

Political budget cycles in U.S. municipalities

C. Adam Bee¹ · Shawn R. Moulton²

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Abstract This paper tests for political budget cycles among U.S. municipalities. According to the political budget cycle hypothesis, in election years government officials engage in opportunistic fiscal policy manipulation for electoral gains. We test that hypothesis using data on taxes, spending, and employment for a panel of 268 U.S. cities over the period 1970–2004. While our estimates provide no evidence of altered total expenditures or taxes in election years, we do find a 0.7% increase in total municipal employment, including increases in police, education, and sanitation employment.

Keywords Political budget cycle · Local fiscal policy

JEL Classification D72 · D78 · H3 · P16

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✉ Shawn R. Moulton
shawn_moulton@abtassoc.com

C. Adam Bee
charles.adam.bee@gmail.com

¹ 3519 17th St NE, Washington, DC 20018, USA

² Abt Associates, 55 Wheeler Street, Cambridge, MA 02138, USA

1 Introduction

The principal-agent problem of asymmetric information is a central difficulty in any representative democracy. A particular case of this problem has come to be called the “political budget cycle”: in election years governments allegedly engage in opportunistic fiscal policy manipulation for electoral gains. This idea was popularized by Rogoff and Sibert (1988), who showed that rational, strategic politicians may lower taxes or increase spending in election years to signal competence to voters. Drazen and Eslava (2010) show that political budget cycles in the allocation of taxation and spending can also emerge, independently of their overall levels. While the existence of political budget cycles at the national level has been well documented, little is known about whether political budget cycles in taxes, spending, or their components exist at the municipal level in the United States.

In this paper, we test for the existence of a political budget cycle in U.S. municipalities. Further, we identify which dimensions of the budget exhibit such cycles. We regress cities’ per capita growth in taxes, expenditures, and employment on an indicator for whether the city held an election that year, while controlling for city and year fixed effects. While we find no evidence of election-year manipulation of aggregate expenditures or taxes, we do find a 0.7 % increase in aggregate municipal employment, including increased staffing in police, education, and sanitation departments, among others.

Although few studies have tested for political budget cycles in U.S. cities, identifying which dimensions of municipal budgets are manipulated in election years is important for at least three reasons. First, as noted by Drazen (2008), the non-smooth paths of expenditures and taxes implied by a political budget cycle are presumably costly to voters.¹ However, before fiscal policy manipulation can be addressed, we must first understand the extent and composition of the problem. No studies have detailed the specific mechanisms through which U.S. municipalities manipulate fiscal policy.² Second, the negative welfare consequences of political budget cycles among U.S. municipalities have the potential to be large because municipal governments comprise a sizable portion of the economy. In 2008, U.S. local governments spent \$1.6 trillion, or 11 % of GDP, and employed 12.3 million workers, or 9 % of civilian employment. Third, there is little consensus in the theoretical literature about the reasons why local politicians would manipulate fiscal policy.

This paper advances the literature on local political budget cycles both by using more comprehensive data than previous analyses and by testing for political budget cycle in the disaggregated components of expenditures and revenue. This study incorporates data from 268 cities over years 1970–2004 (after conducting the sample

¹ Specifically, the excess volatility caused by the political budget cycle reduces welfare by making it more difficult for constituents to smooth their levels of consumption over time.

² As discussed below, Levitt (1997) finds that cities increase police hiring while Krebs (2008) finds evidence of an increase in intergovernmental revenue in election years. However, no other study details how local governments manipulate total expenditures, taxes, and intergovernmental revenue, as well as their disaggregated components. In this sense, our paper adds to our understanding of local political budget cycles by describing the phenomenon in significantly more detail.

restrictions documented in Sect. 3). Previous studies were limited to cities with populations greater than 250,000 (Krebs 2008) or a short time span of only 8 years (Strate et al. 1993).³ Additionally, no previous study has tested for political budget cycles in the disaggregated components of expenditures and revenue in the U.S. context.⁴

Data on mayoral elections were generously provided by Ferreira and Gyourko (2009). Ferreira and Gyourko sent surveys to all U.S. cities, towns, villages, and townships with population greater than 25,000 in year 2000. Data on city-level revenue, tax, expenditure, and employment data come from the publicly available Annual Survey of Governments Historical Finance Database, created by the Census Bureau. We merge and edit these election and fiscal datasets, creating an unbalanced panel of 6,394 city-year observations.

The remainder of this paper proceeds as follows. Section 2 provides background on municipal government finances and the extant literature regarding political budget cycles. Section 3 describes the data used in this study, and Sect. 4 describes the empirical method. Section 5 presents the results. Section 6 concludes.

2 Literature review and background

2.1 Theoretical foundation of political budget cycles

A number of theoretical models have sought to explain why political budget cycles exist. The first political budget cycle model to incorporate strategic interactions between rational politicians and rational voters was detailed by Rogoff and Sibert (1988) and Rogoff (1990). This “adverse selection-type” model assumes each candidate has a competence level (high or low) that is known to the candidate, but unknown to voters. Voters prefer a high-type candidate, as the high-type candidate requires less revenue to provide a given level of government services. To signal competence to voters, the high-type incumbent engages in expansionary fiscal policy and increases the budget deficit in election years. Manipulating fiscal policy is more costly for low-type candidates and they do not engage in signaling.

In addition to caring about the probability of reelection, Rosenberg (1992) notes that politicians may adjust expenditures to “prepare for a rainy day,” improving their post-office career options in the event of election loss. Rosenberg claims that local government incumbents often fail to be reelected and many seek appointments in the private sector or other parts of the public sector post-election. Politicians may allocate pre-election expenditures such that the probability of post-election outlays, such as employment in the private sector, is increased.

Even if voters dislike fiscal deficits, Drazen and Eslava (2010) show that a political budget cycle in the composition of taxes and spending can emerge. Voters are interested in whether a politician’s budget composition preferences are in line with their own. An

³ Levitt (1997) exploits electoral cycles in police hiring to identify the effect of police on crime, but it does not examine political budget cycles *per se*.

⁴ Drazen and Eslava (2010) examine electoral cycles in the composition of taxes and spending in a developing country context.

opportunistic politician can show that his budget composition preferences are aligned with an electorally valuable group by increasing expenditures for goods targeted voters value more, at the expense of goods that voters value less.

Shi and Svensson (2002) model determinants of political budget cycle size. In their model the size of the political budget cycle depends on (1) the level of private benefits a politician gains from being elected, and (2) the share of informed voters. Higher private benefits of office imply a higher return to reelection and greater incentive to influence voters' perceptions prior to election. Similarly, the lower the share of informed voters, the more difficult it is for voters to distinguish between pre-electoral manipulations and mayoral competence. The model predicts that increased transparency of the budget leads to decreased fiscal policy manipulation. Shi and Svensson go on to argue that if transparency increases over time, the size of the political budget cycle should decrease over time.

