Direct democracy, term limits, and fiscal decisions in US municipalities

Colleen Donovan *

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Abstract

This paper provides a direct test of whether the availability of the local voter initiative and recall processes and term limits influences municipal fiscal decisions in the United States. Using an adaptation of Campbell-Mankiw's (1990) model of forward-looking decision making, I find that the threats of voter initiative and recall do not significantly influence local fiscal policies. This suggests that voter preferences and local fiscal policy are aligned in the absence of these institutions. Term limits lead to increased short-sighted decision making, by removing officials' accountability to voters. This effect is roughly equivalent to removing almost all access to municipal borrowing. The analysis improves upon previous studies of US municipal fiscal policy by utilizing a panel of annual observations of government fiscal choices between 1970 and 2004 for more than 500 local jurisdictions . In doing so, the research design identifies the extent to which cities act according to a rational tax-smoothing model or are short-sighted in their fiscal decision making over time. In addition to term limits, large and diverse populations are associated with short-sighted fiscal policies.

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1 Introduction

The extent to which local governments are able to smooth expenditures over time through economic fluctuations is an unresolved question in public finance. Positive political models of taxation and revenue predict that optimizing governments will smooth taxes and revenues across periods by incorporating any knowledge of future revenues into current decisions. Governments smooth taxes and expenditures by accumulating savings during periods of economic growth and utilizing savings or borrowing during economic downturns. However, governments at both the federal and sub-national levels do not consistently smooth taxes and expenditures over time and often face insufficient revenues and savings to cover expenditures and debt obligations. Balanced budget constraints at the state and local level impair smoothing ability by restricting debt usage. Thus, reasonable deviations from perfect tax and expenditure smoothing are expected, but excess fiscal sensitivity to revenue variations leads to frequently distressed budgets.

In the extreme case of fiscal distress, cities have declared bankruptcy or received bailouts from outside agents. In 1975, New York City was famously saved by its teacher union which invested $150 million from pension funds to help the city meet debt obligations. In 1994, Orange County, California was home to the largest municipal bankruptcy in history. Complete bankruptcy is a rare occurrence; since 1937 there have been fewer than 500 Chapter 9 bankruptcy petitions filed.[1] Nonetheless, the threat of bankruptcy, drastic cuts in services, or tax increases required to avoid bankruptcy, can each have serious long-term consequences for a city and its residents. A significantly short-sighted fiscal policy is associated with volatile swings in service provision, in government employment, and in tax rates. Alone, this volatility creates dead weight loss. And, to the extent that state and federal governments provide distressed governments with additional grants, short-sighted municipal budgeting imposes a potentially significant externality on other governments.

There are several policy decisions associated with tax-smoothing. During good economic times, for example, voters are likely to seek tax cuts, not recognizing a need for excess revenues and perhaps not trusting politicians to save the funds. Indeed, politicians face various incentives to spend liquid funds to please their constituents or to implement their own agendas. But, such tax cuts or spending programs limit the ability to save liquid funds during upswings and thus the ability to utilize savings during down-

[1] http://www.uscourts.gov/bankruptcycourts/bankruptcybasics/chapter9.html

turns. Issuing debt during downturns is an alternative to saving but balanced budget requirements limit deficit financing.

In this paper, I examine several institutional factors that influence policy decisions, namely the local initiative, the local recall option, and term limits. The initiative process, whereby voters bring policies to the ballot box by collecting signatures, has come under attack for poor fiscal policy actions. With limited empirical evidence supporting any effect of the initiative on fiscal policy, the criticism has largely stemmed from a few controversial voter initiatives, like Proposition 13 in California.

The initiative can systematically affect fiscal policy if voters are consistently better or worse decision makers than politicians. Opponents of the initiative process argue that voters make poor decisions because they are not well-informed, or they use the initiative for personal gains rather than for the good of the electorate. However, proponents argue that the initiative is an important disciplining mechanism and leads to better policies.

The initiative has the potential to influence policy directly, through voter-initiated policies, and indirectly as a threat to politicians. If politicians perceive that voters will pass a law at the ballot box, then politicians stand to make electoral gains by passing it themselves.[16, 7] Whether or not the initiative process affects fiscal policy is a highly relevant policy debate; in California, opponents blame the initiative for many of the states fiscal woes.

Another form of direct democracy, the recall enables citizens to vote for a politician's removal from office by collecting signatures . The threat of being recalled from office gives politicians incentives to manipulate taxes and spending to please voters. During good economic times, for example, a politician's seat is threatened if she does not cut taxes according to voter demands. Importantly, both the initiative and recall option serve to align constituent and politician interests by giving voters direct access to ballot items.

Term limits, on the other hand, may serve to either align or misalign voter and politician interests. Since 1970, voters have increasingly supported the implementation of term limits. When term limits are implemented, it is most common to limit Mayors to two terms in office. Proponents argue that career politicians become increasingly harder to oust from office; incumbency status and fundraising capabilities provide career politicians with unfair advantages over challengers.

On the other hand, economic theory predicts that term limits misalign interest.

Firstly, there are no formal barriers to ousting a bad politician; with bad politicians voted out of office, term limits only serve to shorten the careers of good politicians. Secondly, term limits dramatically shorten a politician's time horizon, by ensuring that after typically one or two terms, the incumbent will not be eligible to reap the benefits of far-sighted behavior. As a result, she faces incentives to spend government money to implement her preferred policies, or limit the spending behavior of future policy-makers.[2] Besley and Case (1995) provide empirical evidence consistent with the theory that term limits misalign interests; Democratic governors faced with term limits spend and tax more than those eligible for re-election.

With limited empirical evidence supporting arguments for or against term limits, the debate remains relatively theoretical. I contribute to this debate by providing evidence of the effect of term limits on local fiscal policy.

In this paper, I test several hypotheses of why local fiscal policy deviates from reasonable tax-smoothing behavior. I test the roles of voter initiative, recall, term limits, and political preferences in government fiscal policy. In doing so, I contribute to ongoing policy debates surrounding the use of initiative, recall, and term limits. I also identify whether liquidity constraints and population characteristics contribute to the ongoing fiscal troubles faced by many local governments in the United States.

To identify these influences on fiscal policy, I adapt Campbell and Mankiw's (1990) model of forward-looking decision making for government spending. Originally developed to identify the extent to which individuals act according to the permanent income hypothesis, the λ-model estimates the proportions of current spending that is funded by current and permanent resources. This model has been adapted to government spending in only a few studies, all of which have been carried out using European data.[3] The only attempt to estimate the extent to which US states and municipalities are forward-looking was conducted using aggregate data.[4] With one observation for all state and local governments per year, the interpretation and credibility of the previous work is dubious. Without observing microeconomic spending and tax decisions, an estimate of aggregate government fiscal strategy does not provide any insight as to why government policies display excess sensitivity to revenue variatiability. I estimate the model using panel data

[2]See, for example, Tabellini and Alesina, 1990 [3]; Persson and Svensson, 1989[33]; Besley and Case, 1995 [6].

[3]See, for example, Dahlberg and Linstrom (1998) or Borge and Tovmo (2007).

[4]See Holtz-Eakin (1994).

from 506 US municipalities. By observing fiscal decisions for 506 cities over 35 years, I am able to identify the economic conditions and constraints that are associated with decisions to smooth taxes or to act short-sightedly.

My main findings are:

- The threat of local initiative and recall do not significantly influence local fiscal policy. However, I cannot identify the effect of voter initiated policies on fiscal outcomes.
- Municipal term limits are more closely associated with short-sighted fiscal policy.
- Ethnic diversity, liquidity constraints, and local tax structures are also important determinants of fiscal sensitivity to revenue variations.

The next section summarizes the literature related to direct democracy and term limits. I also summarize the few studies that have applied Campbell and Mankiw's model of individual consumption decisions to government spending. Section 3 presents the empirical framework, discusses the econometric obstacles, and presents an instrumental variables approach. Section 4 presents the data used in the empirical estimation. Section 5 provides the empirical results, and section 6 concludes.

2 Empirical Context

This paper is related to two areas of study. First, there is limited evidence of the effects of the initiative, recall, and term limits at the local level; most analyses of these institutions are carried out at the state or federal levels. Second, there is a series of work exploring the extent to which governments comply with an optimal tax-smoothing model. This study uses municipal data to identify the fiscal effects of the initiative, recall, and term limits. In particular, I expand upon the tax-smoothing literature to identify differences in tax-smoothing behavior related to the availability of the initiative and recall options, and the presence of term limits.

There is limited empirical evidence of the effect of the initiative, recall, and term limits at the local level. Tracy Gordon (2004) conducts one of the first studies of local initiatives. She uses data on local initiatives in California and finds that larger and more diverse cities are more likely to bring measures to the ballot. She also concludes that

cities that regularly utilize the initiative have higher per capita own-source revenues and expenditures, compared to cities that rarely bring initiatives to the ballot.

