4.64)	(4.10)	(4.79)	(4.76)	(4.08)	(4.15)	(4.23)
Elec-2	8.27**	8.20**	8.93**	9.30**	8.96*	6.46**	6.47**	9.20**	8.32**
	(3.49)	(3.65)	(3.95)	(3.74)	(4.96)	(3.18)	(2.91)	(3.60)	(3.51)
Elec-1	12.82***	13.08***	12.70**	13.39**	9.67**	13.63**	11.16**	14.54***	14.39***
	(4.38)	(4.51)	(4.95)	(4.96)	(3.93)	(5.48)	(4.45)	(4.43)	(4.28)
Elec	13.31***	13.19***	14.82***	13.63***	12.18***	12.08***	11.54***	13.48***	13.82***
	(2.75)	(2.87)	(2.77)	(2.68)	(3.29)	(2.93)	(3.08)	(2.85)	(2.72)
Elec+1	3.39	2.04	4.00	4.79	5.39	0.25	3.63	4.47	3.63
	(3.57)	(3.65)	(4.01)	(3.89)	(3.79)	(3.80)	(2.84)	(3.52)	(3.69)
Elec+2	-2.21	-3.70	-0.81	-2.14	2.59	-5.94	-2.97	-1.20	-2.43
	(4.28)	(4.33)	(4.41)	(4.36)	(4.70)	(4.85)	(3.70)	(4.35)	(4.40)
Elec+3	-4.89	-5.81	-4.70	-5.36	-2.06	-7.73	-3.69	-4.02	-3.80
	(3.74)	(3.80)	(4.20)	(3.78)	(3.14)	(4.76)	(3.03)	(3.86)	(3.81)
N	184884	176352	170796	156264	153024	166956	150864	174300	177600
R^2	0.88	0.88	0.88	0.85	0.88	0.88	0.91	0.88	0.88
Omitted division	1	2	3	4	5	6	7	8	9
Counties	1714	1635	1583	1449	1419	1548	1399	1616	1645
States	43	46	45	42	41	45	45	41	44

Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. All specifications include county-quarter, year-quarter fixed effects and controls interacted with time trends (column 2, Table 1.1). 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 1.19: Results of sequentially excluding one of six time periods for the fixed effects approach.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	State government						Local government					
Elec-3	2.19	2.72	-0.60	-0.05	1.44	2.37	5.82	9.56^{*}	3.56	5.86	6.30	10.25^{**}
	(3.32)	(2.80)	(3.15)	(2.66)	(3.42)	(3.56)	(4.50)	(4.93)	(4.20)	(4.90)	(4.22)	(4.44)
Elec-2	5.74	7.83^{**}	4.27	4.54	8.75^{**}	7.76^{*}	6.09	9.01^{*}	1.63	6.87^{*}	9.34^{*}	11.14^{***}
	(4.13)	(2.99)	(3.97)	(3.08)	(3.44)	(4.02)	(4.47)	(4.80)	(3.13)	(3.55)	(5.00)	(4.09)
Elec-1	5.82	7.42^{*}	6.46^{*}	3.49	8.62^{**}	8.11^{**}	16.57^{***}	17.76^{**}	9.13^{*}	12.60^{**}	12.24^{**}	9.56^{**}
	(4.49)	(3.73)	(3.46)	(3.57)	(3.71)	(3.65)	(5.13)	(6.68)	(4.57)	(5.15)	(4.96)	(4.37)
Elec	7.12^{**}	8.01^{***}	7.14^{**}	4.66^{**}	8.35^{***}	7.42^{**}	15.43^{***}	18.03^{***}	8.29^{**}	15.19^{***}	11.71^{***}	11.45^{***}
	(2.71)	(2.23)	(2.97)	(2.26)	(2.39)	(2.84)	(3.22)	(4.46)	(3.12)	(4.62)	(3.47)	(4.18)
Elec+1	4.06	4.49^{**}	6.52^{***}	2.53	7.14^{**}	4.71^{**}	7.99^{***}	-1.05	3.36	-0.94	8.35^{**}	3.49
	(2.45)	(2.22)	(2.33)	(2.51)	(2.78)	(2.08)	(2.59)	(5.25)	(3.00)	(5.38)	(3.45)	(5.28)
Elec+2	1.11	-0.78	4.59	-1.08	6.13^{*}	0.69	1.17	-6.94	-2.14	-7.63	5.45	-1.43
	(3.37)	(3.47)	(3.61)	(3.61)	(3.52)	(2.92)	(3.99)	(5.62)	(4.82)	(5.07)	(4.65)	(5.72)
Elec+3	-1.84	-3.13	0.22	-1.63	1.50	-0.92	-2.48	-7.52^{**}	-1.64	-13.05^{**}	3.42	-10.72^{***}
	(3.44)	(4.15)	(4.27)	(3.87)	(3.61)	(3.57)	(4.62)	(3.50)	(4.44)	(5.88)	(5.26)	(3.43)
N	157362	157362	157362	157530	157392	157392	157362	157362	157362	157530	157392	157392
R^2	0.97	0.97	0.96	0.96	0.97	0.97	0.90	0.89	0.88	0.87	0.88	0.89
Omitted time period	1	2	3	4	5	6	1	2	3	4	5	6

Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. All specifications include county-quarter, year-quarter fixed effects and controls interacted with time trends (column 2, Table 1.1). 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.

Table 1.20: 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).

	(1)	(2)	(3)	(4)	(5)	(6)
	State	Local	State	Local	Local	
Elec-3	4.98*	7.50	5.86*	6.28	7.03*	7.04
	(2.49)	(4.52)	(3.08)	(6.41)	(3.87)	(5.50)
Elec-2	9.81***	7.94**	9.66***	10.03*	8.09**	11.95**
	(3.19)	(3.77)	(2.71)	(5.91)	(3.46)	(4.87)
Elec-1	9.66***	14.02***	8.20***	14.61**	13.01***	15.78***
	(2.91)	(4.97)	(2.72)	(5.44)	(3.98)	(3.58)
Elec	9.76***	11.31***	7.08**	7.25*	14.31***	10.15***
	(2.12)	(2.89)	(2.94)	(4.07)	(2.60)	(3.34)
Elec+1	8.22***	2.42	4.80*	0.36	6.84*	10.29
	(1.81)	(3.54)	(2.57)	(4.49)	(3.55)	(7.31)
Elec+2	4.28	-2.83	3.40	-6.20	0.96	2.97
	(2.72)	(3.54)	(2.61)	(6.26)	(4.12)	(7.56)
Elec+3	0.94	-3.76	-1.05	0.18	-3.59	4.98
	(2.82)	(3.52)	(2.58)	(4.37)	(3.31)	(6.01)
N	131960	131960	82296	82296	224888	124200
R^2	0.98	0.92	0.98	0.89	0.89	0.92
Fixed effects	Y	Y			Y	
County-pair			Y	Y		Y
Dropped outliers	Y	Y	Y	Y		
Larger sample					Y	Y
Counties	1223	1223	450	450	2085	637
County-pairs			381	381		575
Border segments			69	69		80
States	49	49	43	43	50	45

Dependent variable is employment per capita; coefficients and standard errors multiplied by 100 thousand for a per 100 thousand capita interpretation. Preferred specifications (Table 1.1, columns 2 and 5). Standard errors (in parentheses) clustered at the state level for the fixed effects models and two way at the state and border segment level for the county-pair models. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

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Chapter 2

The Democratic-Republican presidential growth gap and the partisan balance of the state governments

Abstract: The US economy grew faster during Democratic presidencies, but this Democratic-Republican presidential GDP growth gap cannot be fully attributed to policy differences, nor did Democratic presidents happen to systematically benefit from more favorable external shocks (Blinder and Watson 2016). The question of why thus remains open. We postulate that, if this is a real effect, a Democratic performance advantage should be present in a broader range of settings beyond just the presidency. We investigate the partisan control of the state governments and show that national GDP growth was higher when more states had Democratic governors and state legislatures. Our results suggest this effect occurs *on top of* the presidential D-R growth gap, suggesting that the Democratic growth advantage may be a more generalized phenomenon. One implication of our results is that future research investigating the effects of government ideology on national economic outcomes may benefit from considering partisan politics at lower jurisdictional levels as well.

Keywords: Democratic-Republican GDP growth gap, federalism, partisan politics, government ideology, Democrats, Republicans.

2.1 Introduction

Short-term GDP growth in the United States was higher under Democratic presidents than under Republicans. Scholars arrived at this conclusion quite some time ago.[1] The difference in economic performance under Democratic and Republican presidents, the D-R

[1] Hibbs (1986, 1987), Alesina and Sachs (1988), Haynes and Stone (1990), Alesina and Rosenthal (1995), Belke (1996), Alesina et al. (1997), Blomberg and Hess (2003), Santa-Clara and Valkanov (2003), Verstyuk (2004), Krause (2005), Bartels (2008), Grier (2008), Rohlfs et al. (2015).

presidential growth gap, has enjoyed a great deal of attention recently owing to the study by Blinder and Watson (2016) – abbreviated as BW in the following. The authors show that, over the period 1949-2012, the annualized quarterly GDP growth rate was on average around 1.79 percentage points higher under Democratic compared to Republican presidents (4.33 percent under Democratic presidents versus 2.54 under Republicans). The question is why.

The partisan theories (Hibbs 1977, Chappell and Keech 1986, Alesina 1987) propose that GDP growth is higher under Democratic presidents than under Republican presidents because Democrats implement more expansionary fiscal and monetary policies, e.g., increasing government expenditure or decreasing interest rates.[2] However, fiscal policies under Democratic presidents hardly differed from those of Republican presidents, while monetary policies differed only to some extent (Hibbs 1986 and 1987, Havrilesky 1987, Alesina et al. 1997, Faust and Irons 1999, Caporale and Grier 2000 and 2005, Abrams and Iossifov 2006, Chen and Wang 2013, BW, Pastor and Veronesi 2017).[3] The results of BW suggest that national fiscal and monetary policies do not help to explain the D-R presidential growth gap.

BW use many other variables to explain the D-R presidential growth gap. In part, Democrats just had good luck – benign oil shocks, superior total factor productivity performance and a more favorable international environment explain about half of the higher GDP growth under Democratic presidents. A substantial portion of the gap, however,

[2]For example, interest rates were expected to be higher and the dollar to be stronger under a George W. Bush presidency than under John Kerry (Snowberg et al. 2007a, b).

[3]On ideology-induced policies in OECD countries see Schmidt (1996) and Potrafke (2017). Long-run growth was higher under rightwing than leftwing governments in industrialized and developing countries (Bjrnskov 2005).

remains unexplained. BW (p. 1043) conclude: "these factors together explain up to 56 percent of the D-R growth gap in the full sample . . . The rest remains, for now, a mystery of the still mostly unexplored continent. The word research taken literally, means search again. We invite other researchers to do so." We propose to investigate this puzzle from a somewhat different angle. To the extent that there is a fundamental reason behind the D-R presidential growth gap, we might expect to see a similar phenomenon in the case of other measures of Democratic control, not just for the presidency. We therefore examine the relationship between partisan politics at the state level, namely the share of the state governorships and legislatures controlled by the Democratic Party (as well as unified state governments), and national economic performance.

State politics is a particularly natural place to look next – the US state governments have quite some leeway to implement discretionary economic policies which, in turn, are likely to influence GDP growth both in the long-run and the short-run (business cycle). We focus on short-run quarterly and annual GDP growth. For example, state governments design tax rates and minimum wages and, to a large extent, decide on the composition of the state budget, and expansionary policies implemented by individual state governments, especially in highly populated and economically influential US states such as California, Texas or New York, are likely to influence national quarterly and annual GDP growth. Indeed, California is the worlds "7th largest economy", and these states often set the trends for the rest of the nation.[4] Key components of US GDP may be highly dependent on state policies. For example, energy has seen a recent boom in Texas and other currently Republican leaning states such as North Dakota, due partly to developments in hydraulic

[4]https://www.bloomberg.com/news/articles/2015-01-16/brown-s-california-overtakes-brazil-with-companies-leading-world.

fracturing, a procedure banned in Democratic leaning New York, Vermont and Maryland. While governors and state legislatures have substantial influence on their own (Brudney and Herbert 1987, Jens 2017, Cahan 2019), state government partisanship is likely to be most important when the governorship and majorities in the State House and State Senate are controlled by the same party, that is, under a unified state government. Empirically, as well, it is advantageous to look at state governments because the share of Democratic state governments exhibits much greater variation over time compared to a simple dummy variable for the party of the president. Despite the apparent importance of the state governments, however, previous studies did not examine the connection between partisan control at the state level and the national level economic outcomes.

Our results suggest that more Democrats governed in state governments during times of higher short-run national GDP growth – there is a strong positive correlation between GDP growth and the fraction of US states controlled by Democratic governors or by unified Democratic state governments. This correlation is present during both Democratic and Republican presidencies. Given this finding, we might expect that the Democratic control variables at the state level may be highly correlated with the Democratic president dummy variable, so that we are simply picking up presidential growth gap with a slightly different variable. However, this is not the case. Quite the opposite. The Democratic state government variables are largely orthogonal to the Democratic president variable, suggesting that we are observing an additional Democratic performance advantage on top of the D-R presidential gap already known. If anything, the party of the president tends to lose state governorships and legislatures over the course of the presidents tenure, and this state-level effect may even work to dampen the D-R presidential growth gap. We predict using the VAR-based method of BW that GDP growth gap might have been even larger, by as much as 0.42-0.60 percentage points, were it not for changes in the partisan control of the state governments. This finding anything but explains the D-R presidential growth gap. Rather, the Democratic performance advantage appears to extend to the partisan balance of the state governments as well, on top of the D-R presidential growth gap, making the latter phenomenon seem perhaps even more puzzling.

If more Democratic governorships or state legislatures are associated with higher economic performance at the national level, we also expect that an individual state should experience higher performance under a Democratic governor or legislature. Indeed, this was the finding of previous studies (Chang et al. 2009). Here we also provide additional evidence that states with Democratic state governments had higher annual income per capita growth than states with Republican state governments, paying particular attention to highly populated states such as California, Texas and New York because these large states contribute disproportionately to the national GDP (for example, the top 10 states in terms of population made up 54 percent of the national population in 2016).

In line with BW, we acknowledge that we do not estimate a causal effect of (state) government partisanship on national GDP growth. Pastor and Veronesi (2017) suggest that Democratic presidents did not cause higher GDP growth than Republican presidents. In fact, risk-aversion, which is high during economic crises, is proposed as the source of the Democratic advantage because Democrats are likely to provide more social insurance than Republicans. Indeed, this argument should also apply at the state level, providing a plausible interpretation of our findings. Kane (2017) argues that the D-R presidential growth gap becomes smaller when a longer time lag between the inauguration of a new president and economic outcomes is used. We elaborate on the timing issue and discuss different lag choices. In any case, while there is no econometric strategy to estimate causal effects of government partisanship on macroeconomic variables at the national level, the D-R presidential growth gap remains an important empirical regularity in need of further investigation.