2.2 Empirical evidence of political budget cycles

Empirical work on political budget cycles has focused on analyzing country-level data to determine the existence, magnitude, and composition (taxes vs. spending) of political budget cycles and how these effects vary across countries.⁵ While there have been many country-level studies, the most comprehensive country-level studies of political budget cycles are from Shi and Svensson (2002) and Brender and Drazen (2005a) Brender and Drazen (2005b).⁶ Shi and Svensson examined data from 123 developed and developing countries over a 21-year period and found that spending increased and revenues decreased in election years, leading to larger election-year deficits. This effect was larger in developing countries relative to developed countries. Brender and Drazen (2005a), using data from 74 countries over a 44-year period, showed that the stronger effect in developing countries is driven by new democracies. The political budget cycle was only present in the first few elections after a country has made the transition from non-democracy to democracy. Once elections in these new democracies are removed from the sample, the political budget cycle disappears. The fact that new democracies comprise a relatively large share of developing countries

⁵ See Shi and Svensson (2003) and Drazen (2000) for a review of the literature.

⁶ Using data from the U.S., Tufte (1978) finds evidence of a pre-election increase in social security payments and veterans' benefits. Alesina (1988), using US data over the years 1961–1985 finds an increase in net transfers over GNP in election years. Alesina et al. (1992), Alesina et al. (1997) examine data from thirteen OECD countries over years 1961–1993 and find that the government budget deficit is 0.6% of GDP higher in election years. There has also been substantial work examining the prevalence of country-level political budget cycles in developing countries. According to Schuknecht (1996), developing countries have greater potential to manipulate fiscal policy, as checks and balances are weaker and the incumbent has more power over fiscal policy. Schuknecht examines 35 developing countries over years 1970–1992 and finds that the deficit is increased in election years, but decreased the year following an election. Using data from 17 Latin American countries over years 1947–1982, Ames (1987) finds that government expenditures increased by 6.3% in election years, but decreased by 7.6% in the year after elections. Block (2000) used data from 44 sub-Saharan African countries over years 1980–1995 and finds that the deficit, as a share of GDP, increases by 1.0–2.6% in election years and decreases by 1.5% in the year after an election.

explains the stronger developing country effect. [Brender and Drazen \(2005b\)](#) go on to show that the election-year deficits have no effect on the probability of reelection.

Little work has tested for political budget cycles at the municipal level, particularly in the U.S.⁷ [Strate et al. \(1993\)](#) use data from U.S. cities with population greater than 100,000 over years 1978–1985 and find no evidence that local governments manipulate taxes in election years. Using data from the 58 largest U.S. cities over years 1970–1992, [Krebs](#) finds that in election years local governments tend to replace own-source revenue with increased state intergovernmental transfers. Other work has concentrated on individual components of the municipal budget. [Levitt \(1997\)](#) shows that an electoral cycle in police hiring exists in large U.S. cities, and then uses elections as an instrument for police hiring to estimate the causal effect of police hiring on crime rates. [Finkelstein \(2009\)](#) shows that toll increases are significantly lower during state election years, and that this election year difference is attenuated by the implementation of electronic tolling.

2.3 Mayoral elections and municipal government fiscal policy

In this section, we begin by providing institutional context related to mayoral elections. We then describe the environment in which local governments operate, including the fiscal policy tools available to local governments as well as the constraints faced by local governments. Finally, we describe how local government expenditures, taxes, and intergovernmental revenue have changed over time.

As described in detail in Sect. 3, our sample includes a panel of 268 U.S. cities that directly elect a mayor over the period 1970–2004. Mayoral term lengths vary across the 268 cities in our sample: 41 cities hold elections every two years, 82 cities hold elections every 4 years, and 145 cities fall into some other category (e.g., they hold elections every three years, they hold elections every six years, or they changed their term lengths at some point in our sample timeframe). In total, we observe a mayoral election in 30% of the city-year observations in our sample, where only 16% of mayoral elections occurred in the same year as a presidential election. Most (63%) of mayoral elections in our sample are non-partisan.

Most U.S. cities are constrained in their ability to alter revenue and spending in ways that state and federal governments are not. Forty-six states have some form of statewide limitation on the fiscal behavior of their units of local government ([Mullins and Wallin 2004](#)). These limitations take the form of total revenue or expenditure limits; limits on property value increases through reassessments; and property tax rate and revenue limits.

Limits on property tax rates and revenue have the potential to be especially restrictive because there is some evidence that manipulating property taxes is a relatively easy way for local governments to generate additional revenue. According to [Anderson and Pape \(2008\)](#) and [Bogart and Bradford \(1990\)](#), local governments treat property taxes

⁷ [Veiga and Veiga \(2007\)](#) and [Foucault et al. \(2008\)](#) find evidence of increased pre-election expenditures at the municipal level in Portugal and France, respectively. [Drazen and Eslava \(2010\)](#), using municipal election data from Colombia, find evidence of changes in the composition of expenditures in election years.

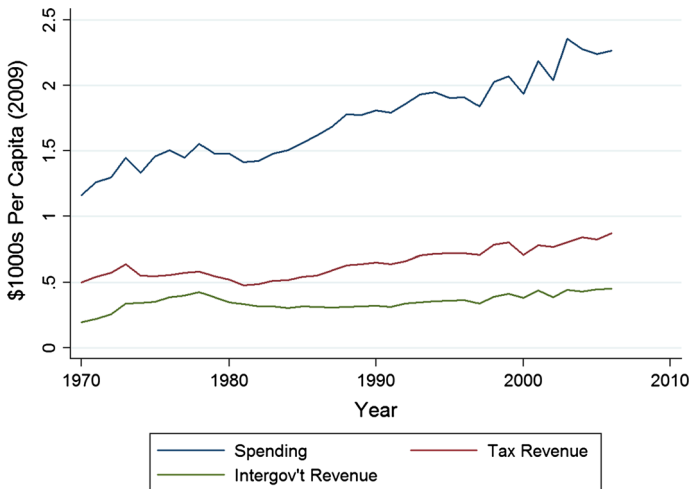


Fig. 1 Trends in local fiscal policy

as a “residual tax,” where the property tax each year is determined by the difference between desired expenditures and all other revenue sources. Consequently, as [Anderson \(2006\)](#) notes, income and sales tax rates rarely change from year-to-year, while property tax rates are constantly changing. For example, [Anderson \(2006\)](#) shows that between 1995 and 1996, every city and township in Minnesota with population over 500 altered their property tax rate. Thus, local property tax limitations are important in that they have the potential to attenuate the political budget cycle.

Following [Poterba and Rubein \(1995\)](#), we empirically test for whether political budget cycles in taxes, spending, or their components are smaller in cities located in states with local fiscal policy limitations compared to cities without such limitations. Following [Mullins and Cox \(1995\)](#) and [Poterba and Rubein \(1995\)](#), we define states as having effective local government property tax limits as those that limit property-tax revenues, property tax rates, or general revenues or expenditures.⁸ While we cannot reject that political budget cycles are the same in cities with and without fiscal policy limitations, our estimates are imprecise.

Figure 1 shows how local government expenditures, taxes, and intergovernmental revenue have changed over time. Per capita expenditures, taxes, and intergovernmental revenue, all measured in real 2009 dollars, have roughly doubled between 1970 and 2006. Additionally, we observe that these measures increased steadily over time, which may indicate that municipal expenditures, taxes, and intergovernmental revenue remain relatively stable when met with outside forces such as recessions or presidential elections.

⁸ [Poterba and Rubein \(1995\)](#) define the following 11 states as those that do not have effective limits: Connecticut, Georgia, Hawaii, Maine, Maryland, New Hampshire, South Carolina, Tennessee, Vermont, Virginia, and Wisconsin.