At the state level, studies of direct democracy have addressed several aspects of the initiative and recall; for example, whether voters make better or worse decisions using the initiative than do state legislators, and if voters are ill-equipped make complicated decisions. The general finding is that, on average, voters are able to utilize "informational shortcuts" and do not approve worse policy than lawmakers.[11, 26] Another branch of literature explores whether or not majority groups use the initiative to oppress minorities. The findings here are mixed.[5] Matsusaka (1995) explores the fiscal effects of voter initiatives at the state level. He finds that states with the initiative spend less and rely more on fees and charges and less on taxes than do non-initiative states.[27]

Term limits have been shown to influence fiscal policy at the state and federal levels. Besley and Case (1995) show that Democratic governors in the United States spend and tax more during years when they are faced with binding term limits, compared to years when they are eligible for re-election.[6] At the federal level, Johnson and Crain (2004) find that single-term limits lead to a steady increase in government size over time. In countries where terms are limited to two consecutive terms, fiscal policy is more volatile and government growth is smaller than growth in countries with single-term limits.[22]

The tax-smoothing literature is based on Barro (1979) who predicts that all current knowledge of future revenues is incorporated into government spending, and changes in taxes or expenditures are updated in every period according to new information.[5] Excess sensitivity to shocks represents a departure from tax-smoothing. This prediction is similar to the permanent income hypothesis of individuals.

Expanding on the work of Hall (1978)[19], Campbell and Mankiw (1990) present the λ-model in the context of individual consumption decisions. The model identifies the extent to which individuals deviate from the permanent income hypothesis in consumption.[10][6] Holtz-Eakin et al. (1994) adapts the Campbell and Mankiw model to government expenditure decisions. Using this model and aggregated NIPA data for US states and local governemnts, Holtz-Eakin (1994) finds that all changes in spending are determined by short-term fluctuations in revenues; state and local governments are

[5]See, for example, Wenzel et. al (1998) [36].

[6]Campbell and Mankiw estimate that individuals consume between 40 and 50 percent of their current income, which is a significant departure from the permanent income hypothesis. On the other hand, Hall (1978) concludes that individual consumption closely resembles the permanent income hypothesis.

excessively sensitive to unexpected changes in revenues. [15] As mentioned, this measure says little about the decision-making process associated with government tax-smoothing and is relatively uninformative as to why sub-national governments in the United States have short-sighted fiscal policies.

Since then, there have been few studies focused on US municipal fiscal policy. Wildasin (2002) uses time-series techniques in an attempt to identify the dynamics of municipal revenues and expenditures under balanced budget constraints. He concludes that grants play an important tax-smoothing role for larger municipalities and those with larger debt burdens.[9] He does not address the extent to which changes in revenues and expenditures are anticipated and planned, respectively.

Borge and Tovmo (2007) use data from Norwegian local governments to estimate the Campbell-Mankiw model for local governments. To test whether balanced budget constraints or other institutional factors limit forward-looking behavior, they split the sample in various ways. They consider party fragmentation and fiscal health as possible constraints to tax-smoothing budgeting behavior. They find that increased liquidity leads to more tax-smoothing behavior, and political fragmentation is associated with more short-sighted budgeting.[8]

Studies using dis-aggregated European data find that European local governments smooth taxes and expenditures relatively well.[7] In many cases, these studies fail to reject perfect tax-smoothing behavior, whereas Holtz-Eakin et al. (1994) does not reject fully short-sighted behavior. Of course, the difference in estimates between European countries and the US might be attributed to differing political and financial institutions. But, until now, there have been no studies using dis-aggregated data for the United States. Thus, as Dahlberg and Lindstrom (1998) point out, it is impossible to know whether the difference in estimates between the United States and European countries is a result of institutional differences, or simply an artifact of the use of aggregated versus dis-aggreated data.[12] To answer this question, I estimate a model of forward-looking government decision making, using a panel of 506 municipalities in the United States under various liquidity and political costraints.

In the United States, the effects of liquidity constraints and political institutions on local fiscal policy have not been rigorously tested. In the remainder of the paper, I provide a framework in which to test the magnitude and variation of the effects of

[7]See for example, Dahlberg and Lindstrom (1998), Borge and Tovmo (2007).[12, 8]

various constraints and institutions on local fiscal policy.

3 Empirical Strategy

This section presents an adaptation of Campbell-Mankiw's (1990) model of forward-looking consumption decisions. The model provides a convenient way to parameterize the extent to which fiscal decisions are short-sighted or consistent with tax-smoothing policy. Barro (1979) predicts optimizing governments will smooth taxes and expenditures across years by incorporating all knowledge of future revenues into current fiscal decisions. The extent to which decisions to change tax rates and expenditures are made by fully incorporating information about the future, or with excess sensitivity to temporal changes in revenues, is an empirical question. Deviations from tax-smoothing policy are potentially explained by political incentives to spend or change tax rates. Alternatively, political incentives might influence fiscal decisions away from reasonable tax-smoothing policies toward short-sighted decisions. The model outlined below identifies the extent to which governments act according to a tax-smoothing model or deviate by enacting short-sighted tax and expenditure policies. By estimating this model for similar cities facing different political institutions, I am able to identify whether or not these institutions affect fiscal decisions.

Municipal planners are assumed to maximize an time-separable inter-temporal utility function $U(G_t)$, subject to a balanced budget constraint. Planners choose the level of government spending in t. The utility function is general; it can be interpreted as the utility function of the median voter under the assumption that planners are responsive to the median voter. Alternatively, if the planner implements her preferred policy, the utility function can be thought of as the planner's utility over spending. Thus, the utility function is not constrained by any specific social welfare function, re-election concerns, or political agendas, for example.

$$\max_{G_t} E_t[\sum_{j=0}^{\infty}(1+\rho)^{-j}U(G_{t+j})]$$

s.t.

$$G_t \leq R_t + T_t - K_t + S_t(1+r_t)$$

R is current revenues, T is transfers from other governments, K is capital spending, and S represents beginning of period savings. G is current government spending minus

capital expenditures. Capital expenditures are included in the budget constraint and are assumed to be exogenous. Similar to the interest rate and transfers, in this model the timing and magnitude of capital projects are exogenous across years and do not directly enter the utility function. Intuitively, these expenditures affect current expenditures to the extent that capital project costs enter into the general budget.

In practice, capital projects are undertaken for several reasons. Capital expenditures would not be exogenous to current revenues if surpluses were invested as capital projectst during upswings, for example. For now, I categorize capital projects and budgets as exogenous to the current expenditure decision. Because of this, I include capital expenditures in the right hand side of the regression. I further address this assumption in the results section. Capital expenditures should also be included if it is the case projects are funded through a separate budget; it is plausible that capital expenditures influence the marginal utility of current spending, through some process not captured by the utility function.

The Euler equation resulting from the optimization problem characterizes the optimal spending path.[8]

$$E_t\left[\left(\frac{1+r_{t+1}}{1+\rho}\right)\frac{U'(G_{t+1})}{U'(G_t)}\right] = 1$$

The planner will equate the expected marginal utility of government spending across periods .

Following Hansen and Singleton [20], the Euler equation can be rewritten as,[9]

$$E_{t-1}[\Delta lnG_t] = \mu + \sigma E_{t-1}[r_t] \tag{1}$$

If governments smooth expenditures perfectly across years, then they act according to equation (1); changes in current spending are funded by permanent resources μ and the expectation of the interest rate. If, on the other hand, governments are completely short-sighted, then changes in current government spending are financed by concurrent

[8]The budget constraint can also be written as,

$$\sum_{t=0}^{\infty}\left[\prod_{s=0}^{t}\left(\frac{1}{1+r_{t-s}}\right)\right]G_t = S_t + \sum_{t=0}^{\infty}\left[\prod_{s=0}^{t}\left(\frac{1}{1+r_{t-s}}\right)\right](R_t + T_t - K_t)$$

[9]This assumes

$$U(G_t) = \frac{G_t^{1-\theta}}{1-\theta}$$

Also, $\sigma = \frac{1}{\theta}$.

changes in revenues as in equation (2). This is derived by forcing savings to be zero in each period. Intuitively, a planner would act short-sightedly if she did not have an intertemporal utility function and was only concerned with utility in the current period. This would occur if she were only concerned with re-election or was faced with term limits, for example.

$$E_{t-1}[\Delta lnG_t] = E_{t-1}[\Delta lnR_t] \tag{2}$$

In reality, governments have incentives to act according to a tax-smoothing model in addition to responding to changes in current revenues; actual fiscal policy is likely to fall somewhere between perfect tax-smoothing and completely short-sighted budgeting. The extent to which local governments act according to the tax-smoothing model and engage in short-sighted budgeting is identified by taking a weighted average of equations (1) and (2). In equation (3), an estimate of λ equal to 0 is consistent with perfect tax and expenditure smoothing; λ equal to 1 is consistent with fully short-sighted government spending behavior.

$$E_{t-1}[\Delta lnG_t] = (1-\lambda)\mu + \lambda E_{t-1}[\Delta lnR_t] + (1-\lambda)E_{t-1}[r_t] \tag{3}$$

As discussed above, capital spending, the real interest rate, and transfers from state and federal governments are important exogenous influences on the general budget. Thus, the main empirical specification includes these variables. Removing the expectations, the empirical specification is presented in equation (4). In the data, only the actual changes in revenues and expenditures are available, not the expectations. The error term consists of updated beliefs about future permanent resources, as well as a transitory shock that affects revenues. Updated expectations are correlated with changes in current revenues, as the shock contains information about the future. The next section addresses this correlation.

$$\Delta lnG_t = (1-\lambda)\mu + \lambda\Delta lnR_t + \theta_1\Delta lnK_t + \theta_2\Delta lnT_t + \theta_3 r_t + (1-\lambda)\epsilon_t \tag{4}$$

Estimates of λ are presented in section 5. Government spending, current revenues, capital expenditures, and transfers are measured per capita. General spending is defined as general expenditures minus capital expenditures, per capita. Current revenues is defined as general revenues minus intergovernmental transfers, per capita. I also include

the real interest rate on the right hand side of the regression. [10]

3.1 Endogenous current resources

In the model, ϵ_t represents an innovation in the assessment of permanent income from $t-1$ to t. These innovations are not orthogonal to changes in current revenues, which are likely to contain information about future permanent resources. Thus, OLS estimates of λ will be inconsistent. The common correction for endogenous current resources is to use instrumental variables. Importantly, ϵ_t is orthogonal to any variable which is in the government's information set at $t-1$. Following Campbell and Mankiw (1990), I employ lagged values of the right hand side variables as instruments for changes in current revenues in time t. Lagged changes in revenues are orthogonal to innovations of permanent resources from $t-1$ to t and predict revenues in time t. Similarly, lagged values of transfers, capital expenditures, and the interest rate also predict changes in revenues and are included as instruments in the estimation.