2.2 The political pendulum at the state level

Over time the political pendulum swings, and the popularity of the incumbent president generally decreases. The president cannot be voted out of office until his term expires, but ample opportunities arise in lower level elections to express dissatisfaction (or apathy, by not turning out to vote). Figure 1 shows the share of state governorships that are controlled by the Democratic Party, over the period 1949-2017. We weight the share of Democratic governors by the population of the individual states because we will relate these variables to national GDP growth in sections 3 and 4, and states with larger populations contribute more to national GDP than less populated states.[5] The pattern is stark: at the beginning of Democratic presidential terms, the share of population weighted Democratic governors was 56 percent on average. By contrast, in the last year of Democratic presidential terms, the share of population weighted Democratic governors was 45 percent on average. In the first year of a Republican term, it was 46 percent, rising to 57 percent by the last year of the term. We also examine Democratic legislatures and unified state

[5]The weighted and unweighted measures are similar: the correlation coefficients between the weighted and unweighted Democratic share of governors, legislatures, and unified state governments are all about 0.92. The results are similar when we weight by state income (gross state product is not available over the full sample period).

governments (Figures 2 and 3). The share of Democratic legislatures (unified governments) was around 54 percent (39 percent) in the first and 40 percent (28 percent) in the last year of a Democratic presidential term. The share of Republican legislatures (unified governments) was 32 percent (24 percent) in the first year of Republican terms, falling to 21 percent (13 percent) in the last year.

Thus, newly elected presidents enjoyed many copartisan governors and unified state governments – but tended to lose them over time. In the case of governorships, the share of Democratic governors was almost always decreasing during Democratic presidencies, except for Clinton, during whose second term the Democratic Party picked up state governorships (most notably California in 1998). While the trends under Eisenhower, Nixon, and Ford are of large and steady losses of Republican governorships, the Republican Party was successful in state elections under Reagan, G. H. W. and G. W. Bush – the trends are quite flat and include periods of gains.

In the case of state legislature control, the share of legislatures controlled by the presidents party does not follow such a regular pattern – both parties tended to lose state legislatures over time, but there were frequent gains as well. The partisan balance was quite stable during Reagans and G. H. W. Bushs terms, although Republicans held very few states to begin with, so there was not much to lose. Nixon-Ford performed notably poorly, the share of Republican state legislatures dropping from almost 50 percent at the beginning to essentially zero at the end of Fords term, a lot of the loss coming in the 1974 midterm elections, only three months after Nixons resignation. Clinton also oversaw substantial loses during his first term, though the situation stabilized during his second term.

For unified state governments, Nixon and Ford again stand out as especially weak.

Johnson also lost many unified governments late in his term in the 1966 midterm elections, during the height of the unpopular Vietnam war and shortly following the passage of the Voting Rights Act of 1965. Obamas first term is characterized by a substantial increase in Republican unified governments in the 2010 midterm elections, though the number of Democratic unified governments remained fairly stable and even increased at times during his presidency.

2.3 State government partisanship and national GDP growth

In the first year of Democratic presidential terms, we observe both (a) large shares of Democratic governors and unified Democratic state governments and (b) especially pronounced quarterly GDP growth (see also BW) – on average quarterly real GDP growth (annualized) was 4.47 during the first year of Democratic terms compared to 0.67 percent during the first year of Republican terms (2.1). We therefore expect the share of Democratic governors and unified Democratic state governments to be an excellent predictor of national GDP growth. In this section establish the correlation between the partisan control of the state governments and national GDP growth – in the next section we will investigate to what extent it explains the D-R presidential growth gap or, in other words, to what extent it is a separate phenomenon.

We estimate a linear regression model with Newey-West standard errors using the quarterly growth in real national GDP (annualized) as the dependent variable and a Democratic president dummy variable as the explanatory variable. In 2.2, column

(1), we simply estimate the D-R presidential growth gap for the period 1949:II-2017:I. The coefficient estimate of the Democratic president dummy variable is 1.50, indicating that the D-R growth gap was 1.50 percentage points, moderately smaller than BWs D-R growth gap of 1.79 for the period 1949:II-2013:I. We follow BW and assign the quarter during which the new president is inaugurated and power changes hands (the first quarter, January-March, of the post-election year) to the outgoing president. In column (2), we only include the share of Democratic governors, without the presidential dummy. Following the same convention as for presidents, a new governors influence is assumed to start in their first full quarter (April-June of the post gubernatorial election year). In columns (3) and (4) we include the share of Democratic and Republican controlled legislatures and unified Democratic and Republican state governments as explanatory variables, again without the presidential dummy. In columns (5) to (7) we include the Democratic president variable together with either the share of Democratic governors, the share of Democratic and Republican controlled legislatures, or the unified Democratic and Republican state government variable. The coefficient estimate for the share of Democratic governors is 4.39 in column (2). Since the standard deviation of the Democratic share of governors is 0.13, we conclude that a one standard deviation increase in the share of Democratic governorships is associated with a 0.57 percentage point increase in the real national GDP growth rate. The coefficient estimate for the share of Democratic state legislatures is 7.25, so that a one standard deviation (about 0.16) increase (at the expense of split legislatures) in the share of Democratic state legislatures is associated with a 1.16 percentage point increase in the real national GDP growth rate. Similarly, the coefficient for Democratic unified state governments, 5.91 (in column 4), suggests that a one standard deviation (about

0.13) increase (at the expense of non-unified state governments) in the Democratic unified state government share is associated with a 0.77 percentage point increase in the real GDP growth rate. The variables measuring Republican state government control lack statistical significance.[6]

An obvious objection is that the share of Democratic governors or unified governments and the Democratic president dummy variable may be highly correlated and essentially measure the same thing. However, the correlation coefficients are only -0.056 and 0.060, and the coefficient estimate for the Democratic president dummy variable remains statistically significant and similar in magnitude in columns (5) to (7). This suggests that the state government partisanship variables are highly correlated with GDP growth, but are largely orthogonal to the Democratic president dummy variable. We investigate this further in the next section. Including interaction terms, the positive correlation between the share of Democratic governors or unified governments and national GDP growth is present under both Democratic and Republican presidencies. Column (8) shows that the correlation between the share of Democratic governors and national GDP growth is positive under both Democratic and Republican presidents, though falls short of statistical significance at conventional levels during Republican presidencies. In column (9), the coefficient estimate for the share of Democratic unified state governments is also positive during both Democratic and Republican presidencies and is statistically significant at the 5 percent and 10 percent levels.

An important issue is the timing of when changes in partisan control is likely to predict annual GDP growth. Following BW, we have so far assumed that a new president

[6]We do not include the share of Republican governors. As there were only a handful of independent governors, it is almost exactly one minus the share of Democratic governors.

or governor starts to take effect during their first full quarter. Kane (2017) maintains that it takes longer than one quarter for a new government to affect GDP growth, because it takes quite some time for new legislation and policies to be implemented. Consumer behavior and firm investment decisions have, however, been shown to immediately respond to electoral outcomes due to shifts in expectations (Snowberg 2007a and 2007b, Gerber and Huber 2009, Julio and Yook 2012, Jens 2017, Falk and Shelton 2018). In 2.3, we use different lags and leads of state government partisanship as explanatory variables. A lag (or lead) of *k* means that we regress GDP growth in the current quarter on the government *k* quarters from the current quarter – up to now we used the BW assumption of one lag. The correlation between Democratic state government control and GDP growth is strong for lags 0 to 3. For governors and legislatures, the correlation is less pronounced and no longer statistically significant when we consider lags of more than three quarters, while for unified governments it remains positive and statistically significant at the 10 percent level up to the 10th lag. The first and second leads of Democratic state government control (governors and unified governments) are also positively and significantly correlated with GDP growth, suggesting that high national GDP growth may have preceded Democratic victories in state elections (Pastor and Veronesi 2017). We return to alternative lag assumptions in the next section.

2.4 Explaining the D-R growth gap (BW model)

2.4.1 Methods

We investigate whether state government control can "explain" the D-R presidential growth gap in the framework of BW, which is informative to whether what we are observing is a separate phenomenon that occurs on top of the D-R presidential growth gap, or whether we may be just picking up the same effect. BW (p. 1028f.) consider many explanatory variables potentially accounting for the observed D-R presidential growth gap. Intuitively, the idea is that the D-R presidential growth gap may be attributed to these potentially omitted variables rather than to the Democratic president dummy. The explanatory variables include, for example, oil shocks from Hamilton (2003), defense spending shocks from Ramey (2011), monetary policy shocks from Romer and Romer (2004) and Sims (2006). We investigate similar models incorporating our variables capturing the partisan balance of the state governments. A (z,x)-VAR model is estimated, where x consists of explanatory variables and z includes the GDP growth rate, inflation (measured by the GDP deflator), the three-month Treasury bill rate, and commodity prices. The lag length used is six quarters. The residuals e_t from the VAR model are then used as regressors in a distributed lag model, in which the growth rate of real GDP is regressed on e_t and six of its lags: that is, the model is $y_t = \gamma(L)e_t +$ other factors. As BW (p. 1028) explain, the average realization of $\gamma(L)e_t$ during Democratic presidencies may be different than during Republican presidencies. First, the shocks e_t are time varying, and their realization will differ over different time periods. Second, the coefficients (lag weights) γ may be different during Democratic and Republican presidencies, because different parties may respond to or be affected by the same shock differently. Following BW, we run specifications where the γ are constrained to be the same for both parties (common lag weights), and specifications where they are not constrained (party-specific lag weights). There are good reasons to believe that the lag weights should be able to differ by party – a decrease in the share of Democratic state governments may certainly elicit a different response from a Democratic presidential administration compared to a Republican administration.

BW show (a) univariate results that are based on regressions only including one *e* variable and (b) multivariate results that are based on regressions including more than one *e* variable. We do the same including our state politics variables. Again, the purpose is to show how much of the D-R presidential growth gap is "explained" by the *e* variables. That is, based on the *e* shocks and the estimated lag weights $\hat{\gamma}$, one estimates the predicted D-R presidential growth gap due to the variable of interest: the predicted GDP growth rate under Democratic presidents minus the predicted GDP growth rate under Republican presidents (p. 1029f.). This is the explained portion of the D-R presidential growth gap. For a given variable, if the explained portion of the D-R presidential growth gap is positive, it means that the variable predicts higher GDP growth under Democratic compared to Republican presidents, thus contributing to the D-R presidential growth gap and helping to explain it. For example, the Hamilton oil price shock explains about 50 basis points of the full sample 179 basis point D-R presidential growth gap. If the explained portion is negative, it means that the variable predicts higher GDP growth under Republican compared to Democratic presidents, thus working to close the D-R growth gap, meaning that, were it not for that variable, we would predict the presidential growth gap to be even larger.

2.4.2 Results

2.4 shows the univariate results for various combinations of the state government partisanship variables. The share of Democratic governors does not explain the D-R presidential growth gap. Neither do the share of Democratic state legislatures or unified Democratic state governments. More than that, these variables even "push in the wrong direction" (BW p. 1037): the explained D-R presidential growth gap is negative and large in magnitude in columns (1) to (5), though only attains statistical significance in some specifications. Our point estimates (party-specific lag weights) for the explained portion of the D-R presidential growth gap are large: -0.31 for Democratic governors alone, increasing to -0.42, -0.54 and -0.60 for combinations capturing more aspects of state government control (Democratic governors and legislatures together, and Democratic governors and unified governments together, and Democratic governors, legislatures and unified governments together). An explained portion of -0.42 means that, absent the effect of the share of Democratic governors, we predict the D-R presidential growth gap would have been 2.18 percentage points rather than the actual D-R gap of 1.76 (a difference of 0.42 percentage points).[7]

Multivariate results including oil price shocks, defense expenditure shocks, and other variables proposed by BW, together with the share of Democratic governors indicate that again the share of Democratic governors explained a negative fraction of the D-R

[7]The baseline D-R growth gap for our sample is 176 basis points, not 179 as quoted in BW, because we consider the period 1950:I-2015:I rather than 1949:II-2013:I. Some early observations are lost due to the lags included in the VAR model of BW. This also happens for BW when they consider the Baa-Aaa spread and the Baker et al. (2013) uncertainty index, for which VAR models are also used. A few shocks like the Hamilton shock are available slightly earlier (1949:II) because they are not constructed by BW using the VAR. We also extended the dataset through to 2015:I for most of our models, though inferences are very similar if we end the sample at the same time as BW.

growth gap, though the effects lack statistical significance at conventional levels using BW's lag structure (2.5).

The effects are sensitive to the assumption about when a newly elected politician can begin to affect the economy. BW assume this occurs during the first full quarter in office (April-June after the presidential elections in November), attributing the quarter of the election (October-December) and the quarter during which the inauguration takes place (January-March) to the predecessor. The explained proportions of the D-R presidential growth gap for the state politics variables are negative, large in magnitude, and statistically significant when we instead assume that a new politician starts to have an effect in the quarter of the election, or in the quarter of the inauguration (2.6). For lags longer than considered in BW, the explained portions of the D-R presidential growth gap become somewhat smaller in magnitude and attain lower levels of statistical significance, though remain large and negative in all specifications and statistically significant in some of them.

There is no accepted convention for choosing when newly elected politicians begin to affect the economy. BW acknowledge (p. 1017) that their assumption, chosen "on a priori grounds," is the one that maximizes the size of the D-R gap while recognizing that political scientists usually prefer lags of a year or more (Bartels 2008; Comiskey and Marsh 2012). Kane (2017) takes issue with this assumption and shows that the D-R growth gap becomes much smaller when considering longer lags. We do not take a stand on which lag choice is the most suitable. It is certainly true that policies are implemented with a lag. On the other hand, when uncertainty about the election outcome is resolved, agents immediately begin to update their expectations about future economic conditions, even before any policies have been implemented. Consequently, their decisions may change immediately (or even before, if the result is anticipated) following the election (Snowberg 2007a and 2007b, Gerber and Huber 2009, Julio and Yook 2012, Jens 2017, Falk and Shelton 2018).[8] Of course, this all assumes a causal interpretation for the observed correlation. It is equally plausible that GDP growth rates leading up to elections influence voting behavior (Pastor and Veronesi 2017). In any event, the share of Democratic governors does not explain the D-R presidential growth gap, confirming that the Democratic performance advantage with respect to partisan balance of the state governments is a separate effect from the D-R presidential growth gap. At one extreme, it appears to be an independent effect on top of the D-R presidential gap; at the other, it even works in the opposite direction, suggesting that the D-R gap might have been even bigger otherwise.

2.5 Economic performance and government partisanship in the US states

If Democratic state governments are associated with higher economic growth at the national level, then we should also expect individual states to grow faster under Democratic state politicians. While the link between state politicians and national GDP growth has been largely unexplored, there is a large literature on the effects of government partisan control on economic performance and policies in the US states (for a survey, see Potrafke 2018). In previous studies, many outcome variables such as income per capita, tax rates, public expenditure, etc., were regressed on variables measuring the party affiliation of the governor and majorities in the State House and State Senate. Chang et al. (2009) find, for

[8]See also BWs online appendix 5 for durable good consumption and investment.

example, that state real personal income growth over the period 1951-2004 was higher under Democratic than Republican governors, especially in the first part of a legislative period.

2.5.1 Annual income per capita growth: some new empirical evidence

We examine whether state annual personal income per capita growth in the US states was higher under Democratic than Republican state governments (there is no data for GDP at the state level available until the 1960s). Unlike Chang et al. (2009), we pay particular attention to highly populated states such as California, Texas and New York because these large states contribute more to the national GDP. Because states with large populations and crystal-clear political majorities are central to our study, we cannot use regression discontinuity designs (RDD) and rather use descriptive statistics and estimate linear panel data models that report correlations between state government partisanship and state annual personal income per capita growth.