3 Data

To test for a political budget cycle, we match municipal fiscal data to dates of mayoral elections. Data on mayoral elections from years 1950–2006 were generously provided by [Ferreira and Gyourko \(2009\)](#). Ferreira and Gyourko sent surveys to the 1,892 U.S. cities and townships with population greater than 25,000 in year 2000. They requested information on mayoral elections since 1950 including the timing (year and month) of the election, names of the mayor and second-place candidate, aggregate vote totals and vote totals for each candidate, candidate party affiliation, *etc.* The data we received from Ferreira and Gyourko in July 2010 included 899 municipalities.⁹

Data on city-level revenue, expenditure, and employment data for years 1967 and 1970–2006 are from the publicly available, time-consistent, Annual Survey of Governments Historical Finance Database ([U.S. Census Bureau 1970–2006](#)). This dataset provides detailed, annual information on local government finances. Some relevant variables for our analysis include total revenue and revenue from intergovernmental transfers; total, property, sales, and public utility taxes; total, police, parks and recreation, payroll, education, and construction expenditures; and the number of government workers employed across various departments.¹⁰

We merge these mayoral election and Annual Survey of Governments data, creating an unbalanced panel of 22,007 city-year observations from 745 cities across years 1970–2006.¹¹ We take several measures to clean our data, beginning by dropping 58 city-year observations with missing population, total tax, or total expenditure data.

Measurement error in the election-year indicator variable is a concern in the raw election data. The universe of the Ferreira and Gyourko data is limited to city-years in which an election was held, so if a city-year is not in their dataset it is difficult to determine whether it is because an election was not held or because an election was held but the survey respondents did not provide data for it. It is therefore difficult to distinguish non-election years from missing data. Measurement error in the election-year indicator variable will bias our results toward zero, so we undertake a series of sample restrictions to obtain the cleanest election data possible.¹²

The proportion of cities holding elections declines unexpectedly in 2005 and 2006, indicating that a number of city-years are falsely coded as non-election years.¹³ We therefore drop data from years 2005–2006, leaving 21,026 city-year observations from 745 cities over the period 1970–2004. Additionally, the proportion of years with

⁹ [Smart et al. \(2011\)](#), using data on German local governments, find evidence that changing from a council-manager system to a mayor-council system leads to increased local government expenditures. Since the sample used in our analysis is limited to city-years in which a mayor-council system is used, we do not expect that this “switching forms of government” effect is influencing our results.

¹⁰ Other descriptions of this dataset can be found in [Brooks et al. \(2011\)](#) and [Brooks and Phillips \(2010\)](#).

¹¹ To examine year-to-year growth rates in spending we need consecutive years of fiscal data, so we forgo the use of 1967 data.

¹² If an election takes place in a city-year, but we do not directly observe this election, it will be falsely recorded as a non-election year. If we then compare outcomes for election years and non-election years, the difference in means between these two groups would be biased downwards.

¹³ Elections were held in 27.5% of city-year observations in 2003–2004, but only 18.5% of city-year observations in 2005–2006.

observed elections was substantially higher in larger cities; as such, we dropped all cities with a mean population below 50,000, leaving 9403 city-year observations in 287 cities.¹⁴ Finally, some cities only reported election data for a portion of the period, say, 1980–1990, leading to long stretches at the beginning and end of the period in which no elections are observed.¹⁵ We address this by dropping the 2430 observations falling two or more years before a city’s first observed election or two or more years after a city’s last observed election. This restriction leaves 6973 observations.

Ferreira and Gyourko’s mayoral election survey allows respondents to indicate whether the election was unusual in some way. Among the 8075 elections in their survey, 676 are unopposed, 29 are an appointment due to the incumbent’s death or resignation, 466 are regular appointments, 39 are runoff elections, and 17 are other kinds of “special” elections. While our estimates were not sensitive to the deletion of these observations, we follow Ferreira and Gyourko (2009) in dropping these 297 election city-years, leaving 6676 observations from 286 cities. This restriction helps ensure the exogeneity of election timing, which we further discuss in the Methods section.

Some local governments are known as “towns” or “townships”. In some states like New Jersey or Connecticut, these are full-fledged municipalities with elected mayors and a wide range of public services. In other states (Indiana and Illinois) these are minor civil divisions, providing few services and having no mayor. These minor civil divisions often contain a portion of a municipality sharing the same name. For example, the Downers Grove Township minor civil division in Illinois contains a portion of the Village of Downers Grove, and it is only the latter of these which elects a mayor. The mayoral election data for some municipalities is sometimes erroneously paired to townships, so we drop all 15 remaining towns and townships from the sample, leaving 6,490 observations in 271 cities.

Finally, we dropped city-year observations with an exceptionally large or small value of total taxes or expenditures, relative to their trend in expenditures or taxes. Specifically, we regressed log total expenditures (and log total tax revenues) on a city-specific time trend. We then dropped city-year observations for which the absolute value of its residual was in the top two percentiles of the sample. Specifically, 98 % of city-year observations had a value of total expenditures within 65 % of that city’s long-term trend in expenditures. We are left with 6394 city-year observations from 268 cities.¹⁶

¹⁴ Elections were held in 24.8 % of city-year observations with population less than 50,000, and 29.3 % of city-year observations with population greater than or equal to 50,000. Hoover, AL; Miramar, FL; Mount Vernon, IN; Wells, NV; and Newark, OH, are all examples of cities with population less than 50,000 reporting only one election in years 1970–2004. This could be due to underreporting of elections, as we found Hoover, AL, held elections every 4 years from 1980 through 2008 (Velasco 2000; U.S. States News 2008).

¹⁵ For instance, no elections are reported for Berkeley, CA, before 1998, but Shirley Dean was first elected Mayor of Berkeley in 1994. See <http://www.berkeley.edu/news/berkeleyan/1998/1209/dean.html>. Similarly, no mayoral elections are reported for Las Vegas, NV, after 1999, but Oscar Goodman was reelected twice after first becoming mayor in 1999. See <http://www.deseretnews.com/article/700124524/Vegas-Mayor-Goodman-backs-wife-in-city-election.html>.

¹⁶ When testing for manipulation of education spending in election years we further restrict our sample to cities with dependent school districts. Limiting our sample to cities with dependent school districts

Table 1 reports descriptive statistics for the 6394 city-year observations in our final sample. All revenue, tax, and expenditure data are adjusted to 2009 dollars and are reported in thousands of dollars. The mean population is 240,797, and on average cities collected \$796.0 million per year, primarily from state transfers (\$172.6 million) and property taxes (\$130.6 million). The average city spent \$784.0 million per year, spending \$97.8 million on construction, \$26.5 million on parks and recreation, and \$63.2 million on police. The 89 cities in our sample with dependent school districts spent an average of \$458.1 million on education. Local governments hired an average of 5727 employees, primarily for police protection (852) and fire (418).