Using lagged variables as instruments for changes in current resources might be problematic if there exists serial correlation in forecasts of permanent resources. This might happen if fiscal adjustment is slow and the effect of a given shock is distributed across subsequent years. To address this concern, I use lags of three to six years as instruments. Under the assumption that the instruments are uncorrelated with the error term, the two-step generalized method of moments (GMM) estimator is the appropriate GMM estimator. The GMM estimator is used to address the fact that I am using four lagged values of each of five variables to instrument for the change in current revenues in time t. The Hansen J statistic is used to test the hypothesis that the instruments are exogenous. The null hypothesis of the Hansen J statistic is that the over identifying restrictions are valid. A failure to reject this hypothesis supports the intuition that the instruments are exogenous. Upon inspection of various lag specifications, I find that lags of three to six years most consistently satisfy this assumption.

For a discussion and presentation of an OLS and IV specifications, please see the appendix.

[10] Real interest rate computed as $\frac{1+nominal_t}{1+\frac{GDPdeflator_t}{GDPdeflator_{t-1}}} - 1$.

4 Data

To estimate λ, the extent to which municipal governments are short-sighted in their fiscal policies, I use panel data for 506 municipalities in the United States. Fiscal data are available from the Annual Survey of Government Finances, conducted by the Census Bureau.

I use the 1996 International City/County Management Association (ICMA) Form of Government survey to identify the cities in the Annual Survey of Government Finances panel have the initiative or recall option as well as whether term limits were in place in 1996. The ICMA Form of Government survey is circulated to local governments every five years beginning in 1981. Surveys are sent to about half of all cities in the country. The survey includes about forty questions, including whether the initiative or recall are available to residents at the local level. As with all survey data, the ICMA survey is subject to some shortcomings. Nonetheless, the ICMA survey is the most robust source of data on local legislative laws. Across years, there is variation in the response rate and wording of questions, so it is difficult to compare summary statistics across years.[28]

Of the 506 cities in the Survey of Governments sample, 323 responded to the ICMA survey. To the extent that these cities responded accurately, the shortcomings of the ICMA survey should not affect the identification of initiative, recall, and term limits for these cities. Figures 1, 2, and 3 summarize the survey response rates and share of cities with these institutions, by population. Figure 4 presents the distribution of years that term limits were enacted.[11] Cities that have access to state level initiative and recall are more likely to have the local initiative and recall. Similarly, cities that have term limits are likely to have both the local initiative and recall. Figure 5 displays the relative frequencies of these institutions for cities in states with and without the state-level initiative.

Demographic data are from the City and County Data Books, published every five years. Of the 506 cities in the Annual Survey sample, 435 appear in the City and County Data Books. The data are summarized in Table 1.

[11] Two-thirds of term limit cities in this sample limit the Mayor to two terms in office, 24 percent allow three terms, and the remaining cities allow for four terms in office.