We use annual data for state real personal income per capita growth across the 50 US states over the period 1949-2016 (inferences are very similar for the shorter sample of 1949-2012 considered in BW). With annual rather than quarterly data, we assume new politicians take effect the year of their inauguration (almost always in January of that year). Descriptive statistics (2.7) show that state annual real personal income per capita growth was on average higher under Democratic governors than under Republican governors (2.02 percent versus 1.79 percent, t-statistic 2.27), and higher under Democratic unified governments than under Republican unified governments (2.16 percent versus 1.79 percent, t-statistic 2.47). The difference between Democratic and Republican legislatures does not turn out to be statistically significant.

We split the sample based on population and consider the top 10 states by population (in 2016) which account for about 54 percent of the population (California, Texas, New York, Florida, Illinois, Pennsylvania, Ohio, Georgia, North Carolina, Michigan) and the remaining 40 states. The results show that, in the top 10 states, state income per capita growth was on average higher under Democratic than Republican governors (2.06 percent versus 1.65 percent). The differences were also pronounced under Democratic and Republican legislatures (2.07 percent versus 1.69) and, especially, under unified state governments (2.24 percent versus 1.63 percent). These differences are larger than for the bottom 40 states, suggesting that the more populated states may be making a disproportionate contribution.

We estimate linear panel data models regressing state income per capita growth on state government partisanship variables including state and year fixed effects. The results in 2.8 show that growth in state income per capita was around 0.16 percentage points higher under Democratic than Republican governors, or 0.24 percentage points if we weight by population. In both cases the estimates are statistically significant at the 5 percent level. For legislatures, income per capita growth was around 0.30 percentage points (0.44 when weighting) higher when Democrats had control relative to when Republicans had control (statistically significant at the 1 percent level). When there was a Democratic unified state government, state income per capita growth was about 0.15 percentage points (0.21 when weighting) higher than when the governorship and the legislature were not held by the same party. When there was a Republican unified state government, state income per capita growth was about 0.24 percentage points (0.30 when weighting) lower than when the governorship and the legislature were not held by the same party (statistically significant at the 5 and 1 percent levels).

When we estimate the same panel regressions looking only at the top 10 and bottom 40 states in terms of population (columns 3 to 6 of 2.8), the results are similar for both size categories. For the top 10 states, the differences are quite pronounced, especially for legislatures, often attaining statistical significance despite the small number of states.[9] This suggests that those states that matter the most for national GDP growth indeed experience pronounced differences in state-level performance under Democratic and Republican state governments. It does not, however, establish causality or identify which policies, if any, may have been responsible.

2.5.2 Southern Democrats and changes in party ideology

That the national economy grows faster when Democrats control more state governments is, of course, not necessarily a causal relationship – the government partisanship variable is endogenous and, indeed, the mechanism proposed by Pastor and Veronesi (2017) may well also apply at the state level. In addition, any causal effect need not be thanks to the "modern" Democratic Party. As noted by BW (page 1017), the D-R presidential growth gap gets smaller over time. At the same time, the platforms and constituencies of both parties have seen fundamental changes since the immediate post-WWII period (McCarty et al. 2006, Gentzkow et al. 2016). The most notable change was the large-scale realignment of the "Solid South" away from the Democratic Party towards the Republican Party through the 1960s to the 1990s. We find that, while the South certainly contributes to our findings, it was not alone in experiencing differences in state income per capita growth under the

[9]Because of the small number of states when looking at subsets of the 50 states, we report heteroscedasticity robust standard errors as well.

two parties, for both presidents and governors (2.9).

2.6 Conclusions

We examine the extent to which party politics at the US state level explain national GDP growth. The results are stark: higher national GDP growth was generated when more US states had Democratic governors and unified Democratic state governments. Over the period 1949:II-2017:I, a one standard deviation increase in the share of governorships controlled by the Democratic Party (unified Democratic state governments) was associated with a 0.57 percentage point (0.77 percentage point) increase in the real national GDP growth rate. Moreover, this Democratic performance advantage with respect to the state governments occurs in addition to the D-R presidential growth gap, suggesting that there is a more generalized Democratic performance advantage that extends beyond just the presidency.

Our results have three important implications for future research. The first is whether Democratic or Republican state governments may have been associated with pronounced *long-run* growth in real personal income per capita and GDP. Short run economic performance is different from long run growth. Also, it does not necessarily reflect "good" governance – growth-oriented policies must be traded off against other considerations (e. g., stimulus packages versus budget consolidation). Different constituencies have different priorities, and elected officials are tasked with representing these interests (see, for example, Kitschelt 2000).

Second, studies examining the effects of government ideology on national economic performance in federal states may benefit by considering party politics at the lower jurisdictional level. This includes industrialized countries such as Canada and Germany but also somewhat less developed countries such as India. For example, there may have been strategic interaction and interjurisdictional competition across state governments due to fiscal externalities.

Third, the D-R presidential growth gap remains puzzling. Our results suggest that the Democratic advantage investigated by BW extends to the state level, as well. Rather than explaining the D-R presidential growth gap, however, the state government phenomenon appears to exist *in addition* to it. Future research should continue to explore the channels through which the relationship between Democratic politicians and GDP growth at various levels arises and the extent to which, if at all, Democratic policies may have caused higher GDP growth.

2.7 Acknowledgements

Chapter 2 is co-authored with Niklas Potrafke and is currently being prepared for submission for publication of the material. The dissertation author was co-author and co-investigator of this material.

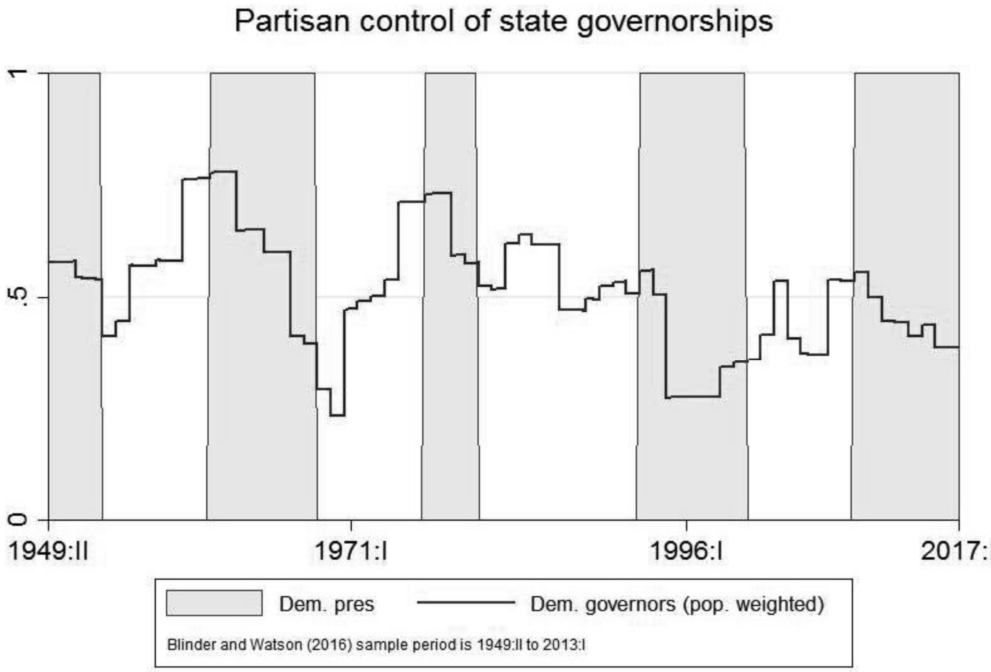

Figure 2.1: The share of state governorships controlled by the Democratic Party was high (low) at the beginning of Democratic (Republican) presidential terms and tended to decrease (increase) drastically during the course of the terms. Source: Data on state level election results are taken from a variety of publicly available online sources, including David Leips Atlas of US Presidential Elections, Carl Klarners datasets (2013a), state agency websites.

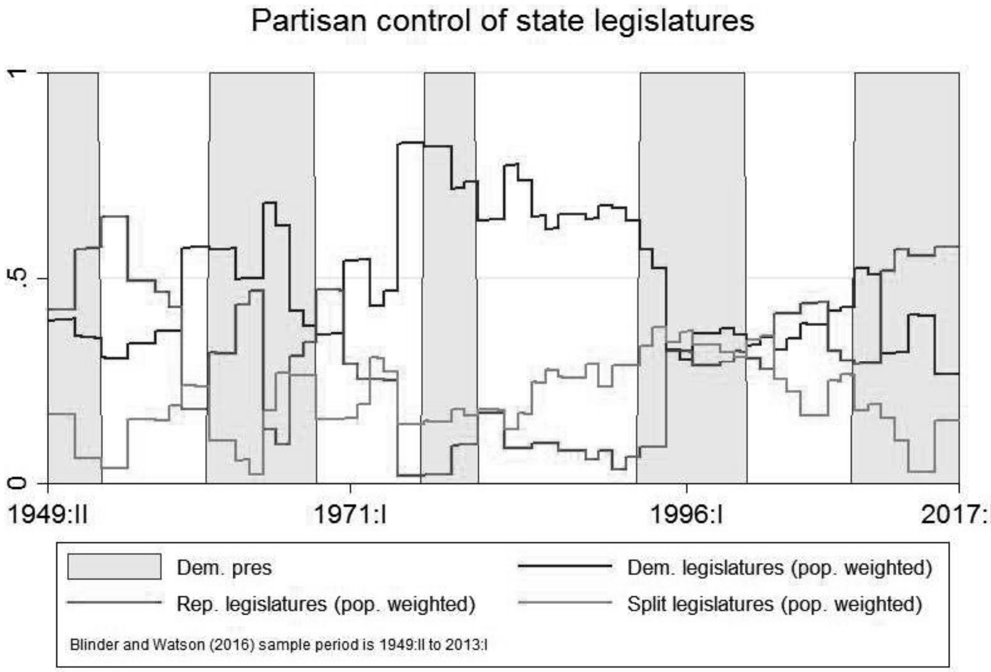

Figure 2.2: The share of state legislatures that were controlled by the incumbent presidents party also decreased in the course of the presidential term (though not as consistently and not as drastically as in the case of control of governorships). Source: Klarner (2013b), own calculations.

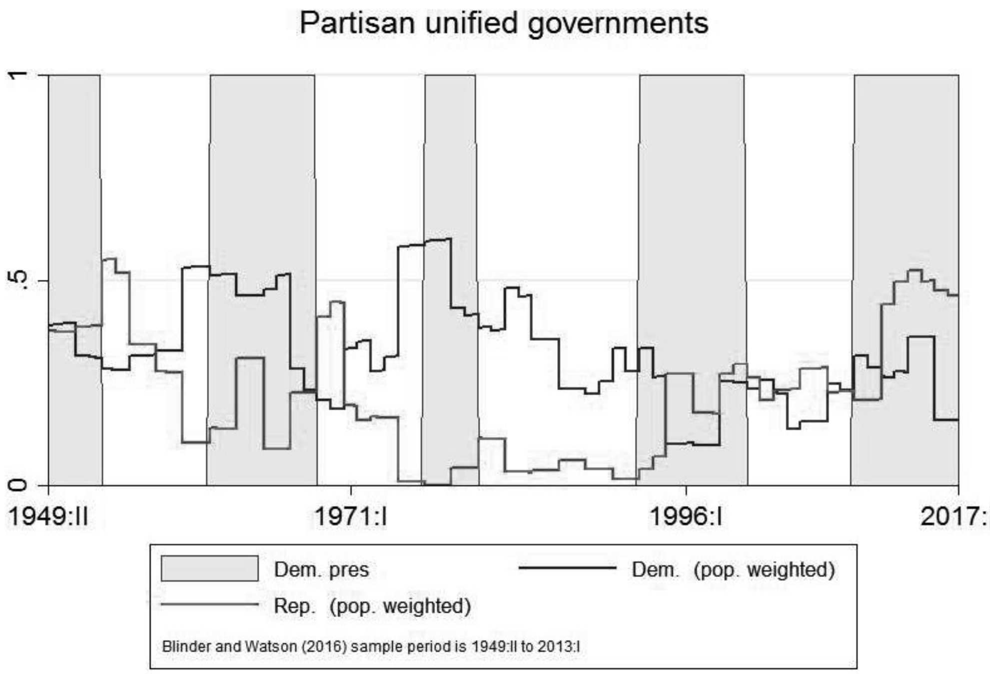

Figure 2.3: Newly elected presidents enjoyed many copartisan governors and unified state governments but tended to lose them over time. Source: Klarner (2013a, b), own calculations.

Table 2.1: Descriptive statistics on real national quarterly GDP growth (annualized) under Democratic and Republican presidents (1949:II-2017:I).

	No. of quarters	Avg. annualized GDP growth	Dem. gov.	Dem. leg.	Rep. leg.	Dem. unified govts.	Rep. unified govts.
Dem. pres.	128	4.05	0.51	0.47	0.33	0.33	0.25
First year	32	4.47	0.56	0.54	0.26	0.39	0.19
Second year	32	4.67	0.55	0.53	0.26	0.38	0.19
Third year	32	3.49	0.46	0.41	0.39	0.28	0.30
Last year	32	3.57	0.45	0.40	0.41	0.28	0.31
Rep. pres	144	2.54	0.52	0.52	0.27	0.32	0.18
First year	36	0.67	0.46	0.46	0.32	0.27	0.24
Second year	36	2.28	0.47	0.47	0.32	0.28	0.23
Third year	36	4.37	0.58	0.57	0.21	0.37	0.14
Last year	36	2.86	0.57	0.57	0.21	0.35	0.13
Overall	272	3.25	0.51	0.50	0.30	0.32	0.22

Notes: Government ideology measured with one lag (BW).

Table 2.2: State government ideology predicting real national (annualized) quarterly GDP growth (1949:II-2017:I).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dem. pres.	1.50**				1.57**	1.59**	1.50**	0.91	1.70
	(0.63)				(0.61)	(0.63)	(0.62)	(1.87)	(1.35)
Dem. governors		4.39**			4.72***				
		(1.92)			(1.65)				
Dem. leg.			7.25**			7.01**			
			(3.64)			(3.22)			
Rep. leg.			4.49			3.53			
			(3.00)			(2.56)			
Dem. unified govts.				5.91**			4.86**		
				(2.49)			(2.12)		
Rep. unified govts.				0.33			-1.06		
				(2.09)			(2.12)		
Dem. pres.								5.27**	
×Dem. gov.								(2.20)	
Rep. pres.								3.98	
×Dem. governors								(2.81)	
Dem. pres.									5.05**
×Dem. unified govts.									(2.26)
Rep. pres.									5.91*
×Dem. unified govts.									(3.32)
Cons.	2.54***	1.00	-1.67	1.27	0.09	-2.02	1.20	0.48	0.67
	(0.45)	(0.98)	(2.62)	(1.15)	(0.92)	(2.24)	(0.99)	(1.44)	(1.11)
R^2	0.04	0.02	0.02	0.04	0.06	0.06	0.07	0.06	0.07

Notes: Dependent variable is quarterly GDP growth (annualized). Newey-West (6 lag) standard errors in parentheses. $N = 272$. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$..