4 Methods

As described above, we have data at the city-year level, and we would like to test whether there is a systematic change in the growth of per capita spending or per capita taxation immediately prior to elections. As such, the ideal relationship we would like to estimate is as follows:

$$\ln\left(\frac{y_{cst}}{pop_{cst}}\right) - \ln\left(\frac{y_{cst-1}}{pop_{cst-1}}\right) = \beta_0 + \beta_1 ELECTION_{cst} + \varepsilon_{cst} \quad (1)$$

where y_{cst} represents either real tax revenues or real expenditures in city c in state s and year t and $ELECTION_{cst}$ is an indicator variable equal to one in years when city c holds an election. Each observation represents one city-year.¹⁷ The resulting estimate $\hat{\beta}_1$ would simply be the average¹⁸ additional rate of growth in election years. This specification follows that used by Levitt (1997), among others; he regressed first-differenced log per capita values on an indicator for whether an election was held in a city-year.¹⁹

Assuming $ELECTION_{cst}$ is orthogonal to ε_{cst} and measured without error, $\hat{\beta}_1$ is a consistent, unbiased estimator of the causal effect of electorally motivated manipulation on local budgets: the expected proportional change in taxes or spending in an election year, for an average city.

Footnote 16 continued

decreases our sample size from 6394 city-year observations to 1345 city-year observations. Additionally, as there were large year-to-year changes in per capita education expenditures in some cities, we delete potential education spending outliers by dropping city-year observations with a year-to-year change in per capita education expenditures greater than 100%. This leaves us with data on education expenditures for 1218 city-year observations.

¹⁷ We do this because our analysis is intended to describe the typical mayor or typical city, rather than the expected effect for the typical city resident. As a robustness check, we also tested all specifications with observations weighted by population, such that the results represent the average person rather than the average city. All results were very similar in magnitude, direction, and statistical significance, suggesting that there is little heterogeneity of effects across city-year population sizes.

¹⁸ By average we mean log-average, or geometric mean.

¹⁹ The specification of Finkelstein (2009) was also notably similar, as she regressed the first difference in the log state minimum tolls on an indicator for whether state elections are held.

Table 1 Descriptive statistics

	Obs.	Mean	SD	Cities
Population	6394	240,797	617,092	268
Election took place	6394	0.303	0.46	268
Winner's vote share	1651	0.647	0.116	240
Close election indicator (winner had <55 % of vote share)	1651	0.241	0.428	240
Incumbent running in election indicator	1936	0.565	0.496	268
<i>Revenue</i>				
Total	6394	796	4553	268
Intergovernmental federal	6088	47	219	264
Intergovernmental state	6267	173	1405	264
Total own source	6,285	576	3,022	264
Taxes	6392	271	1724	268
General sales	3436	79	359	179
Property	6278	131	768	264
Public utility	4167	22	61	203
Select sales	4970	33	137	233
<i>Expenditures</i>				
Total	6394	784	4,488	268
Construction	6087	98	401	264
Education	1318	458	1,876	89
Parks and recreation	6158	27	60	264
Police	6285	63	240	264
Salaries and wages	6261	282	1675	264
<i>Employees</i>				
Total	5618	5727	31,024	263
Financial administration	5613	124	389	263
Fire total	5382	418	1,117	249
Firefighters	4622	384	1,013	246
Highways	5585	195	605	263
Judicial	3691	114	492	250
Libraries	3554	173	394	179
Parks and recreation	5349	350	705	258
Police total	5580	852	3186	262
Police officers	4773	677	2921	256
Sanitation	5308	311	1067	258

All revenue and expenditure means and SD are in thousands of 2009 dollars

The identification strategy relies on the arbitrary timing of elections. An election typically occurs regularly every few years, regardless of circumstances, so elections are unlikely to be correlated with underlying trends in regional economic characteristics, and local business cycles are unlikely to have the same frequency as elections.

Still, we include several controls to address potential threats to the exogeneity of $ELECTION_{cst}$. First, much of the variation in the growth of per capita taxes and spending is across cities rather than within cities over time. Even weak correlation between observed election frequency and city fiscal growth would potentially bias our estimates. This could occur if wealthier cities hold elections more frequently, for example, which would bias our estimate $\hat{\beta}_1$ upward and potentially generate a Type I error. To address this concern we include city fixed effects, which absorbs each city’s average rate of spending and taxation growth.

Second, cities may be more likely to hold elections in years in which macroeconomic conditions are particularly good (or bad). For example, some local elections may be held simultaneously with Presidential elections in order to increase voter turnout and save on the costs of holding a separate election. Presidential elections may introduce uninsurable uncertainty and thereby adversely affect macroeconomic conditions, and if local governments tend to respond by increasing spending in years with a Presidential election, then this will positively bias our results. We therefore include a full set of year effects. Our preferred specification, then, is

$$\ln\left(\frac{y_{cst}}{pop_{cst}}\right) - \ln\left(\frac{y_{cst-1}}{pop_{cst-1}}\right) = \beta_1 ELECTION_{cst} + CITY_c + YEAR_t + \varepsilon_{cst} \quad (2)$$

where $CITY_c$ is a full set of dummy variables for cities and $YEAR_t$ is a full set of year effects. Errors are clustered at the city level to allow for potential autocorrelation within cities over time of per capita growth rates in taxes and spending.

This specification does not fully address omitted variables that vary by state-year, however. For example, senatorial and gubernatorial races could generate spurious correlations between fiscal growth and local elections within states, analogous to those described above in the context of Presidential elections. Further, timing of local elections may be set at the state level, and cities may be encouraged to hold elections more frequently in periods when their respective states are wealthier and more populous.

Including state-specific year effects would address this group of potential threats to consistent identification of β_1 , but it introduces other problems. First, including state-specific year effects may be overcontrolling. If some of the cyclicity in taxes and spending are generated by intergovernmental transfers at the state level, for example, those could be part of the effect we want β_1 to capture, but including state-year effects would remove that variation from β_1 . Second, including state-year effects absorbs substantially more variation in the independent variable of interest, such that less variation is available to identify the model. R^2 typically more than triples when including state-year effects, compared to models with only year effects.

Therefore as a robustness check we also regress

$$\begin{aligned} &\ln\left(\frac{y_{cst}}{pop_{cst}}\right) - \ln\left(\frac{y_{cst-1}}{pop_{cst-1}}\right) \\ &= \beta_1 ELECTION_{cst} + CITY_c + STATEYEAR_{st} + \varepsilon_{cst} \end{aligned} \quad (3)$$

where $STATEYEAR_t$ is a full set of dummy variables for all state-year cells. As above, we cluster errors by city. Full results of this robustness check are reported in the appen-

dix, as the inclusion of state-year effects generally does not change point estimates but only widens confidence intervals.

5 Results

5.1 Expenditures

Table 2 presents estimation results for four different models of log first-differenced expenditures, each including progressively more fixed effects. In the first row of Column 1, the estimated coefficient on $ELECTION_{cst}$ implies that the average election year expenditures are 0.1 % lower than those in non-election years. This estimate is small and not statistically significant, indicating that we cannot reject the null that total election-year expenditures are no different than total non-election year expenditures. Although this estimate is statistically insignificant, the parameter estimate and SE are small enough that we can reject the null that the political budget cycle increases or decreases spending by a modest amount. The 95 % confidence interval is between -0.95 and 1.10 %.

Since we include over 250 cities of all sizes, however, the variance in the growth rates of spending is quite large. Even over nearly four decades, the majority of this variance is across cities rather than within cities over time. Even incidental correlation between the frequency of elections and the long-term growth of the city's budget will yield biased estimates. We include city fixed effects to remove cross-city variation from the analysis. Column 2 of Table 2 presents the results of the city fixed-effect specification. The inclusion of city-fixed effects has little effect on our estimated coefficient.