Figure 1: **Distribution of cities with local initiative by population size**

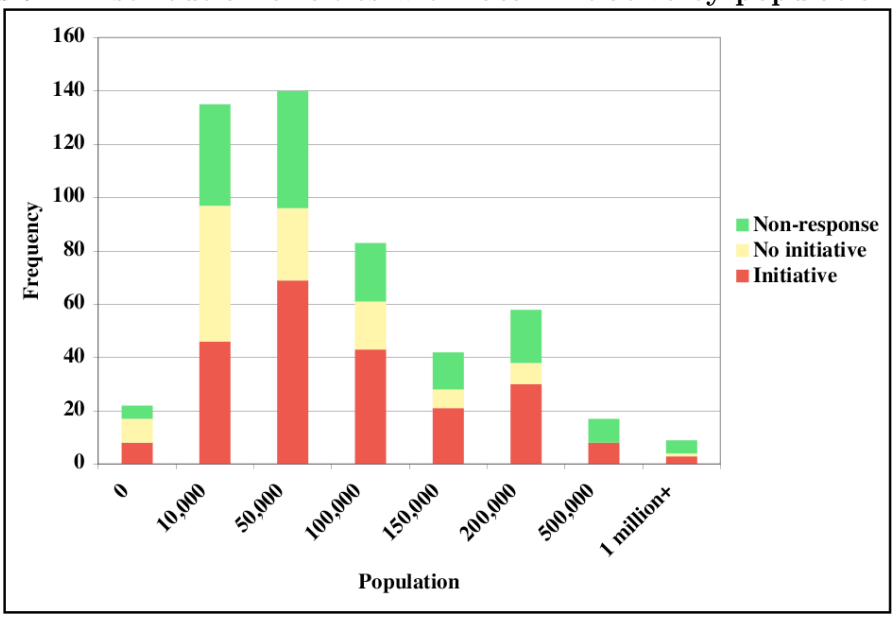


Figure 2: **Distribution of cities with local recall by population size**

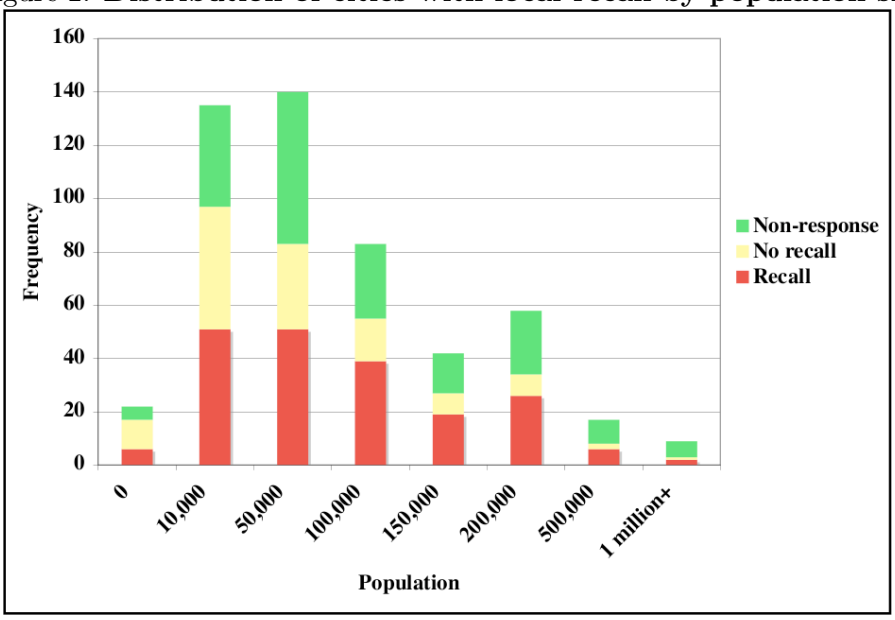


Figure 3: **Distribution of cities with mayoral term limits by population size**

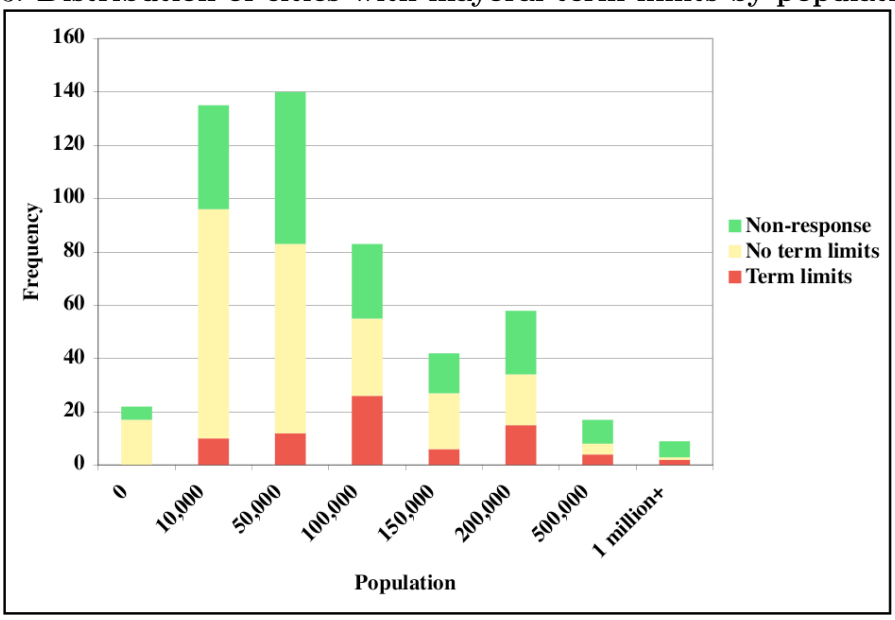


Figure 4: **Number of cities in ICMA survey enacting term limits, by year**

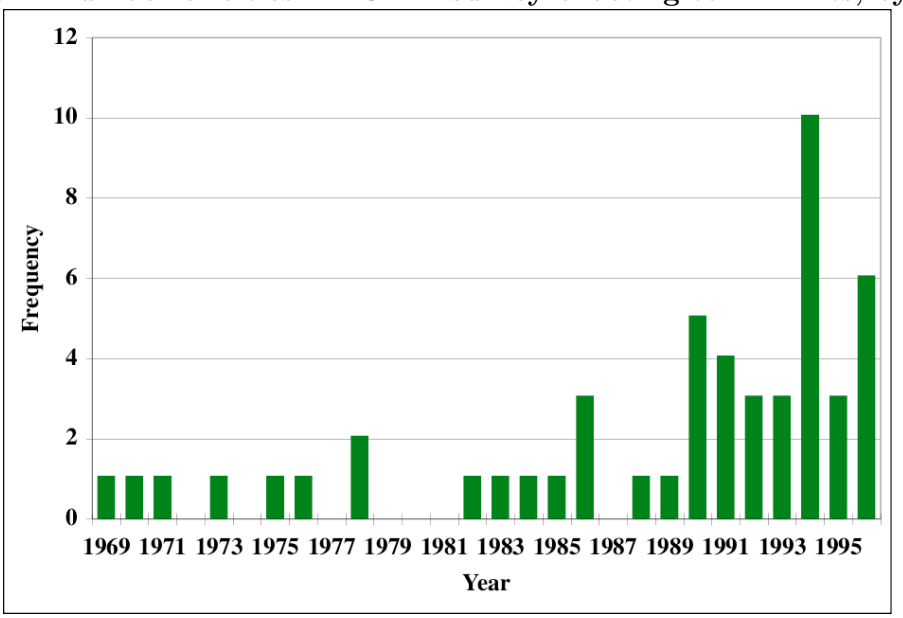


Figure 5: **Shares of cities with initiative, recall, and/or term limits**

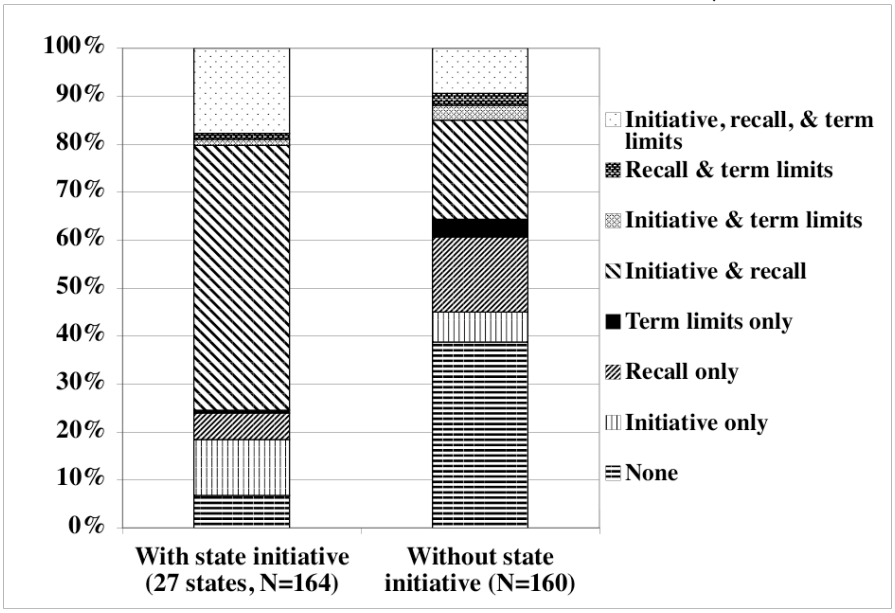


Table 1: **Summary Statistics, year 2000 cross-section**

	All cities in annual Census	**All cities in ICMA & Census**	**Initiative**	**Initiative ex. CA**	**No initiative**
Revenue ($)	2,111	1,957	1,885	1,978	2,094
Transfers from State ($)	324	257	237	234	295
Direct Expenditures ($)	2,031	1,876	1,810	1,922	2,000
Capital Expenditures ($)	342	324	324	342	324
Outstanding debt ($)	2,001	1,989	1,931	2,002	2,100
Population	167,796	140,723	161,396	131,284	101,769
% White	68	69	68	76	70
% Black	16	13	10	12	20
% Hispanic	16	17	20	13	8
% Over age 65	12	12	12	13	13
% Owner Occupied	56	58	57	58	60
Median HHd Income ($)	40,895	42,488	42,948	38,726	41,336
Political Score	8	4.4	3.8	0.4	5.7
# Cities	506	349	228	160	121
	Recall	**Recall ex. CA**	**No Recall**	**Term limits (as of 1996)**	**No term limits (as of 1996)**
Revenue ($)	1,967	2,010	2,024	2,064	1,972
Transfers from State ($)	233	248	320	209	282
Direct Expenditures ($)	1,878	1,934	1,961	1,928	1,906
Capital Expenditures ($)	333	340	324	341	326
Outstanding debt ($)	1,875	1,884	2,226	2,374	1,920
Population	153,162	126,086	110,266	255,221	107,900
% White	71	75	71	68	72
% Black	11	13	17	12	13
% Hispanic	17	13	9	19	13
% Over age 65	12	13	13	12	13
% Owner Occupied	58	59	59	57	58
Median HHd Income ($)	42,190	38,565	41,937	43,072	41,751
Political Score	0.5	-0.6	7.6	-1.4	4.34
# Cities	232	164	117	64	259
Dollar amounts are per capita					

I argue that the presence of the local initiative and recall is exogenous to present city characteristics. The vast majority of cities that adopted direct democracy institutions did so in the early 1900s, around the same time that it was adopted by most of the 24 states that have adopted the initiative. [29] As illustrated in figure 5, cities with access to the state level initiative are more likely to also have local access to both the initiative and recall than cities without access to the state level initiative. Term limits are most likely to be passed in cities with both the recall and the initiative. About 20 percent of the cities in both initiative and non-initiative states have term limits.

I compare cities with and without the initiative and recall processes, thereby estimating the average effect of these institutions. It should be noted that, in practice, the accessibility of the initiative process varies across cities. In New York City, for example, signature gatherers have 120 days to collect 50,000 signatures. In Los Angeles, signature gatherers have 200 days to collect as many signatures as 15 percent of votes cast in the previous Mayoral election. For cities that require a certain percentage of registered voters, this number typically ranges from 5 percent to 15 percent. Time requirements to gather signatures range from 60 days to an unlimited number of days.[28] Due to these differences, the threat of initiative is stronger in some cities than in others.[12]

The summary statistics presented in table 1 show that cities with and without the initiative and recall have similar fiscal characteristics, except non-initiative and non-recall cities have more outstanding debt, on average. Cities with recall and initiative have slightly lower incomes and larger population and are more Republican.[13]

Term limits, however, have been largely implemented since 1970 with a bulk of cities passing term limit laws in the 1990s. It is possible that the implementation of term limits is endogenous to fiscal policy; cities with worse fiscal policy are more likely to adopt term limits as a disciplining mechanism for officials. Or simply, cities that adopt term limits are different from cities that do not adopt term limits. This endogeneity is a problem only if the factors that led to the implementation of term limits is associated with the financing decisions of current revenues. It is unlikely that term limits were enacted in response to fiscal choices; the main arguments presented in favor of term limits center around the fund-raising and public awareness advantages held by incumbents. Many of the arguments for term limits are emotional appeals to limit the powers of government

[12]Further research will address the credibility of the initiative threat in more detail.

[13]The political score is a measure derived in Donovan (2009b)[?]. It is discussed in more detail in the appendix.

similar to "We want a government of the people, by the people, and for the people, not a tyrannical ruling class who care more about deals to benefit themselves, than their constituents."[14]

According to the summary statistics, cities with term limits have larger populations and a larger share of Republicans than cities without term limits. They also have more outstanding debt, but appear similar in other fiscal and demographic characteristics. Comparing fiscal policy of cities with and without term limits is likely to give a biased estimate of the effect of term limits on fiscal policy. Thus, to identify the effect of term limits on fiscal decisions, I estimate equation (4) for cities that adopt term limits for the five year time periods before and after the adoption of term limits.[15]

5 Results

The presence of initiative does not guarantee its utilization, of course. But the threat of initiative provides incentives to city officials to implement policies preferred by the median voter. Thus, by simply having the initiative option, voters potentially have a larger influence over local government. [16] Unfortunately, I do not have data on the number of voter-initiated ballot measures at the municipal level, so I can only test the hypothesis that the threat of initiative has an effect on fiscal policy.