Table 2.3: State government ideology predicting real national quarterly GDP growth (annualized) for alternative lags of state government ideology (1949:II-2017:I).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No. of state govt. ideology lags	-2	-1	0	1 (BW)	2	3	4
Governors							
Dem. pres.	0.84	0.86	1.33**	1.57**	1.15*	0.73	0.42
	(0.62)	(0.61)	(0.58)	(0.61)	(0.61)	(0.61)	(0.66)
Dem. governors	3.57*	4.04**	4.88***	4.72***	3.92**	3.69*	2.75
	(1.98)	(1.83)	(1.68)	(1.65)	(1.77)	(1.92)	(1.94)
Cons.	1.05	0.79	0.13	0.09	0.70	1.02	1.64
	(0.99)	(0.93)	(0.85)	(0.92)	(1.00)	(1.06)	(1.11)
Legislatures							
Dem. pres.	0.86	0.89	1.37**	1.59**	1.14*	0.68	0.33
	(0.62)	(0.62)	(0.60)	(0.63)	(0.63)	(0.61)	(0.65)
Dem. leg.	1.36	2.60	5.03*	7.01**	6.03*	5.49	4.83
	(3.06)	(2.80)	(2.75)	(3.22)	(3.37)	(3.57)	(3.52)
Rep. leg.	-0.32	0.45	1.79	3.53	3.41	3.43	3.91
	(2.71)	(2.31)	(2.13)	(2.56)	(2.69)	(2.86)	(2.91)
Cons.	2.28	1.43	-0.41	-2.02	-1.29	-0.81	-0.46
	(2.14)	(1.93)	(1.89)	(2.24)	(2.37)	(2.56)	(2.57)
Unified governments							
Dem. pres.	0.80	0.79	1.29**	1.50**	1.00	0.53	0.14
	(0.62)	(0.61)	(0.59)	(0.62)	(0.62)	(0.61)	(0.64)
Dem. unified gov.	3.25	4.14*	4.36**	4.86**	5.16**	5.25**	5.16**
	(2.24)	(2.14)	(2.04)	(2.12)	(2.27)	(2.43)	(2.55)
Rep. unified gov.	-0.93	-0.70	-1.59	-1.06	0.09	0.63	1.92
	(2.05)	(2.07)	(1.99)	(2.12)	(2.07)	(1.97)	(2.14)
Cons.	2.05**	1.71*	1.58*	1.20	1.09	1.16	1.09
	(0.90)	(0.94)	(0.94)	(0.99)	(1.01)	(1.06)	(1.13)
N	270	271	272	272	272	272	272

Notes: Dependent variable is average quarterly GDP growth (annualized). Newey-West (6 lag) standard errors in parentheses. 0 lags correspond to assigning the quarter during which a politician is inaugurated to the incoming politician. 1 lag is the BW baseline, where politicians are assigned their first full quarter in office. All political variables in a single regression assume the same lag. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 2.4: Explaining the D-R-growth gap with state government ideology. BW model.

	(1)	(2)	(3)	(4)	(5)	(6)
Begin	1950:I	1950:I	1950:I	1950:I	1950:I	1950:I
End	2015:I	2015:I	2015:I	2015:I	2015:I	2015:I
Total D-R gap	1.76	1.76	1.76	1.76	1.76	1.76
	(0.66)	(0.66)	(0.66)	(0.66)	(0.66)	(0.66)
Dem. governors	-0.23			-0.16	-0.15	-0.40
	(0.17)			(0.22)	(0.35)	(0.34)
Dem. leg.		-0.22		-0.16		-0.37
		(0.17)		(0.22)		(0.24)
Rep. leg.		-0.00		0.04		
		(0.27)		(0.26)		
Dem. unified gov.			0.02		0.07	0.25
			(0.07)		(0.13)	(0.20)
Rep. unified gov.			-0.36		-0.32	
			(0.19)		(0.20)	
Explained D-R gap	-0.23	-0.22	-0.34	-0.28	-0.40	-0.52
(common lag weights)	(0.17)	(0.17)	(0.16)	(0.21)	(0.23)	(0.28)
Explained D-R gap	-0.31	-0.23	-0.39	-0.42	-0.54	-0.60
(party-specific lag weights)	(0.17)	(0.19)	(0.16)	(0.26)	(0.26)	(0.29)
p-value	0.51	0.28	0.01	0.05	0.00	0.00

Notes: Total D-R gap refers to the difference in average growth between Democratic and Republican presidents for the corresponding time period. The explained D-R gap is computed as described in the text using the combination of shocks indicated. With common lag weights, distributed lag weights are assumed the same for Democratic and Republican presidents; with party-specific lag weights, they can be different. Newey-West (6 lag) standard errors in parentheses. The *p*-value corresponds to F-tests for equality between the party-specific distributed lag coefficients.

Table 2.5: Explaining the D-R-growth gap with the share of Democratic governors. Multivariate results. BW model.

	(1)	(2)	(3)	(4)	(5)
Begin	1950:I	1950:III	1950:III	1950:III	1950:III
End	2013:I	2013:I	2013:I	2013:I	2007:IV
Total D-R gap	1.90	1.70	1.70	1.70	1.91
	(0.68)	(0.62)	(0.62)	(0.62)	(0.62)
Oil (Hamilton)	0.47	0.37	0.37	0.37	0.17
	(0.10)	(0.11)	(0.11)	(0.11)	(0.09)
Defense (Ramey)	0.18	0.14	0.13	0.12	0.17
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
TFP (BW)		0.38	0.38	0.38	0.38
		(0.07)	(0.07)	(0.07)	(0.10)
Baa-Aaa spread			-0.03		
			(0.10)		
Uncertainty (BBD)				-0.02	
				(0.05)	
Taxes (RR)					-0.01
					(0.01)
Dem. governors	-0.19	-0.16	-0.16	-0.16	-0.15
	(0.16)	(0.14)	(0.14)	(0.15)	(0.12)
Explained D-R gap	0.46	0.72	0.69	0.69	0.56
(common lag weights)	(0.19)	(0.17)	(0.22)	(0.19)	(0.16)
Explained D-R gap	0.48	0.79	0.61	0.75	0.45
(party-specific lag weights)	(0.45)	(0.48)	(0.55)	(0.50)	(0.50)

Notes: Total D-R gap refers to the difference in average growth between Democratic and Republican presidents for the corresponding time period. The explained D-R gap is computed as described in the text using the combination of shocks indicated. With common lag weights, distributed lag weights are assumed the same for Democratic and Republican presidents; with party-specific lag weights, they can be different. Newey-West (6 lag) standard errors in parentheses. The *p*-value corresponds to F-tests for equality between the party-specific distributed lag coefficients. Similar to BW, Table 8.

Table 2.6: Explaining the D-R-growth gap with state government ideology and alternative lag assumptions. BW model.

No. lags	Shocks included	Sample period	Explained D-R gap; distributed lag model Total D-R gap	Common	Party-specific	*p*-value
-2	DG	1950:I-2015:I	0.98 (0.68)	-0.33 (0.17)	-0.40 (0.18)	0.56
	DG DL RL	1950:I-2015:I	0.98 (0.68)	-0.36 (0.20)	-0.51 (0.23)	0.02
	DG DUG RUG	1950:I-2015:I	0.98 (0.68)	-0.42 (0.21)	-0.52 (0.24)	0.00
-1	DG	1950:I-2015:I	0.99 (0.67)	-0.33 (0.19)	-0.43 (0.19)	0.43
	DG DL RL	1950:I-2015:I	0.99 (0.67)	-0.41 (0.23)	-0.58 (0.25)	0.02
	DG DUG RUG	1950:I-2015:I	0.99 (0.67)	-0.45 (0.22)	-0.57 (0.23)	0.02
0	DG	1950:I-2015:I	1.48 (0.65)	-0.38 (0.18)	-0.45 (0.18)	0.25
	DG DL RL	1950:I-2015:I	1.48 (0.65)	-0.40 (0.20)	-0.50 (0.23)	0.00
	DG DUG RUG	1950:I-2015:I	1.48 (0.65)	-0.47 (0.23)	-0.58 (0.25)	0.00
1 (BW)	DG	1950:I-2015:I	1.76 (0.66)	-0.23 (0.17)	-0.31 (0.19)	0.51
	DG DL RL	1950:I-2015:I	1.76 (0.66)	-0.28 (0.21)	-0.42 (0.26)	0.05
	DG DUG RUG	1950:I-2015:I	1.76 (0.66)	-0.40 (0.23)	-0.54 (0.26)	0.00
2	DG	1950:I-2015:I	1.55 (0.66)	-0.10 (0.16)	-0.19 (0.18)	0.46
	DG DL RL	1950:I-2015:I	1.55 (0.66)	-0.16 (0.20)	-0.28 (0.21)	0.30
	DG DUG RUG	1950:I-2015:I	1.55 (0.66)	-0.18 (0.22)	-0.33 (0.23)	0.00
3	DG	1950:I-2015:I	1.34 (0.65)	-0.11 (0.15)	-0.19 (0.17)	0.71
	DG DL RL	1950:I-2015:I	1.34 (0.65)	-0.19 (0.19)	-0.27 (0.19)	0.37
	DG DUG RUG	1950:I-2015:I	1.34 (0.65)	-0.15 (0.21)	-0.30 (0.21)	0.00

Notes: Number of lags refers to when the effect of incoming politicians is assumed to start, relative to the quarter in which they are inaugurated. DG, DL, DUG (RG, RL, RUG) refer to Democratic (Republican) governors, legislatures and unified governments. Newey-West standard errors (6 lags) in parentheses. The *p*-values corresponds to F-tests of equality of coefficients across party-specific and common lag weight specifications. See also Table 2.5.

Table 2.7: Annual growth in states income per capita under Democratic and Republican state governments.

	Income per capita growth (1949-2016) in percent		
	All states	Top 10	Bottom 40
Overall	1.91 (3396)	1.87 (680)	1.92 (2716)
Dem. governor	2.02 (1793)	2.06 (359)	2.01 (1434)
Rep. governor	1.79 (1577)	1.65 (321)	1.82 (1256)
Independent/other	1.18 (26)	- (0)	1.18 (28)
D-R difference	0.24[2.27]	0.42 [1.89]	0.19 [1.61]
Dem. legislature	2.02 (1631)	2.07 (319)	2.01 (1312)
Rep. legislature	1.81 (1104)	1.71 (213)	1.83 (891)
Split. legislature	1.78 (619)	1.69 (151)	1.81 (468)
D-R difference	0.21 [1.62]	0.37 [1.21]	0.17 [1.19]
Dem. unified government	2.16 (1073)	2.24 (229)	2.14 (844)
Rep. unified government	1.79 (735)	1.63 (158)	1.83 (577)
Non unified government	1.79 (1520)	1.71 (293)	1.81 (1227)
D-R difference	0.37 [2.47]	0.61 [2.01]	0.30 [1.79]

Average values by state government partisanship. Number of state-years in parentheses. Nebraska is not included with respect to legislatures, since it has a nonpartisan unicameral legislature. For the D-R differences, the t-statistic (in square brackets) is calculated by regressing the outcome on state government dummy variables, clustering at the state level. The top 10 states by population (in 2016) are CA, TX, FL, NY, IL, PA, OH, GA, NC, MI, and account for about 54 percent of the population.

Table 2.8: Annual growth in states income per capita under Democratic and Republican state governments.

	Income per capita growth (1949-2016)					
	All states		Top 10		Bottom 40	
	(1)	(2)	(3)	(4)	(5)	(6)
Governors						
Dem.	0.16**	0.24**	0.18	0.29	0.15*	0.17*
	(0.07)	(0.10)	(0.17)	(0.17)	(0.08)	(0.09)
			[0.12]	[0.12]	[0.11]	[0.08]
Indep.	-0.16	-0.02	-	-	-0.19	-0.18
	(0.33)	(0.21)	-	-	(0.34)	(0.29)
			-	-	[0.37]	[0.32]
R^2	0.43	0.62	0.74	0.74	0.39	0.54
N	3396	3396	680	680	2716	2716
Legislatures						
Dem.	0.30***	0.44***	0.53**	0.54**	0.20	0.30**
	(0.11)	(0.13)	(0.19)	(0.20)	(0.12)	(0.10)
			[0.19]	[0.19]	[0.17]	[0.12]
Split	0.18	0.36***	0.48***	0.56***	0.09	0.13
	(0.11)	(0.11)	(0.15)	(0.14)	(0.13)	(0.12)
			[0.18]	[0.18]	[0.19]	[0.13]
R^2	0.43	0.63	0.74	0.74	0.40	0.55
N	3328	3328	680	680	2648	2648
Unified governments						
Dem.	0.15*	0.21*	0.07	0.20	0.16	0.21**
	(0.08)	(0.11)	(0.16)	(0.17)	(0.09)	(0.09)
			[0.15]	[0.15]	[0.11]	[0.09]
Rep.	-0.24**	-0.30***	-0.38*	-0.35*	-0.19	-0.25**
	(0.11)	(0.10)	(0.18)	(0.13)	(0.13)	(0.12)
			[0.18]	[0.18]	[0.18]	[0.12]
R^2	0.43	0.63	0.74	0.74	0.40	0.55
N	3328	3328	680	680	2648	2648
Pop. weighted		Y		Y		Y

Notes: State and year fixed effects included. Standard errors clustered at the state level in parentheses, heteroscedasticity robust standard errors in square brackets (asterisks based on the more conservative standard errors). Nebraska is excluded from the legislature and unified government regressions, since it has a nonpartisan unicameral legislature. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

Table 2.9: Panel regression results. Annual growth in real state income per capita under Democratic and Republican governors and presidents, by region.

	Income per capita growth (1949-2016)					
	(1)	(2)	(3)	(4)	(5)	(6)
South	2.01***	1.71***	1.55***	1.78***	1.48***	1.33***
	(0.07)	(0.11)	(0.16)	(0.09)	(0.09)	(0.14)
West	1.46***	1.72***	1.53***	1.18***	1.44***	1.15***
	(0.08)	(0.12)	(0.14)	(0.06)	(0.08)	(0.10)
Northeast	1.76***	2.16***	1.93***	1.73***	2.19***	2.02***
	(0.08)	(0.10)	(0.08)	(0.02)	(0.08)	(0.08)
Midwest	1.74***	1.63***	1.55***	1.38***	1.53***	1.24***
	(0.18)	(0.09)	(0.20)	(0.10)	(0.08)	(0.15)
Dem. pres.	0.28**		0.31**	0.21		0.28*
×South	(0.12)		(0.14)	(0.13)		(0.15)
Dem. pres.	0.40**		0.41**	0.73***		0.72***
×West	(0.18)		(0.17)	(0.18)		(0.16)
Dem. pres.	0.49***		0.50***	0.38***		0.32***
×Northeast	(0.11)		(0.14)	(0.06)		(0.08)
Dem. pres.	0.16		0.18	0.57***		0.59***
×Midwest	(0.28)		(0.29)	(0.18)		(0.21)
Dem. gov.		0.64***	0.66***		0.70***	0.72***
×South		(0.19)	(0.20)		(0.14)	(0.15)
Dem. gov.		-0.12	-0.11		0.19	0.06
×West		(0.18)	(0.17)		(0.20)	(0.12)
Dem. gov.		-0.32*	-0.34*		-0.58***	-0.55***
×Northeast		(0.18)	(0.18)		(0.16)	(0.17)
Dem. gov.		0.45***	0.46***		0.29	0.32
×Midwest		(0.14)	(0.14)		(0.19)	(0.22)
R^2	0.26	0.26	0.26	0.31	0.31	0.31
Pop. weighted				Y	Y	Y

Notes: Variables for independent governors also included, omitted from table; constant excluded from regression to avoid collinearity. Standard errors clustered at state level. $N = 3396$. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$.