The model presented in Column 3 of Table 2 adds controls for year effects. The inclusion of year effects removes variation in elections and spending that is shared by all cities in each year. Without these controls, any spurious correlation between years with high spending and years with more cities holding elections would bias the results.

Table 2 Expenditures in election years progressively adding fixed effects full unbalanced panel of 268 cities, 1970–2004

	(1)	(2)	(3)	(4)
<i>ELECTION</i>	−0.0011	−0.0008	0.0019	−0.0024
SE	(0.0048)	(0.0051)	(0.0047)	(0.0068)
<i>t</i> -statistic	0.22	0.15	0.42	0.35
R ²	0.000	0.015	0.082	0.376
City FE	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
State-year FE	No	No	No	Yes

DV: $\ln(\text{expenditures}_t/\text{population}_t) - \ln(\text{expenditures}_{t-1}/\text{population}_{t-1})$

Observations: 6,394 Mean(expenditures per capita): \$1,866 Number of cities: 268

SE clustered at city level. Expenditures are adjusted to 2009 dollars

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 3 Taxes in election years progressively adding fixed effects full unbalanced panel of 268 cities, 1970–2004

	(1)	(2)	(3)	(4)
<i>ELECTION</i>	0.0000	0.0003	0.0004	−0.0012
SE	(0.0033)	(0.0035)	(0.0040)	(0.0049)
<i>t</i> -statistic	0.00	0.08	0.10	0.24
R ²	0.000	0.019	0.159	0.482
City FE	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
State-year FE	No	No	No	Yes

DV: $\ln(\text{taxes}_t/\text{population}_t) - \ln(\text{taxes}_{t-1}/\text{population}_{t-1})$

Observations: 6,392

Mean(taxes per capita): \$634

Number of cities: 268

SE clustered at city level. Taxes are adjusted to 2009 dollars

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Our estimates are robust to the inclusion of year effects, as the estimated coefficient of 0.2% in Column 3 is close to zero and statistically insignificant.

Column 4 of Table 2 includes controls for state-year effects. All variables that vary only by state and year are controlled here, such as the state unemployment rate or the party of the governor or state legislature. Once again, the estimated coefficient of −0.2% is close to zero and statistically insignificant. The estimates in Columns 1–4 of Table 2 are robust to the progressive inclusion of additional fixed effects as the estimated coefficients in each column are close to zero and statistically insignificant. Our finding of no election-year spending manipulation contradicts the hypothesis that local officials engage in opportunistic fiscal policy manipulation.

5.2 Tax revenues

Table 3 presents the analogous results for tax revenues. Again the estimates in Columns 1–4 are all close to zero and statistically insignificant. Additionally, the estimates are precisely estimated as we can reject tax increases greater than 0.9% or tax decreases greater than 1.1% at the 95% level for all four specifications of Table 3. These results indicate that there is no difference in election-year tax revenues relative to non-election year tax revenues and once again contradict the hypothesis that local officials engage in opportunistic fiscal policy manipulation.

Our finding of no aggregate election-year expenditure or tax manipulation is not surprising if officials are constrained in their ability to change overall levels of spending or revenues. However, even if they are so constrained, [Drazen and Eslava \(2010\)](#) show that a political budget cycle in the composition of taxes and spending can emerge if voters are interested in whether a politician's budget composition preferences are in line with their own. Following [Drazen and Eslava \(2010\)](#), we test for changes in the composition of expenditures and taxes in Tables 4, 5, and 6.

Table 4 Selected expenditure categories in election years city and year effects, but no state-year effects full unbalanced panel of 268 cities, 1970–2004

	Total	Police	Parks and recreation	Education	Construction	Other	Salaries and wages
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>ELECTION</i>	0.0019	0.00542	0.0194	0.0245	−0.0212	0.0001	0.0042
SE	(0.0047)	(0.0049)	(0.0122)	(0.0222)	(0.0212)	(0.0058)	(0.0062)
<i>t</i> -statistic	0.42	1.11	1.59	1.11	1.00	0.02	0.67
Mean <i>per cap.</i>	\$1866	\$192	\$95	\$788	\$262	\$1148	\$658
Observations	6394	6418	6287	1218	6216	6415	6394
R ²	0.082	0.089	0.032	0.137	0.029	0.079	0.043

SE clustered at city-level. All regressions include city and state-year fixed effects. All dependent variables are of the form $\ln(Y_t/pop_t) - \ln(Y_{t-1}/pop_{t-1})$. All expenditure data are adjusted to 2009 dollars. “Other” category is sum of all other expenditure categories besides police, parks and recreation, education, and construction

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

5.3 Components of budget increases

Tables 4, 5, and 6 disaggregate the increases documented above by spending category, by employment category, and by revenue source. As discussed above, the inclusion of state-year effects absorbs too much variation to obtain reasonably sized confidence intervals, so in these tables we use specifications that include both city and year effects but not state-year effects. For completeness, however, we do include in the appendix the analogous tables with state-year effects.²⁰

Outcomes examined in Table 4 include total expenditures (repeated from Table 2 for comparison); police, parks and recreation, education and construction expenditures; all other expenditures besides those four categories; and total salaries and wages across all categories. We find little evidence that elected officials are increasing total expenditures, but we also do not find any evidence that officials manipulate the composition of spending. This stands in contrast to the results of Drazen and Eslava (2010), who find that Colombian municipalities alter the composition of spending in election years but not the overall level of spending.

When state-year effects are included in the model, point estimates are nearly identical for almost every category but confidence intervals are much larger as less variation is available to identify the coefficients of interest, as shown in Appendix Table 13²¹

²⁰ In addition, we analyzed several other categories of spending, employment, and revenues, but in the interests of space we here report only results with substantial statistical or economic significance. The rest of the results are available upon request.

²¹ The only exception is in the parks and recreation spending category. In Column 3 of Table 4, we observe a 1.9% increase in parks and recreation spending with a p value of 0.11. In Column 3 of Table 13, when state fixed effects are included, the point estimate increases to 3.0% with a p value of 0.09, marginally statistically significant at the 10% level. Tables 14 and 15 provide similar results for disaggregated employment categories and revenue sources, respectively.

Table 5 Selected employment categories in election years city and year effects, but no state-year effects full unbalanced panel of 268 cities, 1970–2004

	Total employment		Employment categories						Full- and part-time employment	
	Total (1)		Police (2)	Parks and rec. (3)	Education (4)	Sanitation (5)	Financial admin (6)	Other (7)	Full time (8)	Part time (9)
<i>ELECTION</i>	0.0067*		0.0058*	0.0033	0.0156*	0.0235**	0.0014	0.0072*	0.0041	0.0200
SE	(0.0035)		(0.0031)	(0.0131)	(0.0082)	(0.0104)	(0.0089)	(0.0040)	(0.0042)	(0.0316)
<i>t</i> -statistic	1.90		1.91	0.26	1.89	2.27	0.16	1.82	0.99	0.63
Mean per cap.	0.0153		0.0027	0.0014	0.0202	0.0011	0.0005	0.0082	0.0132	0.0022
Observations	5733		5692	5456	729	5414	5728	5733	5466	5362
R ²	0.045		0.082	0.060	0.300	0.035	0.034	0.103	0.062	0.034