Similarly, the threat of recall is a potentially significant factor in local fiscal decisions. For example, voters might prefer short-sighted policy decisions like tax cuts during good economic times. Responding to the threat of removal from office, local officials have greater incentives to implement voter-preferred policy. Ex ante, it is unclear whether voter policy preferences are more short-sighted or forward-looking than policies implemented by officials, in the absence of the threat of removal from office.

The threat of local recall is significant in many states. In 2009, there have been efforts made to recall mayors in Portland Oregon, Toledo Ohio, Mansfield Ohio, Point Pleasant Beach New Jersey, Soledad California, and many other cities across the US. In California, local recall data are available from the California Elections Data Archive.

[14] From www.termlimits.org, a major lobbying group for term limits.

[15] An additional reason for this choice of specification is that the ICMA survey provides term limit status as of 1996. Since then, several more cities have adopted term limits. Thus, an estimate of λ for cities without term limits will include information from cities inaccurately categorized as not having term limits if they enacted limits after 1996. I am currently exploring ways to update the ICMA data.

Between 1995 and 2004, there were 73 municipal recall votes in 30 different cities. On average, there are eight recall ballot measures per year in three different cities. City council recalls represent a large share of these recalls. Once a recall measure reaches the ballot in California, it is passed 74 percent of the time.

Table 2 presents the estimation of equation (4) for all cities responding to the ICMA survey, and for cities with and without the local initiative and recall. California cities are excluded from these estimates because all cities in California have access to the local initiative and recall.[16] Table 2 also presents the results for cities that enacted term limits, for the five year periods prior to and after the implementation of term limits. The p-values for the Anderson and Hansen J test statistics are reported. Newey-West standard errors, robust to arbitrary serial correlation, are reported.

The estimate of λ for all cities is .643 (column (1)), which is inconsistent with Holtz-Eakin et al.'s estimate of $\lambda = 1$ (although Holtz-Eakin includes state governments, in addition to local governments), and is larger than most estimates for European cities.[17] On average, about 64% of changes in current expenditures are financed with changes in current revenues. There is no statistical difference in forward-looking behavior for cities with and without access to the local initiative; estimates of λ are similar in magnitude.

Note that I am only identifying the availability of the initiative and not voter utilization of the initiative. It might still be the case that the policies enacted by voters are associated with short-sighted or tax-smoothing fiscal policy, relative to policies enacted by government officials. In California at least, most voter initiatives at the local level are concerned with land use and planning and not tax and finance issues.[17] At the state level, tax and expenditure items represent a larger share of voter-initiated ballot items. Thus, it is perhaps no surprise that the threat local initiative has little effect on fiscal policy; these results should not be generalized to the state level.

The estimate for cities with recall are larger than that for those without the recall, but the difference is not statistically significant. On average, the threat of recall does not significantly affect municipal fiscal decisions.

Term limits do significantly influence local fiscal policy, though. Before cities implement term limits, 32 percent of current expenditures are financed with current revenues (column (6)). With 68 percent of current expenditures being financed through perma-

[16] California cities were granted this right in 1911 when the state adopted the initiative and recall under Governor Hiram Johnson.[17]

[17] Borge and Tovmo (2007) estimate $\lambda = .35$ for Norwegian cities, for example.

Table 2: **GMM estimation of equation (4) by access to initiative, recall, and implementation of term limits.**

	All	Initiative[1]	No Initiative	Recall[1]	No Recall	Pre Term Limits	Post Term Limits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔlnR_t, λ	0.643	0.452	0.370	0.369	0.270	0.324	0.663
	(0.116)**	(0.101)**	(0.153)*	(0.104)**	(0.133)*	(0.126)*	(0.109)**
ΔlnK_t	-0.015	-0.010	-0.007	-0.006	-0.006	0.001	-0.017
	(0.004)**	(0.004)*	(0.004)	(0.005)	(0.004)	(0.007)	(0.010)
ΔlnT_t	0.057	0.053	0.056	0.062	0.051	0.076	0.034
	(0.009)**	(0.014)**	(0.011)**	(0.015)**	(0.010)**	(0.023)**	(0.018)
r	0.001	0.001	0.004	0.003	0.005	0.003	0.010
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)*	(0.010)	(0.007)
Constant	0.000	0.003	0.001	0.000	0.001	0.002	-0.022
	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)	(0.028)	(0.017)
Observations	9511	4329	3278	4435	3172	213	272
Anderson (p-value)	.000	.000	.058	.000	.014	.064	.001
Hansen (p-value)	.004	.003	.478	.126	.637	.238	.401

Newey-West standard errors in parentheses

* significant at 5%; ** significant at 1%

1. Excluding California cities.

Three through six year lags of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, and the real interest rate, are included as instruments for ΔlnR_t.

nent resources, these cities are largely making fiscal decisions consistent with a tax-smoothing model. After term limits are implemented, however, fiscal decisions become much more short-sighted with only 34 percent of current expenditures being financed by permanent resources (column (7)). A common argument for term limits is that they prevent entrenchment of city officials who, over time, have more flexibility to implement their desired policies. However, term limits present officials with incentives to implement their desired policies when they are certain to not be in office in the next period. Indeed, the marked change toward short-sighted fiscal decision-making is consistent with the latter argument; term limits lead to less disciplined policy decisions.

5.1 Specification robustness checks

The most basic specification does not require the inclusion of government transfers, capital expenditures, and the real interest rate. However, these are included in the

main specification because it is likely that they affect the expenditure decision. In each estimation, the coefficients on the additional right hand side variables are stable. For completeness, the estimation of equation (4) excluding the additional control variables is presented in Table 3. In columns (3), (5), and (6), changes in current revenues lose explanatory power without the control variables. Further the additional explanatory variables are important as instruments; in columns (5) and (6), the Anderson test for instrument relevance fails to reject the hypothesis that the instruments are not identified.

Table 3: **GMM estimation of equation (4), basic specification**

	All	Initiative[1]	No Initiative	Recall[1]	No Recall	Pre Term Limits	Post Term Limits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔlnR_t, λ	0.529	0.598	0.359	0.389	0.444	0.208	0.692
	(0.117)**	(0.138)**	(0.239)	(0.126)**	(0.338)	(0.190)	(0.162)**
Constant	0.006	0.004	0.011	0.008	0.011	0.006	0.002
	(0.002)*	(0.003)	(0.005)*	(0.003)**	(0.006)	(0.007)	(0.004)
Observations	9769	4477	3388	4589	3276	217	281
Anderson (p-value)	.000	.002	.09	.000	.506	.233	.026
Hansen (p-value)	.000	.001	.822	.005	.574	.175	.295

Newey-West standard errors in parentheses

* significant at 5%; ** significant at 1%

1. Excluding California cities.

Three through six year lags of ΔlnR_t and ΔlnG_t are included as instruments for ΔlnR_t.

Term limits are the only local electoral institution for which I find an effect on fiscal policy in the main specification. In Table 2, I estimate λ for the time periods of five years before and after term limits are enacted. To provide evidence that the result is not spurious to the time periods specified, I estimate λ using different time periods. The sample is limited to cities that enacted term limits after 1990 to ensure that the full set of observations is available for the cities in the estimate.[18] Columns (1) and (2) of Table 4 present the results using a window of three years before and after the enactment of term limits, respectively. During the three years prior to the enactment, cities finance 23% of current expenditures with current revenues, compared to 73% afterward. Using a seven year window (columns (3)-(4)), λ rises to .717 from .25 before term limits. With a ten year window, the difference before and after term limits becomes smaller and is

[18]The hypothesis that the instruments used in regressions including cities that passed term limits between 1987 and 1990 were not identified, could not be rejected.

marginally statistically significant, but the point estimate still increases after term limits are enacted. Point estimates for before and after the enactment of term limits do not significantly change when the estimation window is expanded.

Table 4: **GMM estimation of equation (4). 3,7, and 10-year time windows surrounding enactment of term limits.**

	3 year window		7 year window		10 year window	
	Before	After	Before	After	Before	After
	(1)	(2)	(3)	(4)	(5)	(6)
ΔlnR_t, λ	0.229	0.733	0.250	0.717	0.441	0.660
	(0.102)*	(0.116)**	(0.086)**	(0.137)**	(0.068)**	(0.147)**
ΔlnK_t	0.002	-0.014	0.007	-0.024	-0.001	-0.017
	(0.009)	(0.014)	(0.007)	(0.011)*	(0.007)	(0.010)
ΔlnT_t	0.170	0.017	0.086	0.027	0.084	0.030
	(0.028)**	(0.024)	(0.021)**	(0.018)	(0.024)**	(0.015)
r	0.027	0.016	0.005	-0.028	0.005	-0.040
	(0.019)	(0.027)	(0.016)	(0.024)	(0.009)	(0.015)**
Constant	-0.063	-0.043	-0.007	0.061	-0.002	0.092
	(0.054)	(0.065)	(0.048)	(0.057)	(0.027)	(0.033)**
Observations	85	109	199	221	286	292
Anderson (p-value)	.043	.013	.079	.069	.001	.129
Hansen (p-value)	.488	.444	.246	.554	.212	.342

Newey-West standard errors in parentheses
* significant at 5%; ** significant at 1%

Three through six year lags of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, and the real interest rate, are included as instruments for ΔlnR_t. Only cities enacting term limits after 1990 included.