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Chapter 3

Electoral competition under best-worst voting rules

Abstract: We characterise multi-candidate pure-strategy equilibria in the Hotelling-Downs spatial election model for the class of best-worst voting rules, in which each voter is endowed with both a positive and a negative vote, i.e., each voter votes in favour of their most preferred candidate and against their least preferred. The importance of positive and negative votes in calculating a candidate's net score may be different, so that a negative vote and a positive vote need not cancel out exactly. These rules combine the first-place seeking incentives of plurality with the incentives to avoid being ranked last of antiplurality. We show that, in our simple model, arbitrary best-worst rules admit equilibria, which (except for three candidates) are nonconvergent if and only if the importance of a positive vote exceeds that of a negative vote. The set of equilibria in the latter case is very similar to that of plurality, except the platforms are less extreme due to the moderating effect of negative votes. Moreover: (i) any degree of dispersion between plurality, at one extreme, and full convergence, at the other, can be attained for the correct choice of the weights; and, (ii) when they exist (and there are at least five candidates), there always exist nonconvergent equilibria in which none of the most extreme candidates receive the most electoral support.

3.1 Introduction

Hotelling's (1929) "Main Street" model of spatial competition between firms has—most notably thanks to its adaptation by Downs (1957) to ideological competition among political parties—enjoyed a significant presence in the voting literature. In the classical model, there is a society of voters whose ideal policy platforms lie along the left-right political spectrum. A set of exogenously given political candidates or parties choose platforms to advocate so as to maximise their support from the voters, who vote for the candidate with the platform nearest to his or her personal ideal platform.

Most such studies of Downsian competition have focused on situations in which elections are held under the voting system known as plurality rule. This is the simplest system where voters have one vote each, which they cast for their favourite candidate, and whoever gets the most votes wins. Under plurality, voters' second, third and other preferences—most importantly for this paper, their last place preferences—do not matter. However, voting systems, both used in practice and studied theoretically, come in many varieties. Many of them do take into account voters' partial or full ranking of candidates when producing a winner. These include, among others, approval voting, Borda count, and single transferable vote. When the preferences beyond first matter, candidates' incentives change, and we expect equilibrium outcomes to vary as well. In this paper, we analyze the equilibrium properties of a largely overlooked class of voting rules, which combine positive and negative voting, and are referred to as *best-worst* rules (García-Lapresta et al., 2010). Under these rules, each voter casts one positive and one negative vote and a candidate's total score is the weighted difference of the number of positive votes and the number of negative votes. We allow the weight of a negative vote to be different from that of a positive vote and, hence, this class of voting systems includes as special cases plurality, anti-plurality, and the system in which positive and negative votes are of equal importance.[1]

The main result of this paper is that, in a simple Hotelling-Downs model with uniformly distributed,[2] sincere voters and no exit or entry, there is a close link between the pure-strategy equilibria of general best-worst rules and those of plurality, which is well

[1]Also known as "single-positive-and-single-negative" voting (Myerson, 1999).
[2]We discuss a generalisation of the uniformity assumption in Section 3.5.

known to admit divergent equilibria in which candidates adopt a range of ideologically diverse positions (Eaton and Lipsey, 1975; Denzau et al., 1985). When the importance of a positive vote exceeds that of a negative vote, equilibria take the same general form as those of plurality, with divergent policy platforms advocated. However, the key difference is that, while differentiated, the equilibrium platforms for the best-worst rules exhibit less dispersion. Indeed, these rules present candidates with a clear centrifugal motive to seek first-place rankings, as occurs under plurality, but with the simultaneous incentive to avoid being the most unpopular candidate and receiving negative votes. This last property encourages a degree of policy moderation—adopting extreme platforms is discouraged as doing so is likely to single oneself out as a target for the negative votes of citizens at the opposite end of the ideological spectrum. As the importance of a negative vote increases relative to that of a positive vote, the equilibrium platforms move inwards towards the median voter's ideal platform. Eventually all platforms merge at the median as a negative vote reaches parity with a positive vote (i.e., one negative vote cancels out one positive vote exactly). When a negative vote becomes more important than a positive vote, only convergent equilibria exist, with no policy differentiation.

Describing the equilibrium properties of different voting systems is an important task (Cox, 1985, 1987; Grofman and Lijphart, 1986; Myerson and Weber, 1993; Myerson, 1999; Cahan and Slinko, 2017). When choosing between voting rules, first of all, we would like to know whether or not equilibria exist—their absence may lead to permanent instability and a lack of predictability of outcomes. Second, if they exist, an electoral designer would prefer a rule that admits equilibria with desirable properties. The main consideration here is a tradeoff between discouraging extremism and promoting fair representation—it is undesirable if candidates are incentivised to adopt extremist platforms rather than more centrist platforms, while at the same time the rationale for voting in the first place is to provide citizens with political representation of their varied interests. Besides the platforms that are advocated, which platforms are likely to receive the most support also matters for similar reasons.

Our results show that best-worst rules do well on all counts. They admit nonconvergent equilibria, offering voters a choice over distinct platforms and avoiding Hotelling's "excessive sameness". At the same time, the perhaps excessive extremism associated with plurality (Cox, 1987, 1985; Myerson and Weber, 1993; Laslier and Maniquet, 2010) is moderated. Indeed, depending on the weight placed on a negative vote, we may have any level of dispersion of platforms between that of plurality, at one extreme, and full convergence of platforms, at the other. Moreover, the candidates that adopt the most extreme positions in equilibrium never obtain a strictly higher vote share than any other candidate—in fact, when there are at least five candidates, there always exist NCNE in which the most extreme candidates receive a strictly smaller vote share than at least one less extreme candidate. Finally, best-worst voting rules have the additional advantage that they are simple and easily implementable, requiring only that voters list their first and last choices and not a tedious full ranking.

Best-worst voting itself has not been used in practice, but the idea of voting against candidates in one form or another has been around for some time. Boehm (1976) in an unpublished essay suggested that voters in an election be allowed either to cast a vote for or against a candidate, but not both. A candidate's "negative" votes would be subtracted from his "positive" votes to determine his net vote, and the candidate with the highest net vote would win.[3] Boehm—and many others after him (see, e.g., Leef, 2014)—argued that the introduction of negative votes in United States presidential elections would force the candidates to appeal to voters with positive programs, rather than just fill the airwaves with ads attacking other candidates, sowing doubt among their supporters. The rule suggested by Boehm is now known as negative voting (Brams, 1983). Anti-plurality voting is a similar method in which each voter votes against a single candidate, and the candidate with the fewest votes against wins. In other words, anti-plurality determines who among the candidates is the least unpopular.[4]

The use of some form of negative voting in elections is not so uncommon. For example, Nevada gives voters the option to vote against all candidates by having a "None of these candidates" option on the ballot. Prior to 2000, Lithuanian voters were allowed voters to express approval, neutrality or disapproval of candidates in the proportional representation part of their parliamentary elections (Renwick and Pilet, 2016). Latvia does the same in allocating a party's European Parliament seats to individual candidates from the party list.[5]

A voting system in which voters cast both positive and negative votes, as occurs under best-worst rules, may be even more advantageous. It can give a fighting chance to major or minor centrist parties—it is not unthinkable that people on the extreme left will vote for a leftist candidate and against a right-wing one, while the right-wing voters will do the opposite. Their votes will cancel out and a centrist candidate will be elected.[6] Indeed,

[3]Presumably, if the highest net vote is negative, then nobody is elected.

[4]Anti-plurality is also sometimes referred to as negative voting as well as "veto" (Kang, 2010).

[5]See the following training course for election observers: https://www.cvk.lv/pub/public/30083.html.

[6]See Kang (2010) for an account of more costs, benefits and tradeoffs associated with negative voting.

roughly speaking, this is directly in the spirit of our main results.[7]

The rest of this paper is organised as follows: in Section 3.2 we outline some literature related to this work; in Section 3.3 we present the model; in Section 3.4 we present our main results; Section 3.5 discusses a few of the assumptions and the generalisability of the results; and, Section 3.6 provides our concluding remarks. A few minor and auxiliary results are presented in the Appendix.

3.2 Related literature

Best-worst rules specifically and notions related to them have been considered before in other contexts. The idea that the best and worst alternatives play a special role in the decision process has been prominent in decision theory. For example, the Arrow-Hurwicz (1972) criterion for choice under uncertainty takes a weighted average of the best and worst expected value/utility outcomes and does not take into account intermediate outcomes, and Marley and Louviere (2005) look at probabilistic discrete choice models through the best-worst lense.

García-Lapresta et al. (2010) provide an axiomatic characterisation of the class of best-worst voting rules considered in this paper. Alcantud and Laruelle (2014) characterise a related voting rule in which, for each candidate, voters may express approval, indifference, or disapproval. This rule is also studied in Felsenthal (1989) from the perspective of

[7] We do not model it here, but there may be further arguments in favour—e.g., the number of ways in which voters can express themselves is further diversified, which could increase turnout by appealing to voters who are enticed more by the ability to vote against a candidate than for one (Kang, 2010; Leef, 2014). In an experimental setting, Baujard et al. (2014) find that French voters are generally positive about the use of alternative and more expressive "evaluative" voting methods similar to best-worst rules. We leave these considerations for future work.

voter strategies. Joy and McMunigal (2016) believe that the current system of peremptory challenges in the criminal justice system of the United States makes it easy to exclude qualified African Americans jurors in the process of jury selection and propose that it be replaced with a system of peremptory strikes and peremptory inclusions. In other words, both the defense and the prosecution should be allowed to not just rule potential jurors out, but also “rule them in”.

Baujard et al. (2014), during the first round of the 2012 French presidential election, ran an experiment in which subjects were asked to vote for candidates using various “evaluative voting” methods, which bear many similarities to the best-worst voting rules considered in our paper. Voters “graded” candidates on a numerical scale: for example, under one system they could assign each candidate 1 point, 0 points, or -1 points; under another, they could assign 2 points, 1 point or 0 points. They documented an interesting psychological effect: these systems were not treated the same, despite being mathematically equivalent (see also Igersheim et al., 2016).

None of these papers, thus, look at how the incentives created by these voting systems affect political competition. Given the very natural combination of negative and positive voting embodied in the best-worst rules, it is surprising that, to the best of our knowledge, they have been overlooked in the spatial competition literature. Plurality, a special case, has of course been extensively discussed, and its equilibrium properties are characterised in Eaton and Lipsey (1975) and Denzau et al. (1985). Anti-plurality is known to allow convergent equilibria in which all candidates adopt the same policy platform, but not to allow nonconvergent equilibria (Cox, 1987).

The two most relevant papers to this research are Cox (1987) and Cahan and Slinko

(2017). Both are concerned with Nash equilibria under the class of voting rules known as general scoring rules, of which the best-worst rules are a subclass. Cox (1987) characterised all scoring rules that have convergent Nash equilibria, which leads to a straightforward description of all best-worst rules allowing convergent equilibria, as we will describe in Section 3.4. However, Cox's theorem says nothing about the possibility of divergent equilibria, which is the focus of Cahan and Slinko (2017), and also this paper.

Cahan and Slinko (2017) investigate the existence and properties of nonconvergent equilibria under general scoring rules. In some subclasses of scoring rules—in particular, those whose score vector is convex—they managed to characterise all rules that allow Nash equilibria. These rules appear to be truncated variants of the Borda rule. This result is, however, inapplicable to the best-worst rules, whose score vectors are neither convex nor concave. A general characterisation of scoring rules that allow equilibria remains an open question.

3.3 The model

There is a unit mass of voters with ideal positions distributed uniformly on the interval [0,1], the issue space.[8] There are m candidates—candidate i's position is x_i, and a strategy profile $x = (x_1, \ldots, x_m) \in [0,1]^m$ describes the platforms of all the candidates. A strategy profile implies a set of distinct occupied positions, $x^1 < x^2 < \ldots < x^q$. We denote by n_i the number of candidates at occupied position x^i and we will sometimes use the alternative notation for a strategy profile, $x = ((x^1, n_1), \ldots, (x^q, n_q))$, which gives the

[8] See Section 3.5 for a discussion on limitations, justifications and generalisations of the uniform distribution assumption.

location and number of candidates at each occupied position rather than each individual candidate's position.

We will use notation $[n] = \{1,\dots,n\}$ and if $I = [a,b]$ is an interval, then $\ell(I) = b-a$ is the length of the interval. We assume sincere voters with single-peaked, symmetric utility functions who, hence, rank candidates according to the distance between their advocated platform and the voter's ideal position. Voters who are indifferent between candidates decide on a strict ranking by fair lottery.

A best-worst voting rule can be described as follows: a first-place ranking earns a candidate a normalised 1 point, while a last-place ranking earns the candidate $-c$ points, where $c \geq 0$. Being ranked anywhere other than first or last by a voter earns a candidate nothing. The magnitude of c describes the relative importance of the positive vote relative to the negative vote, which is the parameter of interest here. Thus, a rule can be described by a pair of numbers $s = (c,m)$, where m is the number of candidates.[9]

Candidate i's score is the weighted difference between the number of positive votes and the number of negative votes received, denoted $v_i(x)$. Candidates choose positions simultaneously so as to maximise $v_i(x)$.[10] Our equilibrium concept is the Nash equilibrium in pure strategies. Profile $x^* = (x_1^*,\dots,x_m^*)$ is an equilibrium if and only if $v_i(x^*) \geq v_i(x_i, x_{-i}^*)$ for all $i \in [m]$ and for all $x_i \in [0,1]$, where $(x_i, x_{-i}^*) = (x_1^*,\dots,x_{i-1}^*, x_i, x_{i+1}^*,\dots,x_m^*)$. A convergent Nash equilibrium (CNE) is an equilibrium in which all candidates adopt the same platform, while in a non-convergent Nash equilibrium (NCNE), at least two of the

[9]As noted in the Introduction, best-worst rules belong to the class of general scoring rules. A scoring rule is a vector $s = (s_1,\dots,s_m)$, where $s_1 \geq \cdots \geq s_m$, $s_1 > s_m$, and s_i is the number of points assigned to the i-th ranked candidate in a voter's ballot. A best-worst rule $s = (c,m)$, then, is equivalent to scoring rule $s = (1,0,\dots,0,-c)$.

[10]See Section 3.5 for a discussion of this assumption and plausible alternatives.

platforms are distinct. The notation x_i^+ and x_i^- refer to points $x_i + \varepsilon$ and $x_i - \varepsilon$, respectively, for vanishingly small $\varepsilon > 0$.

3.4 Results

Our main result is a general characterisation of NCNE for rules $s = (c, m)$ in Theorem 3.4.3. Before we concentrate on NCNE, however, we should address the issue of CNE—equilibria in which all candidates adopt the same platform. In fact, their characterisation is straightforward, presented below in Proposition 3.4.1. This result follows directly from Cox (1987), who characterised CNE for general scoring rules, a broad class of voting rules to which best-worst rules belong.

Proposition 3.4.1 (Cox, 1987)**.** *A rule $s = (c, m)$ admits CNE if and only if $c \geq 1$, in which case the profile $x = ((x^1, m))$ is a CNE for any $x^1 \in \left[\frac{m-1+c}{m(1+c)}, 1 - \frac{m-1+c}{m(1+c)}\right]$.*

Proof. For $x = ((x^1, m))$ to be a CNE, it should not be beneficial to deviate just to the left or right of x^1. That is, we have CNE if and only if: first, $v_i(x^{1-}, x_{-i}) = x^1 - c(1 - x^1) \leq \frac{1-c}{m} = v_i(x)$; and, second, $v_i(x^{1+}, x_{-i}) = 1 - x^1 - cx^1 \leq v_i(x)$. Together, these two conditions yield the interval of possible CNE, which is nonempty if and only if $c \geq 1$. □

Proposition 3.4.1 tells us that CNE can only exist if the weight on a positive vote does not exceed that of a negative vote. In this case, a small deviation from the common platform differentiates a candidate in a positive way for one side of the electorate, and negatively for the other. The gain in terms of positive votes is not worth the damage due to the negative votes that the candidate will now receive, so candidates will stay put at the common platform. So CNE exist at any point of an interval centered at the median voter's ideal position. As c increases, this interval expands, meaning that a wider range of CNE are possible.[11]

While Proposition 3.4.1 tells us everything there is to know about CNE, it is silent about NCNE. We do know that NCNE exist for plurality (Eaton and Lipsey, 1985), but not for antiplurality (Cox, 1987), both of which are examples of best-worst rules, so the picture is not at all clear in general.