SE clustered at city-level. All regressions include city and state-year fixed effects. All dependent variables are of the form $\ln(Y_t / \text{pop}_t) - \ln(Y_{t-1} / \text{pop}_{t-1})$
 *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 6 Sources of revenue in election years city and year effects, but no state-year effects full unbalanced panel of 268 cities, 1970–2004

	Total revenue (1)	Intergov. transfers (2)	Revenue from own sources (3)	Total fees (4)	Total taxes (5)	Property taxes (6)
<i>ELECTION</i>	−0.0020	0.0008	−0.0056	−0.0125	0.0004	0.0026
SE	(0.0071)	(0.0122)	(0.0072)	(0.0138)	(0.0040)	(0.0066)
<i>t</i> -statistic	0.28	0.06	0.77	0.91	0.10	0.39
Mean <i>per cap.</i>	\$1870	\$404	\$1430	\$832	\$634	\$376
Observations	6417	6409	6417	6412	6392	6410
R ²	0.160	0.109	0.180	0.138	0.159	0.168

SE clustered at city level. All regressions include city and state-year fixed effects. All dependent variables are of the form $\ln(Y_t / \text{pop}_t) - \ln(Y_{t-1} / \text{pop}_{t-1})$. All revenue data are adjusted to 2009 dollars

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 5 shows that although there is little or no cyclicity in total salaries and wages, there is a small, broadly shared increase in employment of city workers across many departments. Total employment of public workers increased an extra 0.7% in election years. Police departments grew 0.6% faster in election years, approximating the findings of [Levitt \(1997\)](#) and [McCrary \(2002\)](#). Education employees and sanitation employees, who may also be relatively visible public employees, also show large, statistically significant increases in their numbers during election years. The confidence interval on the estimate for employment in parks and recreation departments is too wide to rule out similarly sized cycles. Financial administration employees, whom one might expect to be less visible, show little or no cyclicity. Employment in all other departments increases by 0.7%, the same magnitude as total employment, which reflects the broad range of departments that hire additional employees in election years. Finally, the point estimates in Columns 8 and 9 of Table 5 indicate that employment increases were concentrated among part-time workers.

Table 6 shows that there is little or no electoral cyclicity in total revenue or its components. Point estimates are uniformly small, and confidence intervals are also relatively small. The estimate for total taxes from Table 3 is repeated in Column 5 of Table 6 for comparison to other components of revenue. Total revenue shows a slightly negative point estimate at 0.2%, and at the 5% confidence level we can rule out increases larger than 1.2% or decreases larger than 1.6%. Counter to the findings of [Krebs \(2008\)](#), we find no evidence of electoral timing in intergovernmental transfers, which constitute about a quarter of total revenue. Own-source revenue exhibits mild negative correlation with election years, but the coefficient's SE are large enough that an increase of up to 0.8% cannot be rejected. Fees represent a considerable portion of municipal funding, but they are too volatile to generate a precise estimate. Property taxes show little or no electoral cyclicity. Previous work shows property tax millage rates are adjusted annually to balance municipal budgets (see, e.g., [Anderson 2006](#), and [Anderson and Pape 2008](#)), so the absence of a cycle in property taxes may be interpreted as a reflection of the lack of a cycle in overall spending.

Table 7 Expenditure and tax growth in years preceding the election year full unbalanced panel of 268 cities, 1970–2004

	Expenditures in election years (1)	Taxes in election years (2)
Four years before election year	0.0039 (0.0050)	−0.0001 (0.0039)
Three years before election year	−0.0007 (0.0046)	−0.0050 (0.0036)
Two years before election year	−0.0021 (0.0040)	0.0027 (0.0037)
One year before election year	0.0000 (0.0051)	0.0040 (0.0040)
Election year	0.0019 (0.0047)	0.0004 (0.0040)

DV: $\ln(\text{expenditures}_t/\text{population}_t) - \ln(\text{expenditures}_{t-1}/\text{population}_{t-1})$ Number of cities: 268
 SE clustered at city level. Expenditures are adjusted to 2009 dollars. Each row presents estimates from a separate regression. The model includes city and year fixed effects

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

5.4 Election timing

The model presented in Sect. 4 assumes that an election in year t should impact the change in expenditures or taxes from year $t - 1$ to year t . This assumption is reasonable when the election is late in year t , but may not be reasonable when the election is early in the year. When the election is early in the year, expenditures or taxes may change from year $t - 2$ to year $t - 1$. We test for expenditure and tax growth for years preceding the election. As depicted in Table 7, we find no evidence of expenditure or tax growth one, two, three, or four years prior to the election year.

As an additional sensitivity check, we test whether our findings are sensitive to the frequency of mayoral elections by limiting our sample to (1) cities that always have a mayoral term length of 2 years, (2) cities that always have a mayoral term length of four years, and (3) cities that did not change their term length during our sample period. Tables 8 and 9 present our findings on the expenditure and tax revenue outcomes, respectively, for these three sets of cities. We find that the main results presented in Tables 2 and 3 are robust to different mayoral term lengths, as we find no evidence of altered total expenditures or taxes in election years for cities with 2-year mayoral terms, 4-year mayoral terms, or no changes in mayor terms over time.

5.5 Incumbent mayors and close elections

We may expect increased electorally motivated fiscal manipulation when an incumbent is running in the election or when the election is close. To test whether incumbent mayors engage in opportunistic fiscal policy manipulation, we include an interaction between *ELECTION* and an indicator for whether an incumbent is running in the elec-

Table 8 Expenditures in election years by mayoral term length

	Expenditures in election years, two year term cities (1)	Expenditures in election years, four year term cities (2)	Expenditures in election years, dropping cities with change in term length (3)
<i>ELECTION</i>	-0.0099 (0.0097)	-0.0103 (0.0074)	-0.0002 (0.0064)
R ²	0.15	0.10	0.09
Observations	864	1,817	3,693
Number of cities	43	83	140

DV: $\ln(\text{expenditures}_t/\text{population}_t) - \ln(\text{expenditures}_{t-1}/\text{population}_{t-1})$

SE clustered at city level. Expenditures are adjusted to 2009 dollars. The model includes city and year fixed effects

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 9 Taxes in election years by mayoral term length

	Taxes in election years, two year term cities (1)	Taxes in election years, four year term cities (2)	Taxes in election years, dropping cities with change in term length (3)
<i>ELECTION</i>	-0.0045 (0.0090)	-0.0032 (0.0071)	-0.0026 (0.0046)
R ²	0.23	0.17	0.17
Observations	864	1817	3691
Number of cities	43	83	140

DV: $\ln(\text{expenditures}_t/\text{population}_t) - \ln(\text{expenditures}_{t-1}/\text{population}_{t-1})$

SE clustered at city level. Expenditures are adjusted to 2009 dollars. The model includes city and year fixed effects

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

tion to our main specification. As depicted by Table 10, the *ELECTION*INCUMBENT* interaction term is not statistically significant, indicating that we cannot reject the null hypothesis that incumbent mayors do not engage in electorally motivated fiscal manipulation. To test for fiscal policy manipulation in close elections, we include an interaction between *ELECTION* and an indicator for whether the winning mayor secured less than 55 % of the vote share to our main specification. As depicted by Table 11, the *ELECTION*CLOSE ELECTION* interaction term is not statistically significant, indicating that we cannot reject the null hypothesis of no fiscal manipulation in close elections.