Column (1) of table 5 presents the main specification with a time trend included. Years, numbered 0 to 35 are, interacted with changes in current revenues. If short-sightedness increased over time then the coefficient on this term would be greater than zero; empirically, it is not statistically different from zero. Note that this result is robust to including various functions of the year variable. This is evidence that the decision to enact term limits is not endogenous to temporal fiscal choices.

Table 5: **GMM estimation of equation (4) with time trend, liquidity constraints, and property tax shares of revenues.**

	Time trend	Predicted debt	Property tax share of revenues
	(1)	(2)	(3)
$\Delta lnR_t, \lambda$	0.622	0.823	0.208
	(0.085)**	(0.094)**	(0.161)
ΔlnK_t	-0.015	-0.016	-0.018
	(0.003)**	(0.003)**	(0.003)**
ΔlnT_t	0.059	0.054	0.038
	(0.008)**	(0.007)**	(0.012)**
r	0.001	-0.001	-0.000
	(0.001)	(0.002)	(0.002)
$\Delta lnR_t * Year$	0.007		
	(0.004)		
$\Delta lnR_t * \hat{Debt}$		-0.045	
		(0.009)**	
$\Delta lnR_t * \hat{Debt}^2$		0.000	
		(0.000)**	
$\Delta lnR_t * property\_tax\_share$			0.053
			(0.017)**
$\Delta lnR_t * property\_tax\_share^2$			-0.001
			(0.000)**
Constant	0.001	0.007	0.003
	(0.004)	(0.004)*	(0.004)
Anderson (p-value)	.000	.000	.000
Hansen (p-value)	.033	.020	.049
Observations	13729	13242	13729

Newey-West standard errors in parentheses
* significant at 5%; ** significant at 1%

Three through six year lags of ΔlnG_t and all right hand side variables are included as instruments for ΔlnR_t.

5.2 Liquidity Constraints

Liquidity constraints potentially limit the ability of governments to smooth taxes and expenditures during economic downturns. In the United States, the vast majority of state and sub-national governments are subject to a balanced budget constraint.[19] The strin-

[19]All state governments except West Virginia are subject to a statutory or constitutional balanced budget law. Nearly all municipal governments are required by law to balance budgets, either by local

gency of balanced budget constraints varies across states, but typically prohibits running a deficit on the general operating budget and allows for some borrowing. Lastly, there are virtually no formal provisions for enforcing balanced budget laws. Although balanced budget laws are effective in limiting deficits.[20], governments have some flexibility in circumventing these restrictions through debt, creative accounting, or in some states, deficit rollover. When revenues fall short of expenditures, governments must borrow or utilize savings to keep taxes and expenditures constant.[21] Access to credit markets and the reliability of intergovernmental transfers influence a government's ability to issue debt or borrow from other governments. But these sources of liquidity do not exist uniformly across governments.

Cities with higher per capita revenues, for example, are better able to service debt obligations and will have access to larger lines of credit. Similarly, state transfers to local governments are often important sources of revenues. Some of these transfers are more reliable than others, however, as certain localities have more permanent fiscal ties to state governments as a result of political or economic relationships.

Cities without the recall option have more outstanding debt, on average, than cities with the recall option. Similarly cities with term limits have more outstanding debt than do those without term limits. These differences might reflect disparities in access to credit, or they might be a result of short-sighted budgeting behavior or overspending. According to the tax-smoothing model, debt is utilized to smooth taxes and expenditures across years. When faced with political incentives to spend on short-term projects, however, it is likely that some officials use debt to fund current projects in the absence of revenue shortfall. If debt is utilized to smooth expenditures, cities with more debt would be expected to have a smaller estimate of λ. In the case with term limits, however, the exact opposite is observed. This suggests that debt is used to finance short-sighted projects.

Nonetheless, access to credit markets is likely an important determinant of a city's ability to smooth taxes and expenditures across years. Indeed, Borge and Tovmo (2007) find that municipalities with smaller liquidity constraints act according to the tax-smoothing hypothesis. To ensure that estimates of λ are not driven by disparities in

charter or state law.[25]

[20]See, for example, Poterba (1994) [34]

[21]Of course, they can also engage in so-called "creative accounting." Unfortunately, it is difficult to assess the extent to which this occurs in practice.

access to credit, I estimate λ for cities with the greatest access to debt. I estimate access to debt by regressing per capita levels of outstanding debt on population, per capita revenues, the percentage of revenues from local property taxes, and quadratics in each of these terms. State fixed effects and the real interest rate are also included in the regression. For each city, access to debt is the outcome of linear projection of these variables onto outstanding debt. The coefficients of this estimation are reported in Table 6.

Table 7 presents the estimation of equation (4) for only the least constrained cities.[22] The estimate of λ for all of the least constrained cities is smaller than the estimate for the full sample, suggesting that access to liquidity does enable tax-smoothing. The coefficients in columns (2)-(7), however, are remarkably consistent with the full sample specification; the effects of initiative, recall, and term limits on fiscal policy do not vary for cities with significant access to liquidity.

Table 8 presents the same estimation for cities with the least access to liquidity. [23] For these cities, the effect of term limits is no longer present. For cities with no liquidity, fiscal policy is short-sighted in the absence of term limits. When they are enacted, there is little flexibility to implement additional short-sighted policies.

Column (2) of table 5 provides more detail as to how access liquidity influences fiscal decisions. Predicted levels of debt and quadratics of predicted debt are interacted with changes in current revenues. Access to an additional $1,000 per capita of debt leads to a reduction of about 5% in the percent of changes in current spending that are financed with current expenditures.

[22]The least constrained cities are defined as those with predicted levels of per capita debt greater than $1,600, the mean predicted value.

[23]The most constrained cities are defined as those with predicted levels of per capita debt less than $1,000.

Table 6: **OLS prediction of access to debt**

	Per capita outstanding debt
Population	-0.000
	(0.02)
Population^2	-0.000
	(2.51)*
Per capita revenue	1.726
	(97.18)**
(Per capita revenue)^2	-0.007
	(48.59)**
% From property tax	7.084
	(13.80)**
(% From property tax)^2	-7.068
	(9.00)**
r	0.083
	(3.66)**
Constant	-3.493
	(7.67)**
Observations	17204
R^2	0.62
Absolute value of t statistics in parentheses	
* significant at 5%; ** significant at 1%	

Includes state fixed effects.

Table 7: **GMM estimation of equation (4) by access to initiative, recall, and implementation of term limits. Cities with significant access to credit.**

	All	Initiative[1]	No Initiative	Recall[1]	No Recall	Pre Term Limits	Post Term Limits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔlnR_t, λ	0.266	0.404	0.321	0.349	0.248	0.340	0.651
	(0.092)**	(0.095)**	(0.136)*	(0.098)**	(0.126)*	(0.098)**	(0.130)**
ΔlnK_t	-0.000	-0.007	-0.004	-0.010	0.002	-0.011	-0.013
	(0.004)	(0.005)	(0.004)	(0.006)	(0.004)	(0.008)	(0.010)
ΔlnT_t	0.066	0.054	0.042	0.061	0.029	0.046	0.046
	(0.015)**	(0.020)**	(0.013)**	(0.022)**	(0.012)*	(0.021)*	(0.020)*
r	0.004	0.003	0.004	0.004	0.004	0.009	0.016
	(0.002)*	(0.003)	(0.003)	(0.003)	(0.003)	(0.009)	(0.007)*
Constant	0.001	-0.002	0.002	-0.002	0.005	-0.014	-0.037
	(0.005)	(0.007)	(0.008)	(0.006)	(0.008)	(0.026)	(0.019)
Observations	7082	2272	1745	2422	1595	141	175
Anderson (p-value)	.000	.000	.018	.000	.006	.319	.012
Hansen (p-value)	.005	.005	.427	.030	.301	.738	.591

Newey-West standard errors in parentheses

* significant at 5%; ** significant at 1%

1. Excluding California cities.

Three through six year lags of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, and the real interest rate, are included as instruments for ΔlnR_t.

Only cities with predicted per capita debt levels greater than $1,600 are included in this specification.

Table 8: **GMM estimation of equation (4) by access to initiative, recall, and implementation of term limits. Cities with least access to credit.**

	All	Initiative[1]	No Initiative	Recall[1]	No Recall	Pre Term Limits	Post Term Limits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔlnR_t, λ	0.637	0.383	0.250	0.398	0.107	0.674	0.769
	(0.114)**	(0.131)**	(0.158)	(0.114)**	(0.129)	(0.061)**	(0.095)**
ΔlnK_t	-0.015	-0.010	-0.008	-0.006	-0.013	0.027	-0.001
	(0.004)**	(0.006)	(0.006)	(0.007)	(0.005)*	(0.019)	(0.015)
ΔlnT_t	0.084	0.085	0.056	0.075	0.087	0.135	0.011
	(0.013)**	(0.021)**	(0.018)**	(0.020)**	(0.018)**	(0.049)**	(0.020)
r	0.003	0.002	0.002	0.003	0.004	-0.002	0.014
	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)	(0.010)	(0.009)
Constant	-0.001	0.002	0.006	-0.003	0.001	0.005	-0.033
	(0.007)	(0.009)	(0.011)	(0.010)	(0.012)	(0.030)	(0.024)
Observations	3858	1108	857	1111	854	43	137
Anderson (p-value)	.000	.035	.586	.013	.253	.035	.005
Hansen (p-value)	.008	.178	.442	.232	.298	.758	.594

Newey-West standard errors in parentheses

* significant at 5%; ** significant at 1%

1. Excluding California cities.

Three through six year lags of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, and the real interest rate, are included as instruments for ΔlnR_t.