It turns out that, for NCNE to exist, it must be that $c < 1$. In other words, the value of a positive vote must outweigh the value of a negative vote in order for the candidates to be induced to adopt divergent policies. Otherwise, the centripetal incentive to avoid being singled out as the worst candidate is too strong and only CNE can exist. This also implies that CNE and NCNE cannot exist simultaneously for the same rule.[12]

Proposition 3.4.2. *The rule $s = (c, m)$ does not admit NCNE if $c \geq 1$.*

Proof. Consider candidate 1 at position x^1, which is occupied by n_1 candidates, where $2 \leq n_1 \leq m-2$. Consider intervals $I_1 = [0, x^1]$ and $I_2 = [(x^1 + x^q)/2, 1]$. If 1 makes an infinitesimal move to the right of x^1, then in the rankings of voters in I_1, of which there is positive measure by Lemma 3.8.1, she falls behind the other $n_1 - 1$ candidates originally at x^1, thus losing their positive votes. On the other hand, 1 rises ahead of these $n_1 - 1$ candidates in the rankings of all other voters and, in particular, no longer receives a negative vote from any voter. Then, the score candidate 1 loses by making this move is $v_{lost} = \frac{1}{n_1}\ell(I_1)$. On the other hand, 1's gain from this move is $v_{gained} = \frac{1}{n_1}c\ell(I_2)$.

For NCNE, it must be the case that $v_{lost} \geq v_{gained}$, or $\ell(I_1) \geq c\ell(I_2)$. Since we

[11] Provided $m > 2$; if $m = 2$, any rule reduces to plurality.

[12] They can coexist for other scoring rules (Cahan and Slinko, 2017) outside the class of best-worst rules.

assume $c \geq 1$, this implies that $\ell(I_1) \geq \ell(I_2)$, or $x^1 \geq 1-(x^1+x^q)/2$. Similar considerations with respect to candidate q yields the requirement that $\ell([x^q,1]) \geq \ell([0,(x^1+x^q)/2]$, or $1-x^q \geq (x^1+x^q)/2$. Together, these two conditions imply that $x^1 \geq x^q$, an impossibility for an NCNE. □

Before we proceed to our characterisation, some additional notation. Let

(i) $I_1 = [0,(x^1+x^2)/2]$,

(ii) $I_i = [(x^{i-1}+x^i)/2,(x^i+x^{i+1})/2]$ for $2 \leq i \leq q-1$,

(iii) $I_q = [(x^{q-1}+x^q)/2,1]$,

be the "full-electorates" around each of the occupied positions. A full-electorate I_i is the set of voters for whom a given occupied position x^i is the nearest, so that any candidates located there are ranked first equal for these voters. For each $i \in [q]$ let $I_i^L = \{y \in I_i : y \leq x^i\}$ and $I_i^R = \{y \in I_i : y \geq x^i\}$ be the left and right "half-electorates" whose union is the full-electorate I_i. That is, we simply partition a full-electorate into those voters whose ideal positions lie to the left of the given occupied position and those who lie to the right. Note that $\ell(I_i^R) = \ell(I_{i+1}^L)$ for $i \in [q-1]$.

We now present our characterisation of NCNE for best-worst rules, Theorem 3.4.3, which provides five necessary and sufficient conditions for a profile to be an NCNE for a given best-worst rule. Condition (i) states that the outermost occupied positions must be occupied by two candidates apiece. It is clear that they cannot be single candidates, but this condition also excludes the possibility of more than two candidates, as in the well-known case of plurality (see Eaton and Lipsey, 1975). The second condition says that all paired candidates' half-electorates are the same length, excluding end electorates, while (iii) relates these interior half-electorates to the outermost half-electorates. Conditions (iv) and (v) put restrictions on the lengths of various electorates: first, an unpaired candidate's full-electorate cannot be smaller than any half-electorate (excluding end half-electorates); and, second, a paired candidate's half-electorate cannot be smaller than an unpaired candidate's half-electorate (excluding end half-electorates).

An important observation to make is that, with the exception of (iii), all the remaining conditions are identical for any rule—they do not depend directly on c, as long as $c < 1$. This implies that the equilibrium spacing will be affected by c, but not the configuration of the candidates, i.e., the number of occupied positions and how many candidates occupy them. Thus, if they exist (we will see shortly that they do for any rule with $c < 1$), NCNE for best-worse rules will have the same general form as NCNE for plurality, the only difference being the exact location of the platforms $x^1, \ldots, x^q$.

Theorem 3.4.3. *Given a rule $s = (c, m)$, with $c < 1$, the following conditions are necessary and sufficient for a profile x to be an NCNE:*

(i) $n_i \leq 2$ *for all* $i \in [q]$ *and* $n_1 = n_q = 2$*. That is, candidates at the most extreme occupied positions are paired.*

(ii) If $n_i = 2$ *for* $1 < i < q$*, then* $\ell(I_i^L) = \ell(I_i^R) = \ell(I_1^R) = \ell(I_q^L)$*. Let* I^p *denote this common measure. That is, all paired candidates' half-electorates are the same length (except end half-electorates).*

(iii) $\ell(I_1^L) = \ell(I_q^R) = I^p + \frac{c}{2}$*.*

(iv) If $n_i = 1$*, then both* $\ell(I_i) \geq \ell(I_k^L)$ *for all* $k \neq 1$ *and* $\ell(I_i) \geq \ell(I_k^R)$ *for all* $k \neq q$*. That is, any (unpaired) candidate's full-electorate is no smaller than any other half-electorate*

(excluding end half-electorates).

(v) $I^p \geq \ell(I_k^L)$ *for all* $k \neq 1$ *and* $I^p \geq \ell(I_k^R)$ *for all* $k \neq q$. *That is, a paired candidate's half-electorate (excluding end half-electorates) is no smaller than any other (unpaired) candidate's half-electorate (excluding the end half-electorates).*

Proof. That (i) is necessary follows from Lemma 3.8.4, so we start by showing the necessity of (ii). Suppose candidate j is at x^i, where $n_i = 2$ and suppose without loss of generality that $\ell(I_i^L) > \ell(I_i^R)$. Then $v_j(x^{i-}, x_{-j}) = \ell(I_i^L) > \ell(I_i^R) = v_j(x^{i+}, x_{-j})$, contradicting Lemma 3.8.2. So $\ell(I_i^L) = \ell(I_i^R)$. Let I^p denote this common measure. Moreover, note that $v_1(x^{1+}, x_{-1}) = \ell(I_1^R)$. Using Lemmas 3.8.2 and 3.8.3, we know $v_1(x^{1+}, x_{-i}) = v_1(x) = v_j(x) = I^p$, so that $\ell(I_1^R) = I^p$. Similarly, $\ell(I_q^L) = I^p$. Hence, condition (ii) is necessary.

Now condition (iii). Note that we must have $\ell(I_1^L) = \ell(I_q^R)$. Otherwise, if $\ell(I_1^L) > \ell(I_q^R)$, then, using Lemmas 3.8.2 and 3.8.3,

$$v_q(x) = v_1(x) = v_1(x^{1-}, x_{-1}) > v_q(x^{q+}, x_{-q}) = v_q(x),$$

a contradiction. Thus, $(x^1 + x^q)/2 = 1/2$. Hence, $v_1(x) = \frac{1}{2}(\ell(I_1) + I^p) - \frac{c}{4}$, which, by Lemma 3.8.2, is equal to $v_1(x^{1+}, x_{-1}) = I^p$, so $\ell(I_1^L) = I^p + \frac{c}{2}$.

Now conditions (iv) and (v). Let candidate l be at x^i. Then, if $n_i = 1$, we have $v_l(x) = \ell(I_i)$. Suppose there is some $k > 1$ such that $\ell(I_i) < \ell(I_k^L)$. Clearly the half electorate I_k^L could not be i's half electorate, i.e. $k = i$ or $k = i+1$. So we have

$$v_l(x^{k-}, x_{-l}) = \ell(I_k^L) > \ell(I_i) = v_l(x),$$

so this is not an NCNE. So we must have $\ell(I_k^L) \leq \ell(I_i)$. For (v), if $n_i = 2$, then (noting that i can be 1 or q since all paired candidates receive the same score by Lemma 3.8.3) $v_l(x) = I^p$, which, to avoid contradiction, implies $I^p \geq \ell(I_k^L)$ for all $k \neq 1$. Similarly for right electorates.

Now sufficiency. We need to check that no candidate can deviate profitably. Consider candidate i at x^j, where $n_j = 2$ (i could be an end candidate). We know that all paired candidates get the same score, $v_i(x) = I^p$, and that $v_i(x^{1-}, x_{-1}) = v_i(x)$, so i would not want to deviate to x^{1-} or x^{q+}. Also, if $t \in (x^k, x^{k+1})$ for some $k < q$, then $v_i(t, x_{-1}) = \ell(I_k^R) \leq I^p = v_i(x)$ by condition (v). Candidate i would also not deviate to an occupied position x^k, $k \neq j$. Doing so would yield a score of $v_i(x^k, x_{-i}) = \frac{2}{3}I^p < v_i(x)$ if $n_k = 2$ or a score of $v_i(x^k, x_{-i}) = \frac{1}{2}\ell(I_k) = \frac{1}{2}(\ell(I_k^L) + \ell(I_k^R)) \leq I^p = v_i(x)$ if $n_k = 1$, by (v). So no paired candidates would deviate.

Consider an unpaired candidate i at position x^j. Then $v_i(x) = \ell(I_j)$. Clearly any moves within the interval (x^{j-1}, x^{j+1}) do not change i's score. Suppose $t \in (x^k, x^{k+1})$ for some $k \notin \{j-1, j, q\}$. Then $v_i(t, x_{-i}) = \ell(I_k^R) \leq \ell(I_j) = v_i(x)$, so i will not move to any unoccupied position. Suppose $n_k = 2$ and $k \notin \{j-1, j+1\}$. Then $v_i(x^k, x_{-i}) = \frac{2}{3}I^p < I^p \leq \ell(I_j) = v_i(x)$, by (iv). Suppose $n_k = 1$ and $k \notin \{j-1, j+1\}$. Then $v_i(x^k, x_{-i}) = \frac{1}{2}(\ell(I_k^L) + \ell(I_k^R)) \leq \ell(I_j) = v_i(x)$. So no unpaired candidate wants to deviate to any occupied position that is not adjacent to the candidate's current position.

Finally, we check that no unpaired candidate would move to an adjacent occupied position. If $n_{j-1} = 2$, $j-1 \neq 1$, then $v_i(x^{j-1}, x_{-i}) = \frac{1}{3}(I^p + \ell(I_j)) \leq \frac{2}{3}\ell(I_j) < \ell(I_j) = v_i(x)$. If $j-1 = 1$ then $v_i(x^{j-1}, x_{-i}) = \frac{1}{3}(\ell(I_1^L) + \ell(I_j)) - \frac{c}{6} = \frac{1}{3}\left(I^p + \frac{c}{2} + \ell(I_j)\right) - \frac{c}{6} \leq \frac{2}{3}\ell(I_j) < \ell(I_j) = v_i(x)$. If $n_{j-1} = 1$, then $v_i(x^{j-1}, x_{-i}) = \frac{1}{2}(\ell(I_{j-1}^L) + \ell(I_j)) \leq \ell(I_j) = v_i(x)$. So no unpaired candidate wants to move to the next left occupied position or, by similar arguments, to the next right occupied position. We have checked all possible deviations, so x is a NCNE. $\square$

While Theorem 3.4.3 gives necessary and sufficient conditions for an NCNE, it is not yet clear that these conditions can be satisfied for an arbitrary number m of candidates and any $c < 1$. For $m = 2$ only convergent equilibria may exist. For $m = 3$, no equilibria can exist whatsoever by the familiar argument (Eaton and Lipsey, 1975) that one of them would have to be alone at an outermost occupied position, and would have an incentive to move inwards. The next result addresses this question and shows that they do indeed exist for any $m \geq 4$.

Corollary 3.4.4. *For all $m \geq 4$ NCNE exist for rules $s = (c, m)$ with $c < 1$, and for $m \geq 6$ there are infinitely many NCNE for a given rule. Moreover, all NCNE take the same general form as plurality (they have NCNE with the same number of occupied positions, q, with the same number of candidates, n_i, at each one, but perhaps different locations).*

Proof. Consider $m \geq 4$. Suppose candidates are positioned so that all half-electorates have the same length, except for end electorates, and $n_1 = n_q = 2$. That is, $\ell(I_k^L) = \ell(I_j^R) = I^p$ for all $k \neq 1$, $j \neq q$. Then, we place x^1 and x^q so that (iii) is satisfied, from which it follows that $I^p = \frac{1}{2q}(1-c)$. By construction, then, (iv) and (v) are satisfied, and we have an NCNE.

Next, we show that there are infinitely many NCNE for $m \geq 6$. If m is even, construct a profile as above, but with $q = (m+2)/2$ occupied positions, all of them occupied by two candidates except for the two innermost positions, x^k and x^{k+1}, which are occupied by only one candidate each, and all half-electorates except for the outermost of the same length. This will be an equilibrium by the argument of the previous paragraph, with x^1 and x^q chosen to satisfy (iii). Let us increase the length of each half-electorate except for I_k^R and I_{k+1}^L by $\varepsilon > 0$, so that $I^{p'} = I^p + \varepsilon$ (that is, we are moving all positions inwards at the expense of the interior two candidates). This maintains (i)-(iii). Condition (iv) will still be satisfied since the only unpaired candidates are those at x^k and x^{k+1}, who have full-electorates of length $\ell(I_k) = I^{p'} + \ell(I_k^R) > \max\{I^{p'}, \ell(I_k^R)\}$. Clearly, (v) will still be satisfied, since we are increasing the length of I^p and decreasing the length I_k^R and I_{k+1}^L.

If there is an odd number of candidates $m \geq 7$ we can do a similar thing. We start with $q = (m+3)/2$ occupied positions, symmetric about the median, which is occupied by a single candidate. The two innermost occupied positions to the left and right of the median are also occupied by single candidates. Label the occupied position at the median as x^k. All occupied positions other than these three have two candidates apiece, and we place them so that all half-electorates except the outermost are of the same length. Again, choose x^1 and x^q so that (iii) is satisfied. Now, increase the length of all half-electorates except for I_k^L and I_k^R by $\varepsilon > 0$. As above, this maintains (i)-(iii). Condition (iv) will clearly still be satisfied for all $i \neq k$. For I_k, the full electorate is getting smaller, but $\ell(I_k) = 2I^p - J\varepsilon > I^p + \varepsilon$ for small ε, where J is the number of half intervals to one side of the median that increase in length. Condition (v) will still be satisfied, since the paired candidates' half-electorates I^p are increasing in length, while the unpaired candidates' half-electorates are either increasing at the same rate, or getting smaller in the case of I_k^L and I_k^R. □

An important consequence of Theorem 3.4.3 is that plurality rule produces the most dispersed equilibria, while incorporating a negative vote pulls the platforms inward. Essentially, the correct choice of c allows an election designer to pick any level of equilibrium dispersion between that of plurality and full convergence, an important result given the tradeoff between moderation and representation discussed in the Introduction. This is stated in Corollary 3.4.5.