6 Conclusion

This paper finds evidence supporting the political budget cycle hypothesis, specifically that local officials increase city employment during election years. As employment increases were concentrated among part-time employees working in visible depart-

Table 10 Election year* incumbent interactions full unbalanced panel of 268 cities, 1970–2004

	Expenditures in election years (1)	Taxes in election years (2)
<i>ELECTION</i>	–0.0007 (0.0068)	–0.0046 (0.0063)
<i>ELECTION * INCUMBENT</i>	0.0033 (0.0072)	0.0105 (0.0066)
R ²	0.08	0.16
Observations	6374	6372

DV: $\ln(\text{expenditures}_t/\text{population}_t) - \ln(\text{expenditures}_{t-1}/\text{population}_{t-1})$ Number of cities: 268
 SE clustered at city level. Expenditures are adjusted to 2009 dollars. The model includes city and year fixed effects

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 11 Election year* close election interactions full unbalanced panel of 268 cities, 1970–2004

	Expenditures in election years (1)	Taxes in election years (2)
<i>ELECTION</i>	–0.0007 (0.0048)	0.0006 (0.0036)
<i>ELECTION * CLOSE</i>	0.0048 (0.0083)	–0.0006 (0.0084)
R ²	0.08	0.08
Observations	6394	6392

DV: $\ln(\text{expenditures}_t/\text{population}_t) - \ln(\text{expenditures}_{t-1}/\text{population}_{t-1})$ Number of cities: 268
 SE clustered at city level. Expenditures are adjusted to 2009 dollars. The model includes city and year fixed effects. An election is defined as a close election if the winning mayor secured less than 55% of the vote share

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

ments like police, education, and sanitation, this finding may reflect incumbents' desire to temporarily increase visible services in election years, without committing to long-term increases in expenditures.

We find no statistically significant changes in either total expenditures or tax revenues in election years as compared to non-election years. This may be due to fiscal policy limitations placed on municipal governments, which take the form of total revenue or expenditure limits; limits on property value increases through reassessments; and property tax rate and revenue limits. This could also be due to constraints imposed by bond markets, as cities lack the ability to run persistent deficits without risking crisis: bankruptcy or a bailout by the state, with greatly enhanced budgetary oversight.

Another possible explanation for the absence of budget cycle, and especially the puzzling combination of that absence with a slight cycle in employment, is the following. When downturns hit in non-election years, employees are laid off as tax

revenues decrease and budgets are required to stay relatively balanced. When a downturn hits during an election year, however, mayors (or their allies) may instead furlough employees or reduce their hours. When measured against the counterfactual layoffs, this phenomenon shows up as increased employment with similar levels of spending. We attempted to test for this possibility by interacting elections with state-level unemployment rates, but we lacked the statistical power to discern an effect.

An alternative explanation for our negative result is that mayors are relatively weak executives, especially compared to international heads of state. Although there are some notable exceptions (e.g., Chicago), mayors often have little to no additional power compared to city council members.²² It is important to note that municipal political budget cycles need not arise directly through a mayor's actions; her allies could also affect budgeting, or contemporaneous election of other officials could produce a measurable cycle. The fact, then, that we do not observe such a cycle speaks not only to the limited formal power of mayors but also their limited informal power. It is also worth emphasizing that our sample is composed entirely of directly elected mayors rather than those elected by councils. As noted by [Avellaneda \(2009\)](#), directly elected mayors function in a politicized environment and therefore may be prone to spend more to satisfy electoral coalitions. Given that our sample focuses on these politically motivated mayors, we would not expect to find stronger evidence for a political budget cycle using an alternative set of cities.

This explanation, however, raises the question of why local governments tend to have such weak leaders, relative to executives at the national level that have substantially greater agenda-setting and legislative power. The lack of electoral cycles in total expenditures and taxes at the local level might imply that voters may benefit from increased availability of local budget data and easier access to objective, clear estimates of counterfactual spending and taxation. If this is the case, these may represent an argument in favor of increased decentralization in the provision of public services. They may also represent ways that political budget cycles at the national level could be reduced.

Appendix

See Tables [12](#), [13](#), [14](#) and [15](#).

²² Relatedly, [Morgan and Watson \(1995\)](#) show that mayoral strength is not a significant predictor of municipal expenditures.

Table 12 U.S. Municipalities in sample

U.S. MUNICIPALITIES IN SAMPLE			
Alabama	Dos Palos	Morgan Hill	Seaside
Birmingham	El Cajon	Morro Bay	Stockton
Dothan	El Monte	Napa	Torrance
Florence	El Paso De Robles	National	Ukiah
Huntsville	Escondido	Newark	Union
Tuscaloosa	Eureka	Oakland	Upland
	Fresno	Ontario	Vallejo
	Garden Grove	Orange	Vista
Arizona	Gardena	Oroville	Waterford
Casa Grande	Gilroy	Oxnard	Watsonville
Lake Havasu	Greenfield	Pacific Grove	West Sacramento
Tempe	Hawthorne	Palm Springs	Westminster
Tucson	Hayward	Parlier	
	Hughson	Pasadena	Colorado
Arkansas	Imperial Beach	Perris	Denver
Pine Bluff	Inglewood	Pico Rivera	Ft Collins
	Irvine	Pomona	Greeley
California	La Mesa	Poway	Loveland
Alameda	La Puente	Redondo Beach	Wheat Ridge
Anaheim	La Verne	Rialto	
Antioch	Lathrop	Richmond	Connecticut
Arroyo Grande	Livermore	Rio Vista	Andover
Azusa	Livingston	Riverside	Barkhamsted
Bakersfield	Lompoc	Sacramento	Bethel
Baldwin Park	Long Beach	Salinas	Bolton
Berkeley	Los Angeles	San Bernardino	Bridgeport
Carlsbad	Los Banos	San Bruno	Bridgewater
Carmel By The Sea	Manteca	San Diego	Bristol
Ceres	Marina	San Dimas	Brookfield
Chino	Martinez	San Francisco	Brooklyn
Chula Vista	Merced	San Jose	Canterbury
Colton	Milpitas	San Leandro	Canton
Compton	Modesto	San Luis Obispo	Chester
Corning	Montclair	San Rafael	Cromwell
Coronado	Monterey	Santa Ana	Danbury
Del Rey Oaks	Monterey Park	Santa Barbara	Danielson
Derby	Waterbury	Elgin	Dubuque
East Granby	Waterford	Elk Grove	Mason
East Haddam	West Haven	Elmhurst	Kansas
East Windsor	Westbrook	Elmwood Park	Dodge
Easton	Weston	Evanston	Leawood
Fairfield	Wolcott	Glendale Heights	Shawnee
Farmington	Woodbridge	Glenview	Wichita
Greenwich	Woodstock	Granite	
Groton		Hanover Park	Kentucky
Guilford	Delaware	Harvey	Bowling Green
Haddam	Newark	Maywood	Covington
Hamden		Mt Prospect	Paducah
Harwinton	Florida	Naperville	
Lyme	Bradenton	Northbrook	Louisiana
Madison	Deltona	Peoria	Lake Charles
Marl	Ft Lauderdale	Rock Island	Shreveport
Meriden	Hialeah	Skokie	
Monroe	Hollywood	Tinley Park	Maryland
Montville	Lake Worth	Urbana	Baltimore
Morris	Lauderdale Lakes	Wheaton	
Naugatuck	Miami Beach	Wilmette	Massachusetts
New Britain	Miami		Attleboro
New Canaan	Ormond Beach	Indiana	Boston
New Haven	Pinellas Park	Bedford	Everett
New Milford	St Petersburg	Bicknell	Fall River
New	Tallahassee	Bluffton	Gloucester
North Stonington	Tampa	Elwood	Holyoke
Norwalk	West Palm Beach	Evansville	Lawrence
Norwich		Ft Wayne	Leominster
Old Saybrook	Georgia	Gary	Marl