Only cities with predicted per capita debt levels less than $1,000 are included in this specification.

5.3 Capital spending

In the empirical setup, capital expenditures were assumed to be exogenous. In reality, though, cities have some flexibility in the timing of capital expenditures. Certain types of capital projects can be delayed in the case of revenue shortfall, and one-time capital expenditures occur when revenues exceed expectations. The extent to which this substitution occurs depends on the revenue-elasticity of the tax base. In particular, cities that rely more on income and sales tax see more variability in revenues as these tax bases are sensitive to macroeconomic cycles. Cities that rely on property taxes, on the other hand, have more stable revenues and revenues do not often exceed expectations. The more revenue-elastic the tax system, the more variable capital spending is expected to be, as capital projects are more easily postponed to economic boom times. Pagano (2002) provides evidence that cities with revenue-elastic taxes are more likely to substitute funds in and out of capital projects in response to fiscal fluctuations.[32] For cities with revenue-elastic taxes, the timing of capital expenditures is endogenous to revenue fluctuations. By utilizing capital budgets as a method of saving across periods, current expenditures in these cities are expected to be less sensitive to revenue fluctuations than in cities with inelastic taxes.

The property tax share of revenues and its quadratic are interacted with ΔlnR_t and included in the main specification; these results are presented in column (3) of table 5. A city that receives 30 percent of its revenues from the property tax finances 90 percent of current expenditures with changes in current revenues,[24] compared to 21 percent for a city without property tax that relies on sales and income taxes.[25] Cities with revenue-elastic systems smooth current expenditures across years by adjusting the timing of capital projects. However, cities that rely on relatively inelastic taxes display excess sensitivity to current revenues in current spending decisions.

5.4 Voter characteristics

Cities with a greater share of Democrats tend to be larger and more diverse; the median voter in these cities is likely to have different policy preferences from the median voter in a city with a greater share of Republicans. Since the initiative and recall serve

[24] $.208 + 30 * .053 + 30^2 * (-.001) = .898$

[25] Property taxes comprise 17 percent of revenues for the median city; a 30 percent share represents the 80th percentile of property tax shares.

to align constituent and politician preferences, it might be the case that the effect of initiative, recall, or term limits varies by the political orientation of a city. For example, either Democratic or Republican cities might be more responsive to the median voter in the absence of the initiative or recall. Republican cities are likely to be more unified because the are typically more homogenous; thus Democratic cities are more likely to see changes in policy as a result of aligned voter and politician interests. Ex ante, it is unclear whether party preferences would systematically affect the impact of initiative, recall, and term limits.

On average, it is likely that there is a difference in tax-smoothing behavior between Democratic and Republican cities. Homogenous populations tend to agree on policy more than diverse populations; the average distance from the median voter preferred policy is smaller. Because of this, it is easier for politicians to please constituents. However, when populations are diverse, the average utility derived from any public good is smaller. This happens in part because people of one group have diminished utility when they perceive that other groups derive utility from the public good.[2] With lower values of public goods, residents in diverse cities are likely to prefer short-sighted policies and be less concerned with future fiscal health of the city.

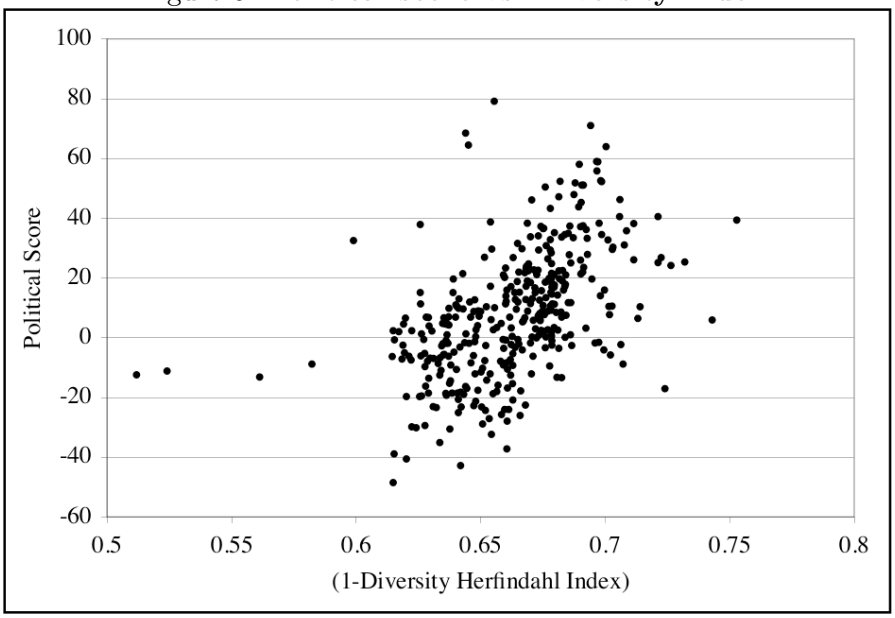
To identify the political party preferences of cities, I utilize the municipal political score derived in Donovan (2009b). This measure utilizes vote returns for presidential, senatorial, and gubernatorial races to predict the underlying preferences of citizens. It is constructed using demographic data and historical vote returns and is highly correlated with voter registration.[26] The political score can be interpreted as the share of Democrats in a city minus the share of Republicans.[27] So a score of 0 means the shares of Democrats and Republicans in a city are equal. See the appendix for more detail on how the political score is constructed. Figure 6 plots the political score and a measure of diversity.[28]

[26]Indeed, voter registration would be the ideal measure of political preferences. Unfortunately, this is not widely available at the municipal level.

[27]Donovan (2009b) [14] is available upon request from the author.

[28]The measure of diversity used is a $1 - H$, where H is a Herfindahl index of ethnic groups.

Figure 6: **Political score vs. Diversity Index**



The estimates for Republican and Democratic cities are reported in table 9 and table 10, respectively. On average, Democratic cities have more short-sighted fiscal policies than do Republican cities ($\lambda = .769$ compared to $\lambda = .459$). The estimates of λ for cities with the initiative are larger than for cities without initiative; this difference is slightly larger for Democratic cities, although the difference is not statistically significant.

Table 9: **GMM estimation of equation (4) by access to initiative, recall, and implementation of term limits. Cities with Republican majority.**

	All[1]	Initiative[1]	No Initiative	Recall[1]	No Recall	Pre Term Limits	Post Term Limits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔlnR_t, λ	0.459	0.441	0.386	0.394	0.182	0.336	0.692
	(0.095)**	(0.076)**	(0.104)**	(0.066)**	(0.100)	(0.169)*	(0.160)**
ΔlnK_t	-0.010	-0.004	-0.016	-0.009	0.000	0.004	0.004
	(0.005)	(0.006)	(0.006)*	(0.005)	(0.009)	(0.030)	(0.016)
ΔlnT_t	0.074	0.066	0.114	0.075	0.090	0.141	0.040
	(0.009)**	(0.012)**	(0.024)**	(0.013)**	(0.023)**	(0.057)*	(0.040)
r	0.002	0.004	-0.000	0.002	0.003	0.002	0.035
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.029)	(0.049)
Constant	0.004	-0.003	0.012	0.006	0.003	0.003	-0.076
	(0.007)	(0.009)	(0.011)	(0.009)	(0.013)	(0.083)	(0.115)
Observations	2064	959	600	1002	557	43	74
Anderson (p-value)	.000	.000	.191	.000	.036	.185	.008
Hansen (p-value)	.159	.128	.808	.214	.818	.515	.815

Newey-West standard errors in parentheses
* significant at 5%; ** significant at 1%

1. Excluding California cities.
Three through six year lags of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, and the real interest rate, are included as instruments for ΔlnR_t.
Only cities with a political score less than -5 are considered in this specification.

Estimates of λ for cities with the recall option are higher than for those without the option and even more so for Democratic cities. This suggests that the recall option leads to more short-sighted policies in Democratic and Republican cities. In these politically unified cities, the recall is a threat because voters have similar political preferences and thus a majority vote in favor of recall is more likely than in divided cities. When only the excluded (politically divided) cities are considered, there is no effect of the recall (not reported).

Table 10: **GMM estimation of equation (4) by access to initiative, recall, and implementation of term limits. Cities with Democratic majority.**

	All [1]	Initiative[1]	No Initiative	Recall[1]	No Recall[1]	Pre Term Limits	Post Term Limits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔlnR_t, λ	0.769	0.510	0.388	0.583	0.087	0.217	0.673
	(0.090)**	(0.106)**	(0.157)*	(0.118)**	(0.127)	(0.097)*	(0.155)**
ΔlnK_t	-0.009	-0.007	-0.010	-0.002	-0.011	0.003	-0.014
	(0.005)*	(0.005)	(0.006)	(0.005)	(0.006)	(0.015)	(0.027)
ΔlnT_t	0.110	0.125	0.058	0.108	0.079	-0.053	-0.006
	(0.022)**	(0.036)**	(0.022)**	(0.036)**	(0.027)**	(0.038)	(0.045)
r	0.001	0.003	0.001	0.005	-0.006	-0.002	0.019
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)*	(0.014)	(0.014)
Constant	0.001	-0.000	0.010	-0.008	0.034	0.021	-0.060
	(0.006)	(0.008)	(0.009)	(0.009)	(0.009)**	(0.043)	(0.042)
Observations	3799	1185	743	1144	784	75	55
Anderson (p-value)	.000	.037	.187	.1397	.001	.000	.001
Hansen (p-value)	.525	.076	.464	.293	.629	.125	.250

Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

1. Excluding California cities.
Three through six year lags of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, and the real interest rate, are included as instruments for ΔlnR_t.
Only cities with a political score greater than 10 are considered in this specification.