Corollary 3.4.5. *For a given configuration of candidates, i.e., fixing q and n_i for $i \in [q]$, but allowing x^i to vary, the most extreme equilibria occur under plurality, and increasing c lowers the attainable levels of dispersion.*

Proof. Given a number of occupied positions q and the number of candidates n_i at each of them, maximising dispersion consists, essentially, in minimising the location of x^1. By (iii), then, we want to minimise I^p. Looking at condition (v), we can see that we will want to to have $\ell(I_k^L) = \ell(I_k^R) = I^p$, which will then imply that (iv) is satisfied. This will partition the issue space into $2q$ intervals I^p and two intervals of length $c/2$. Thus, $2qI^p + c = 1$, so that $I^p = \frac{1}{2q}(1-c)$ and, hence, $x^1 = \frac{1}{2q}(1+c(q-1))$. Increasing c, then, increases x^1, leading to less dispersed equilibria. □

It is also important to consider which candidates receive the most support in equilibrium, as this may determine which platform will be implemented or the distribution of power in parliament, depending on the context. Corollary 3.4.6 shows that, for any $m > 4$, the candidates that adopt the most extreme platforms never win a strictly larger share of the vote than any other candidate and, thus, can never outperform more centrist candidates. Additionally, there always exist NCNE in which the candidates that adopt the most extreme platforms receive strictly less than some other candidate. In these NCNE, one or more less extreme (and unpaired) candidates have a strictly larger vote share.

Corollary 3.4.6. *In NCNE, all paired candidates receive the same vote share, which may* *not strictly exceed an unpaired candidate's share. Thus, candidates at x^1 and x^q cannot do strictly better than any less extreme candidate. Moreover, for any $m > 4$, there always exists an NCNE in which candidates at x^1 and x^q receive strictly less votes than at least one less extreme (unpaired) candidate.*

Proof. The first statement follows from Lemma 3.8.3 and Theorem 3.4.3(iv). For the second statement, the case $m = 5$ follow by Corollary 3.4.7. For $m \geq 6$, such NCNE were constructed in the second and third paragraphs of the proof of Corollary 3.4.4. Namely, we position the candidates so that: there is at least one unpaired candidate; $n_1 = n_q = 2$; x^1 and x^q are positioned so that Theorem 3.4.3(iii) is satisfied; and, all half-electorates are the same length, except for end electorates. In this NCNE, an unpaired candidate obtains twice the score of a paired candidate, since they have the same full-electorate, but do not have to share it. □

In the case of four or five candidates, there is a unique NCNE.

Corollary 3.4.7. *If $m = 4$, then there is a unique NCNE, given by profile $x = ((x^1, 2), (1 - x^1, 2))$, where $x^1 = \frac{1}{4}(1 + c)$. If $m = 5$, then there is a unique NCNE, given by profile $x = ((x^1, 2), (1/2, 1), (1 - x^1, 2))$, where $x^1 = \frac{1}{6}(1 + 2c)$.*

In the four- and five-candidate cases, as expected by Corollary 3.4.5, the amount of dispersion observed in the candidates' positions depends on c and is maximal when the rule is plurality. As c grows towards 1, the positions converge at the median voter position. As c increases beyond 1, by Proposition 3.4.1, we know that infinitely many CNE are possible in an interval that becomes increasingly wide. Hence, there is a bifurcation point that divides CNE from NCNE when $c = 1$. Moving away from this point, more extreme positions are possible—on one side they take the form of CNE, and on the other side they are NCNE.

The six candidate case admits infinitely many equilibria for a given rule, but the pattern is similar.

Example. With six candidates, there are two possible configurations in an NCNE: we can have three occupied positions with two candidates apiece; or, we can have four occupied positions where the inner two positions are occupied by single candidates. Consider the latter profile first—the former will turn out to be a limiting case of the latter. If (i)-(iii) of Theorem 3.4.3 are satisfied, condition (iv) will always be true, since the unpaired candidate at x^2 has full-electorate length $\ell(I_2) = I^p + \ell(I_2^R)$, which is clearly larger than all other half-electorates excluding end electorates, which are of length either I^p or $\ell(I_2^R) = \ell(I_3^L)$. Thus, the only restriction is condition (v).

To get a maximally dispersed equilibrium, we want I^p to be as small as possible, which means setting $I^p = \ell(I_2^R) = \ell(I_3^L)$. This gives equilibrium profile $x = ((x^1,2),(x^2,1),(1-x^2,1),(1-x^1,2))$ where $x^1 = \frac{1}{8}(1+3c)$ and $x^2 = \frac{3}{8}(1+c)$. A number of these maximally dispersed equilibria are pictured in Figure 3.1 for a few different values of c.

To obtain a minimally dispersed equilibrium, we want I^p to be as large as possible. Condition (iv) will always be satisfied, while condition (v) will still be satisfied if the length of the half-electorates I_2^R and I_3^L go to zero. There, the interior two candidates converge at the median and we are left with minimally dispersed equilibrium profile $x = ((x^1,2),(1/2,2),(1-x^1,2))$ where $x^1 = \frac{1}{6}(1+2c)$. Thus, the unique equilibrium with three occupied positions is the limiting case of the equilibria with four occupied positions.

These equilibria are depicted in Figure 3.2.

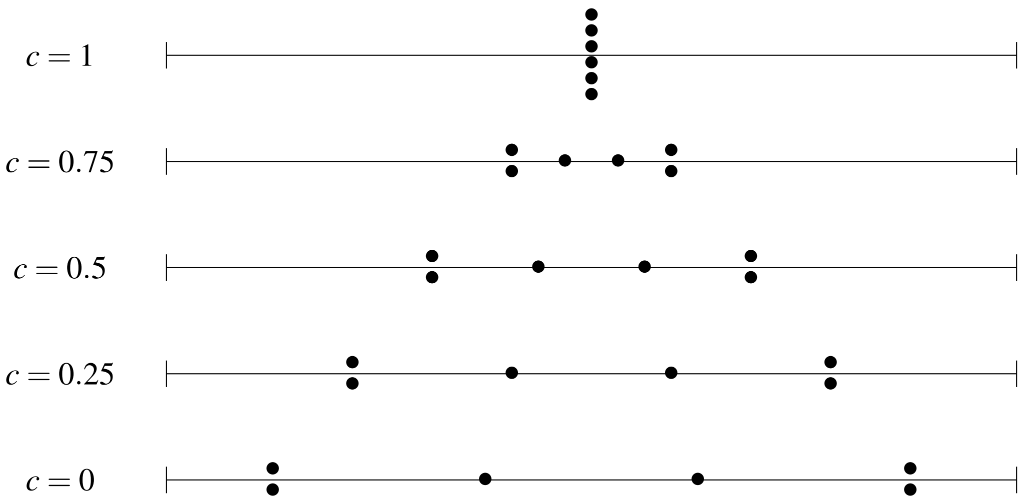

Figure 3.1: Maximally dispersed NCNE for different choices of c.

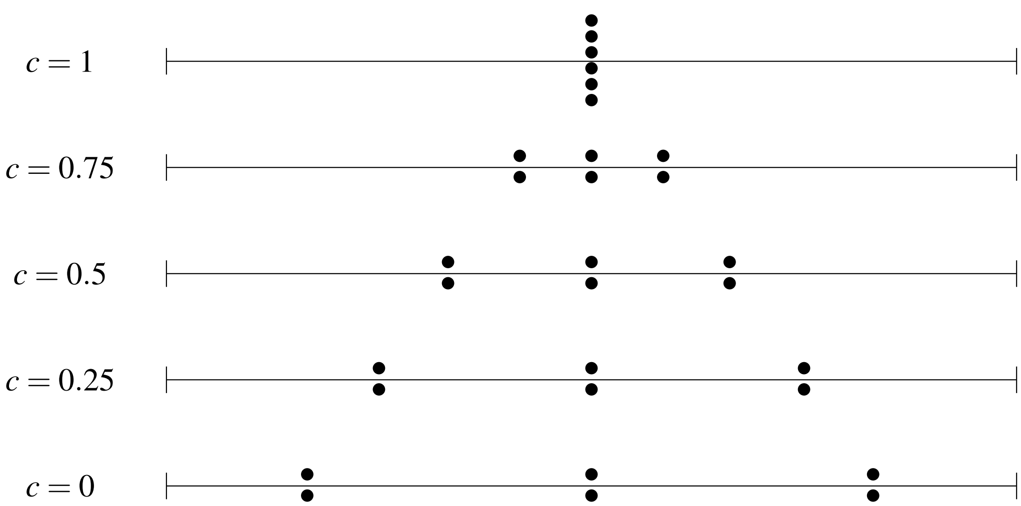

Figure 3.2: Minimally dispersed NCNE for different choices of c.

3.5 Discussion and extensions

Here we discuss a number of the assumptions underlying our model and the extent to which the results extend to more general settings.

3.5.1 Uniform distribution

Eaton and Lipsey (1975) showed that when the assumption of a uniform distribution is relaxed, equilibria seldom exist under plurality. Cox (1990) conjectures that the same is true for scoring rules, and Osborne (1993) elaborates on and extends the generality of the arguments (see also Osborne, 1995, Bol et al., 2016, and Xefteris, 2016, for discussions pertaining to a range of settings, including ours). Our results are subject to the same critique, and small deviations from uniformity would normally lead to nonexistence of NCNE.

While the assumption of uniformity may appear quite restrictive, it has been widely used in the literature, and there are a number of justifications aside from its simplicity. First, it has been noted (Aragonés and Xefteris, 2012; Cahan and Slinko, 2017) that the distribution of voter ideal points does not literally need to be uniform—all we need is that the candidates *believe* the distribution to be uniform or *assume* it as a simplifying assumption in their calculations, which is already substantially more realistic.

Second, our results also extend to distributions that may be arbitrarily non-uniform in the tails. That is, if the profile $x = ((x^1, n_1), \ldots, (x^q, n_q))$ is an NCNE for a uniform distribution, it will still be an NCNE if we distort the shape of the distribution outside of the interval (x^1, x^q), while keeping the mass in each tail the same.[13] This observation is implicit

[13]For a uniform distribution, the fraction of voters in an interval is proportional to the interval's length, which makes the vote share constant between occupied positions. Deviations from uniformity creates peaks

in the result of Eaton and Lipsey (1975) and helps somewhat to alleviate concerns about the implausible step-function nature of the uniform distribution. Perhaps more importantly, combining this argument with our results for best-worst rules leads to a stronger converse of sorts—for any distribution that is uniform on some open interval containing the median voter's ideal point, NCNE exist for $c < 1$, when c is close enough to 1. This is because, as c tends towards 1, the amount of possible dispersion is reduced to the point of full convergence at the median position—at some point all the candidates' adopted positions will lie within this uniform part of the distribution. It is not at all unlikely that the candidates assume the central part of the distribution to be uniform. Indeed, any smooth distribution will approximate a uniform distribution when we zoom in enough and, in many cases, this central part of the distribution could realistically be quite large.

3.5.2 Candidate objectives

We focus on candidates who aim to maximise their vote share. This assumption is natural in a proportional representation setting, where seats are assigned to parties according to the share of the vote obtained. It is perhaps less natural in settings where the winner takes all and the losers end up with nothing.[14]

When candidates seek only to win—i.e., they are indifferent between any outcomes that give them the same probability of being ranked first among the candidates—the well known results for vote maximisation (Eaton and Lipsey, 1975; Denzau et al., 1985; Cox,

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and troughs in the vote share function, which can induces candidates to deviate. However, the best deviation into the tail region is always just to the left of x^1 or just to the right of x^q, regardless of nonlinearities outside these points.

[14]See also Stigler's (1972) argument for the assumption of vote maximisation, as well as a discussion in Denzau et al. (1985).

1987) are substantially different. In particular, Chisik and Lemke (2006) show that, with plurality and three candidates, NCNE exist (they do not exist for vote maximisers) in which one candidate wins outright, and two candidates tie for second (acting as "spoilers" for each other). In Proposition 3.8.5 in the Appendix, we extend this result to the case of best-worst rules. We find that NCNE for best-worst rules take a similar form to NCNE under plurality. In addition—and highly reminiscent of the results under vote maximisation—the importance of the negative vote should not exceed that of a positive vote and, moreover, the negative vote acts as a moderating force on possible equilibrium positions.

Cox (1987) studies a few more plausible objectives. Under plurality maximisation, candidates seek to maximise their margin with respect to the best of their competitors: $v_i(x) - \max_{j \neq i} v_j(x)$. Complete plurality maximisation is similar but candidates care about their margins with respect to all other candidates in the race. Cox's characterisation of CNE easily extends to plurality and complete plurality maximising candidates. With CNE, there is only one occupied position and the calculation is straightforward. With NCNE, considering margins rather than vote shares increases the complexity of the calculations significantly, and we do not know whether our results generalise. We leave this as an open question for future research.

3.5.3 Multiple positive and negative votes

We have considered rules in which voters are endowed with a single negative vote and a single positive vote. A natural generalisation would be a case in which voters have d_1 positive votes and d_2 negative votes. As before, a positive vote earns a candidate 1 point while a negative vote is worth $-c$ points. A rule of this kind can be described by a four-tuple $s = (c, d_1, d_2, m)$.[15] We refer to this class of rules as *generalised* best-worst voting rules as opposed to the *standard* best-worst rules where $d_1 = d_2 = 1$.

As the number of candidates grows, the combinatorics of generalised best-worst rules quickly become daunting, as illustrated by the six-candidate example below. A complete characterisation of NCNE is not straightforward, though we are able to make some progress for the special cases of four, five and six candidates. In the first two cases, only standard best-worst rules allow NCNE.

Proposition 3.5.1. *For $m = 4$ or $m = 5$, the rule $s = (c, d_1, d_2, m)$ allows NCNE only if $d_1 = d_2 = 1$, in which case NCNE are described by Corollary 3.4.7.*

Proof. It can be verified through straightforward but tedious calculations. □

For six candidates, on the other hand, NCNE do exist more broadly. We investigate NCNE of the form $x = ((x^1, 3), (1 - x^1, 3))$ in Example 3.8.6 in the Appendix. We find that rich equilibrium behaviour may be observed for generalised best-worst voting rules. In many cases, properties reminiscent of the standard case carry through—the weight placed on negative votes is bounded above and increasing c reduces the amount of dispersion that may be observed in NCNE. This behaviour, however, is no longer the only show in town, and in one case we see quite the opposite properties. Investigating generalised best-worst rules further would take us outside the scope of this paper, but would be a fruitful avenue for future research.

[15] In scoring rule notation, $s = (\underbrace{1, \ldots, 1}_{d_1}, 0, \ldots, 0, \underbrace{-c, \ldots, -c}_{d_2})$.