Table 12 continued

Plainfield	Alpharetta	Greenfield	Melrose
Plymouth	Atlanta	Hammond	New Bedford
Putnam	Macon	Lafayette	Northampton
Ridgefield	Marietta	Lake Station	Peabody
Rocky Hill		Logansport	Quincy
Roxbury	<u>Illinois</u>	Michigan	Taunton
Simsbury	Addison	Muncie	Waltham
Somers	Bartlett	South Bend	Westfield
Southbury	Bloomington	Tell	
Stafford	Calumet	Terre Haute	<u>Michigan</u>
Stamford	Champaign	Washington	Allen Park
Stonington	Chicago Heights		Burton
Torrington	Cicero	<u>Iowa</u>	Dearborn Heights
Vernon	Decatur	Bettendorf	Detroit
Wallingford	Des Plaines	Council Bluffs	East Detroit
Washington	Downers Grove	Davenport	Farmington Hills
Kalamazoo	<u>New Jersey</u>	<u>Oklahoma</u>	Duncanville
Kentwood	Elizabeth	Oklahoma City	Ft Worth
Lincoln Park	Old Bridge		George
Muskegon	Westfield	<u>Oregon</u>	Grand Prairie
Oak Park		Corvallis	Haltom
Royal Oak	<u>New York</u>	Lake Oswego	Killeen
St Clair Shores	Albany	Mcminnville	Longview
Taylor	Binghamton	Medford	Mesquite
Warren	Buffalo	Oregon	Midland
Wyoming	Freeport	Portland	Pharr
	Middle		San Angelo
<u>Minnesota</u>	New Rochelle	<u>Pennsylvania</u>	San Antonio
Blaine	New York	Allen	Sherman
Bloomington	Niagara Falls	Philadelphia	Tyler
Brooklyn Center	Schenectady	Scranton	
Cottage Grove	Utica	State College	<u>Utah</u>
Minneapolis	<u>North Carolina</u>	Williamsport	Layton
Richfield	Burlington		Midvale
Rochester	Fayetteville	<u>Rhode Island</u>	Orem
Shoreview	Gastonia	Cranston	
Winona	Goldsboro	Pawtucket	<u>Virginia</u>
	Greensboro	Woonsocket	Alexandria
<u>Mississippi</u>	Greenville		Chesapeake
Jackson	Hickory	<u>South Carolina</u>	Hampton
	Raleigh	Greenville	Newport News
<u>Missouri</u>	Wilmington	Sumter	Richmond
Independence	Wilson		Roanoke
Jefferson	Winston Salem	<u>Tennessee</u>	Virginia Beach
Springfield		Chattanooga	
Wildwood	<u>North Dakota</u>	Knoxville	<u>Washington</u>
	Bismarck	Memphis	Renton
<u>Montana</u>	Fargo	Murfreesboro	Seattle
Great Falls			Tacoma
Missoula	<u>Ohio</u>	<u>Texas</u>	Vancouver
	Canton	Abilene	Yakima
<u>Nebraska</u>	Cleveland	Amarillo	
Lincoln	Columbus	Bay	<u>Wisconsin</u>
Omaha	Cuyahoga Falls	Beaumont	Brookfield
	Dayton	Bryan	Green Bay
<u>Nevada</u>	East Cleveland	Cedar Hill	La Crosse
Elko	Kent	Cleburne	Racine
Ely	Kettering	Corpus Christi	Wauwatosa
Henderson	Mansfield	Dallas	
West Wendover	Newark	Deer Park	
	Toledo	Del Rio	
		Denton	

Table 13 Selected expenditure categories in election years city and state-year effects full unbalanced panel of 268 cities, 1970–2004

	Total	Police	Parks and recreation	Education	Construction	Other	Salaries and wages
	(1)	(2)	(3)	(4)	(5)	(7)	(6)
<i>ELECTION</i>	-0.0024	0.0090	0.0296*	0.0445	-0.0557	-0.0029	-0.0010
SE	(0.0068)	(0.0075)	(0.0170)	(0.0748)	(0.0435)	(0.0062)	(0.0078)
<i>t</i> -statistic	0.35	1.20	1.71	0.60	1.28	0.46	0.13
Mean <i>per cap.</i>	\$1866	\$192	\$95	\$788	\$262	\$1148	\$658
Observations	6394	6418	6287	1218	6216	6415	6394
R ²	0.376	0.443	0.297	0.787	0.256	0.361	0.239

SE clustered at city-level. All regressions include city and state-year fixed effects. All dependent variables are of the form $\ln(Y_t/pop_t) - \ln(Y_{t-1}/pop_{t-1})$. All expenditure data are adjusted to 2009 dollars
 *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 14 Selected employment categories in election years city and state-year effects full unbalanced panel of 268 cities, 1970–2004

	Total	Police	Parks and recreation	Education	Sanitation	Financial admin.	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>ELECTION</i>	0.0070*	0.0032	-0.0029	0.0129	0.0187	0.0080	0.0053
SE	(0.0041)	(0.0039)	(0.0142)	(0.0160)	(0.0134)	(0.0106)	(0.0054)
<i>t</i> -statistic	1.70	0.81	0.20	0.81	1.40	0.75	1.00
Mean <i>per cap.</i>	0.0153	0.0027	0.0014	0.0202	0.0011	0.0005	0.0082
Observations	5733	5692	5456	729	5414	5728	5733
R ²	0.376	0.360	0.436	0.769	0.234	0.343	0.405

SE clustered at city-level. All regressions include city and state-year fixed effects. All dependent variables are of the form $\ln(Y_t/pop_t) - \ln(Y_{t-1}/pop_{t-1})$
 *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 15 Sources of revenue in election years city and state-year effects full unbalanced panel of 268 cities, 1970–2004

	Total revenue	Intergov. transfers	Revenue from own sources	Total fees	Total taxes	Property taxes
	(1)	(2)	(3)	(4)	(5)	(6)
<i>ELECTION</i>	0.0013	-0.0006	0.0035	-0.0206	-0.0012	-0.0009
SE	(0.0050)	(0.0140)	(0.0082)	(0.0168)	(0.0049)	(0.0064)
<i>t</i> -statistic	0.26	0.04	0.43	1.23	0.24	0.13
Mean <i>per cap.</i>	\$1870	\$404	\$1430	\$832	\$634	\$376
Observations	6417	6409	6412	5718	6392	6410
R ²	0.449	0.386	0.466	0.579	0.482	0.485

SE clustered at city level. All regressions include city and state-year fixed effects. All dependent variables are of the form $\ln(Y_t/pop_t) - \ln(Y_{t-1}/pop_{t-1})$. All revenue data are adjusted to 2009 dollars
 *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

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