3.6 Conclusion

Different voting systems provide political candidates with different incentives and, hence, lead to different outcomes, not all of which are socially desirable. One would usually want a voting system in which adopting extremist positions is not encouraged while, at the same time, voters are presented with some choice over the policies advocated by the candidates. One might also prefer that the candidates that choose the most extreme positions do not win the greatest electoral support. We have shown that the class of best-worst rules offers a solid middle ground when voters have one positive vote and one negative vote of relatively less importance, i.e., so that one negative vote does not cancel out one positive vote. In particular, nonconvergent equilibria exist, and candidates adopt diffferent platforms in a very similar way to under plurality. Importantly, however, the strong best-rewarding incentives of plurality are tempered by the need to avoid negative votes and, indeed, any degree of dispersion between the extreme cases of plurality and full convergence of antiplurality can be obtained for the correct relative importance of the negative vote. The need to avoid negative votes leads candidates to moderate their platforms, but without sacrificing diversity entirely. Moreover, when there are at least five candidates, there always exist equilibria in which the most extreme candidates do not receive the most support.

Though natural, best-worst rules have not been used in practice, as is the case for many of the voting rules studied in the social choice literature. However, our results provide evidence that this system is worthy of consideration and presents several desirable properties.

Future research should investigate the properties of best-worst voting rules in more realistic spatial models with, for example, strategic or probabilistic voting, or endogenous candidacy. It would also be useful to comprehensively study generalised best-worst voting rules, as well alternative candidate objectives.

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3.8 Appendix

3.8.1 Preliminary results and lemmata

We include here a number of lemmata that are needed for our main results. Several of these minor results are adapted from results in Cahan and Slinko (2017), though similar conditions have appeared in various form in the previous literature since at least Eaton and Lipsey (1975).

The first lemma tells us that the most extreme occupied positions cannot be occupied by single candidates, and they cannot be located at the most extreme points on the issue space.

Lemma 3.8.1 (Cahan and Slinko, 2017)**.** *In an NCNE, we must have* $n_1, n_q \geq 2$*. Moreover, no candidate may adopt the most extreme positions on the issue space. That is,* $0 < x^1$ *and* $x^q < 1$*.*

Proof. Evidently, an unpaired candidate at x^1 could move to the right and capture a larger share of positive votes and, at the same time, reduce the number of negative votes.

To see the second part, suppose $x^1 = 0$. Then the at least two candidates at x^1 are ranked last equal by a positive measure of voters in the interval $(1 - \frac{1}{2}x^q, 1]$. By moving to x^{1+}, however, a candidate originally at x^1 is no longer ranked last by any voters, but still receives the same number of first-place rankings. □

The next lemma puts a condition on the continuity of the function $v_i(t, x_{-i})$ when, in equilibrium, i is at a position occupied by one other candidate and makes a small deviation.

Lemma 3.8.2. *Suppose at profile* x *candidate* i *is at* x^l *and* $n_l = 2$*. Then* $v_i(x^{l-}, x_{-i}) + v_i(x^{l+}, x_{-i}) = 2v_i(x)$*. In particular, when* x *is in NCNE, we have* $v_i(x^{l-}, x_{-i}) = v_i(x^{l+}, x_{-i}) = v_i(x)$*.*

Proof. The issue space can be divided into subintervals of voters who all rank i in the same position. The immediate interval around x^l, $I_l = I_l^L \cup I_l^R$, is the set of voters from which the candidate receives positive votes. Let J be the interval of voters from which i receives negative votes. In particular, J is nonempty only if $l = 1$ or $l = q$, and it is located at the opposite side of the issue space. Thus, if $l \notin \{1, q\}$, we have $v_i(x) = \frac{1}{2}(\ell(I_l^L) + \ell(I_l^R))$. Then $v_i(x^{l-}, x_{-i}) = \ell(I_l^L)$ and $v_i(x^{l+}, x_{-i}) = \ell(I_l^R)$. For NCNE, we need $v_i(x^{l-}, x_{-i}) \leq v_i(x)$ and $v_i(x^{l+}, x_{-i}) \leq v_i(x)$. Summing these inequalities, we need $v_i(x^{l-}, x_{-i}) + v_i(x^{l+}, x_{-i}) \leq$

$2v_i(x)$. This, in fact, turns out to be an equality, so that we must have $v_i(x^{l-}, x_{-i}) = v_i(x^{l+}, x_{-i}) = v_i(x)$.

If $l = 1$ (symmetrically for $l = q$), we have $v_i(x) = \frac{1}{2}(\ell(I_l^L) + \ell(I_l^R)) - \frac{c}{2}\ell(J)$. Also, $v_i(x^{l-}, x_{-i}) = \ell(I_l^L) - c\ell(J)$ and $v_i(x^{l+}, x_{-i}) = \ell(I_l^R)$. As in the previous case, summing the requirements that these two moves not be beneficial, we find that $v_i(x^{l-}, x_{-i}) = v_i(x^{l+}, x_{-i}) = v_i(x)$. □

Lemma 3.8.3. *If $n_i = n_j = 2$, then $v_i(x) = v_j(x)$ in NCNE.*

Proof. Let k be a candidate at x^i and l be a candidate at x^j. Note that if k moves to x^{j+} or x^{j-}, due to the nature of the voting rule, k receives exactly the same score as l would recieve on moving to x^{j+} or x^{j-}. So $v_k(x^{j+}, x_{-k}) = v_l(x^{j+}, x_{-l})$. Then, if x is in NCNE, using Lemma 3.8.2 gives that $v_l(x) = v_l(x^{j+}, x_{-l}) = v_k(x^{j+}, x_{-k}) \leq v_k(x)$. Similarly, $v_l(x^{i+}, x_{-l}) = v_k(x^{i+}, x_{-k})$, from which it follows that $v_k(x) = v_k(x^{i+}, x_{-k}) = v_l(x^{i+}, x_{-l}) \leq v_l(x)$. So $v_k(x) = v_l(x)$. □

Next, we note that there cannot be more than two candidates at any position. In particular, this implies that there cannot exist NCNE for $m = 3$, a well known result.

Lemma 3.8.4. *In any NCNE, at any given position there are no more than two candidates. Moreover, $n_1 = n_q = 2$.*

Proof. By Corollary 3.4.2 we have to consider only the case when $c < 1$.

First, we show that, in NCNE, $n_i \leq 2$ for all $2 \leq i \leq q-1$. If $n_i > 2$, where $2 \leq i \leq q-1$, then candidate k, located at x^i is not ranked last by any voter. Moreover, she is not ranked last by any voter even on deviating to x^{i+} or x^{i-}. So the only change in her score on making these moves is from voters in the immediate subintervals $I_1 = [(x^{i-1} + x^i)/2, x^i]$

and $I_2 = [x^i, (x^i + x^{i+1})/2]$, where voters change candidate k from first equal to first, and from first equal to n_ith, respectively.

In NCNE we must have

$$v_k(x^{i-}, x_{-k}) - v_k(x) = \ell(I_1) - \frac{1}{n_i}(\ell(I_1) + \ell(I_2)) \leq 0$$

and

$$v_k(x^{i+}, x_{-k}) - v_k(x) = \ell(I_2) - \frac{1}{n_i}(\ell(I_1) + \ell(I_2)) \leq 0.$$

Adding together these two inequalities we get the requirement that $n_i \leq 2$.

To show that $n_1 = n_q = 2$, let us introduce the following notation: $I_1 = [0, x^1]$, the voters to the left of candidate 1 (note that by Lemma 3.8.1, this set has positive measure); $I_2 = [x^1, (x^1 + x^2)/2]$, the voters in half the interval between candidates 1 and 2; $I_3 = [(x^1 + x^q)/2, 1]$, the voters for whom 1 is ranked last equal.

Note that $v_1(x) = \frac{1}{n_1}(\ell(I_1) + \ell(I_2)) - \frac{c}{n_1}\ell(I_3)$. Consider if 1 moves to x^{1-}. Then $v_1(x^{1-}, x_{-1}) = \ell(I_1) - c\ell(I_3))$. If 1 moves to x^{1+} then $v_1(x^{1+}, x_{-1}) = \ell(I_2)$. For NCNE we require that these moves not be beneficial to candidate 1. That is, $v_1(x^{1-}, x_{-1}) \leq v_1(x)$ which implies we need

$$\ell(I_1) - c\ell(I_3)) \leq \frac{1}{n_1}(\ell(I_1) + \ell(I_2)) - \frac{c}{n_1}\ell(I_3),$$

or

$$\left(1 - \frac{1}{n_1}\right) c\ell(I_3) \geq \ell(I_1) - \frac{1}{n_1}(\ell(I_1) + \ell(I_2)). \tag{3.1}$$

Similarly, for the other move we have $v_1(x^{1+}, x_{-1}) \le v_1(x)$ which gives us

$$\ell(I_2) \le \frac{1}{n_1}(\ell(I_1)+\ell(I_2)) - \frac{c}{n_1}\ell(I_3),$$

implying

$$\left(1-\frac{1}{n_1}\right)c\ell(I_3) \le \left(1-\frac{1}{n_1}\right)(\ell(I_1)+\ell(I_2)) - (n_1-1)\ell(I_2).$$

Combining this last equation with (3.1) yields $(2-n_1)\ell(I_2) \ge 0$, which means $n_1 \le 2$. Hence, $n_1 = 2$, since we cannot have a lone candidate at x^1. A similar argument gives $n_q = 2$. □

3.8.2 Win maximisation and generalised best-worst voting rules

The following result and example relate to the discussion in Section 3.5.

Proposition 3.8.5. *When candidates only care about winning, the profile $x = (x_1, x_2, x_3)$, $x_1 \le x_2 < x_3$, is an NCNE in which candidate 3 wins if and only if the following are satisfied (by symmetry, a corresponding set of NCNE exists where candidate 1 wins):*

(i) $2(1+c) > (2c+3)x_2 + (2c+1)x_3$;

(ii) $2(1+c) < (2c+3)x_3 + (2c+1)x_1$;

(iii) $x_3 - x_1 < \frac{2}{3}(1-c)$.

Proof. First, there cannot be an equilibrium with a tie for first, since candidates 1 or 3 could move inwards and break the tie. Second, 2 could never win in an NCNE since we would then require that both $v_1(x_2, x_{-1}) < v_3(x_2, x_{-1})$ and $v_3(x_2, x_{-3}) < v_1(x_2, x_{-3})$, that is, neither 1 nor 3 should want to deviate to x_2. Summing together these inequalities leads to the requirement that $v_2(x) = \frac{1}{2}(x_3 - x_1) < \frac{1-c}{3(1+c)}$. If $c \geq 1$, this is impossible. If $c < 1$, note that $v_2(x) < \frac{1-c}{3(1+c)} < \frac{1}{3}(1-c)$, which contradicts that 2 is winning since to be winning 2 must receive more than 1/3 of the total votes. So there must be a unique winner—suppose without loss of generality it is candidate 3.

Assume $x_1 < x_2 < x_3$. For NCNE, 1 should not want to move to x_2^-, x_2 or x_2^+. For the first move, we need $v_1(x_2^-, x_{-1}) < v_3(x_2^-, x_{-1})$, which yields (i). If this move is not beneficial, neither will be the other two. For the second move, 1 and 2 are now tied, and since 3's score does not change (that is, $v_3(x_2, x_{-1}) = v_3(x_2^-, x_{-1})$), 3 must still be winning. For the third move, 3 is still winning, since this is the same as the first move but swapping the labels on candidates 1 and 2. Since none of these three moves are beneficial for 1, it is clear that 2 would not want to move to x_1^-, x_1 or x_1^+ (all three moves would lead to 3 winning by an even bigger margin than if 1 deviated).

We also require that 2 not want to move to x_3^+, x_3 or x_3^-. For the first move, we need $v_1(x_3^+, x_{-2}) > v_2(x_3^+, x_{-2})$, which yields (ii). If this is true then, for the second move, 2 and 3 are now tied, and since 1's score does not change (that is, $v_1(x_3, x_{-2}) = v_1(x_3^+, x_{-2})$), 1 would win here too. For the third move, 1 would again win, since this is the same as the first move but swapping the labels on candidates 2 and 3. Since 2 does not want to move to any of these three positions, it is clear that 1 would not want to move to either (candidate 2 would certainly win).

Next, suppose (iii) is not satisfied, so $x_3 - x_1 \geq \frac{2}{3}(1-c)$. Consider if candidate 2 deviates to any point t between x_1 and x_3, in which case 2's score remains constant at $(x_3 - x_1)/2$. Note also that $v_1(t, x_{-2})$ and $v_3(t, x_{-2})$ are increasing and decreasing, respectively, in t. By the above, when $t = x_1^+$, 3 is the sole winner, while 2 is the sole winner when $t = x_3^-$. Therefore, at some point t' it must be the case that $v_1(t', x_{-2}) = v_3(t', x_{-2})$. For NCNE, it must be that $v_1(t', x_{-2}) = v_3(t', x_{-2}) > v_2(t', x_{-2})$. However, the sum of the scores is fixed at $1 - c$, which contradicts the previous statement and the assumption that $x_3 - x_1 \geq \frac{2}{3}(1 - c)$. So (iii) is necessary. Since 2 does not benefit from any move between x_1 and x_3, it is also the case that 1 would not benefit from moving between x_2 and x_3.

Sufficiency of (i)-(iii) follows by construction, since we have checked all potentially profitable deviations. The above arguments also apply when $x_1 = x_2$, and are simpler because some of the deviations become redundant. □

Example 3.8.6. We investigate equilibria of the form $x = ((x^1, 3), (1 - x^1, 3))$. That is, NCNE with three candidates apiece at two symmetric occupied positions. Recall that for a standard best-worst rule, it is never possible to have three candidates at a single position in an NCNE. For generalised best-worst rules, such NCNE may indeed exist.

We may apply Theorem 5 of Cahan and Slinko (2017), which characterises "bi-positional symmetric" equilibria for arbitrary scoring rules. First, we find that NCNE cannot exist for $d_1 \geq 3$, so there can be at most two positive votes. If $d_1 = 2$, we find that bipositional symmetric NCNE exist for any $d_2 < 5$ and they exist if and only if one of the following is true:

(i) If $d_2 = 0$ (equivalently, $d_2 = 4$) and $1/6 \leq x^1 \leq 1/3$.

(ii) If $d_2 = 1$, $c < 2$, and $(1 + c)/6 \leq x^1 \leq (1 + c)/3$.

(iii) If $d_2 = 2$, $c < 1$, and $(1 + 2c)/6 \leq x^1 \leq (2 + c)/6$.

(iv) If $d_2 = 3$, $c \leq 1$, and $(1 + 3c)/(6(1 + c)) \leq x^1 \leq 1/3$.

In cases (ii)-(iv), we note some interesting similarities to case of standard best-worst rules described in Theorem 3.4.3. First, the weight placed on the negative votes should not be too high for NCNE to exist, although the bound need not always be 1. Second, the presence of negative votes again induces moderation in the set of possible equilibrium profiles. As c approaches its upper bound, the extreme most positions possible in NCNE move closer to the median voter (though need not converge).[16]

If $d_1 = 1$, bipositional symmetric NCNE only exist if $d_2 = 4$ and $c \geq 1$. Any x^1 satisfying $(2+c)/(6(1+c)) \leq x^1 \leq (1+2c)/(6(1+c))$ is an NCNE. This is quite different to the NCNE described above as well as NCNE for standard best-worst rules—here, there is a *lower* bound on the value of c and, as c increases, the range of possible NCNE positions becomes *wider* and includes NCNE that are *more* dispersed. This shows that while negative votes may have similar moderating effects for generalized best-worst rules as for standard best-worst rules in some cases, in other cases these patterns may break down.

[16]Case (i) is equivalent to 2-approval for any c, so it is not surprising that c does not matter.

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