Term limits in Democratic cities increase short-sighted behavior; prior to the implementation of term limits, Democratic cities finance about 22% of current expenditures with current revenue, compared to 67% afterward. Term limits also lead to increased short-sighted budgeting in Republican cities; prior to the implementation of term limits, cities fund about 34% of changes in current expenditures with changes in current revenues, compared to 69% after term limits are enacted. The estimates of λ for Democratic and Republican cities before the implementation of term limits are not significantly different from one another; term limits appear to have a uniform impact on fiscal decisions across cities of different ethnic and political mixtures.

Table 11 provides more detail as to how demographics influence fiscal decisions. Demographic variables are available every five years from the City and County Data Books. For this specification, changes in expenditures and revenues are calculated in

five year intervals. One (five year) lag of all variables used in the specification are included as instruments for ΔlnR_t and $\Delta lnR_T * X$, where X is the diversity index, percent of the population that is white, percent that is over age 65, population size (in thousands), percent that is college educated, median household income (in thousands of year 2000 dollars), percent of housing that is owner occupied, or the political score. Population size has the greatest impact on fiscal decisions (column (4)); the percent of the population that is white, over age 65, and the political score have smaller but statistically significant impacts on fiscal decisions. An increase in population of 1,000 people is associated with a decrease in the share of current spending that is financed by current revenues of 23 percentage points.

Table 11: **GMM estimation of equation (4) including demographic information**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta lnR_t, \lambda$	0.141	0.728	0.816	0.232	0.740	0.482	0.808	0.022
	(1.228)	(0.048)**	(0.148)**	(0.108)*	(0.168)**	(0.124)**	(0.196)**	(0.139)
ΔlnK_t	-0.004	-0.002	-0.002	0.004	-0.011	-0.004	-0.005	-0.001
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
ΔlnT_t	0.118	0.095	0.100	0.047	0.112	0.116	0.119	0.099
	(0.019)**	(0.014)**	(0.016)**	(0.016)**	(0.018)**	(0.022)**	(0.019)**	(0.015)**
r		0.143	0.136	0.139	0.120			0.140
		(0.018)**	(0.017)**	(0.017)**	(0.019)**			(0.017)**
$\Delta lnR_t * Score$								0.005
								(0.001)**
$\Delta lnR_t * \%owner\_occupied$							-0.004	
							(0.003)	
$\Delta lnR_t * median\_income^a$						0.004		
						(0.003)		
$\Delta lnR_t * \%college$					-0.004			
					(0.002)*			
$\Delta lnR_t * population^b$				-0.271				
				(0.081)**				
$\Delta lnR_t * \%over\_65\_years$			-0.039					
			(0.015)*					
$\Delta lnR_t * \%white$		-0.005						
		(0.001)**						
$\Delta lnR_t * diversity^c$	0.008							
	(0.016)							
Constant	-0.007	-0.339	-0.326	-0.313	-0.298	-0.005	-0.005	-0.328
	(0.009)	(0.040)**	(0.042)**	(0.039)**	(0.045)**	(0.012)	(0.008)	(0.040)**
Anderson (p-value)	.000	.000	.000	.000	.000	.000	.000	.000
Hansen (p-value)	.000	.019	.040	.191	.167	.000	.000	.017
Observations	713	1087	1087	1486	1087	713	713	1066

Robust standard errors in parentheses

* significant at 5%; ** significant at 1%

a. \$/1000

b. population/1000

c. 1-Diversity Herfindahl index

Differences are measured over five year intervals. One five year year lag of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, the real interest rate, and $\Delta lnR_t * X$ are included as instruments for ΔlnR_t.

Liquidity constraints, tax revenue elasticity, and demographic variables each influence tax-smoothing behavior. The final specification controls for these factors to verify that term limits influence fiscal decisions independently of these factors. Table 12 presents the estimation of the equation (5) below, where I is a dummy variable indicating whether a city has the local initiative, recall, or is in the five year period following the implementation of term limits. Lagged values of $\Delta lnR_t * \mathbf{Z}$ are included as instruments for ΔlnR_t and $\Delta lnR_t * I$, where $\mathbf{Z}$ is predicted per capita outstanding debt, the property tax share of revenues, population size, and the political score.

$$\Delta lnG_t = (1-\lambda)\mu + \lambda \Delta lnR_t + \lambda_2 \Delta lnR_t * I + \theta_1 \Delta lnK_t + \theta_2 \Delta lnT_t + \theta_3 r_t + (1-\lambda)\epsilon_t \quad (5)$$

λ_2 is the impact of institution I independent of the factors discussed above. As expected $\lambda_2 = 0$ for cities with the initiative and recall (columns (1) and (2) of table 12). For cities with term limits $\lambda_2 = .178$; in the absence of term limits, cities finance about 43% of current expenditures with current revenues. After term limits are implemented, 61% of current expenditures are financed with current revenues. This increase of about 18% is about the same as moving from the 90th percentile of credit access to having no access to borrowing.

Table 12: **GMM estimation of equation (5)**

	Initiative[1]	Recall[1]	Post Term Limits
	(1)	(2)	(3)
$\Delta lnR_t, \lambda$	0.512	0.370	0.425
	(0.108)**	(0.108)**	(0.077)**
ΔlnK_t	-0.010	-0.010	-0.006
	(0.003)**	(0.003)**	(0.007)
ΔlnT_t	0.083	0.081	0.048
	(0.010)**	(0.009)**	(0.016)**
r	0.001	0.001	0.006
	(0.001)	(0.001)	(0.005)
$\Delta lnR_t * Initiative$	0.010		
	(0.142)		
$\Delta lnR_t * Recall$		0.213	
		(0.153)	
$\Delta lnR_t * Post\_Term\_Limit$			0.178
			(0.068)**
Constant	0.003	0.003	-0.013
	(0.004)	(0.004)	(0.015)
Anderson (p-value)	.000	.161	.446
Hansen (p-value)	.017	.013	.369
Observations	4987	4987	394

Newey-West standard errors in parentheses
* significant at 5%; ** significant at 1%

1. Excluding California cities.
Three through six year lags of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, the real interest rate, and $\Delta lnR_t * \mathbf{Z}$ where $\mathbf{Z}$ is predicted per capita outstanding debt, the property tax share of revenues, population size, and the political score, are included as instruments for ΔlnR_t.

6 Conclusion

On average, cities in the United States finance about 65% of changes in current expenditures with current revenues. If city officials acted according to a perfect tax-smoothing model, then current expenditures would be entirely financed with permanent resources. However, municipal budgets in the United States are subject to balanced budget constraints and revenues limit the amount of debt that a city can issue. Thus, some degree of short-sighted budgeting is expected. By utilizing panel data, I have shown that local governments in the United States act according to the tax-smoothing model to a greater extent than previously estimated.

I provide evidence that at the municipal level in the United States, the threat of voter initiatives does not significantly influence fiscal policy decisions. The initiative and recall options serve to align constituent preferences and policy. The finding that local initiative is not associated with different fiscal decisions suggests that fiscal preferences of constituents and policy are aligned in the absence of the initiative. This finding is consistent across Republican and typically homogenous cities and Democratic cities, which are typically more diverse. At the state level, these results might differ; voters tend to initiate more tax and fiscal measures at the state level relative to the local level. Additionally, the fiscal decisions made at the state level are different from the tax and expenditure responsibilities of municipalities.

The recall option leads to short-sighted decisions in Democratic and Republican cities. When cities are politically unified, the recall is a real threat to politicians. However, in the pooled sample, the recall option does not have an average effect on policy.

Theoretically, term limits could either act to align constituent and politician interests or misalign these interests. Voters tend to believe that term limits eliminate unfair political advantages and policy flexibility that accompanies long-term political careers. On the other hand, there are no institutional barriers to ousting poor politicians and there are potentially significant benefits to having an experienced leader in office. I contribute to this debate by presenting evidence that term limits serve to sever constituent and politician interests and that the implementation of mayoral term limits has a detrimental effect on local fiscal policy. Specifically, implementing term limits has a similar effect of losing essentially all access to borrowing.

Aside from term limits, I find that population characteristics, liquidity constraints, and the local tax structure also influence the time horizon of municipal fiscal decisions.

The most forward looking cities in the United States are ethnically homogenous, have significant access to borrowing, and rely minimally on property taxes. These cities are able to borrow from creditors and adjust the timing of capital expenditures in order to maintain tax and current expenditure levels. Further, ethnic homogeneity reduces political disagreement and short-sighted decisions aimed at winning votes.

Short-sighted budgeting is associated with more volatile taxes and expenditures. In itself, this variance creates a dead weight loss. Further, in the case of economic downturns, cities with short-sighted practices do not have savings to sustain tax and expenditure levels. Intergovernmental grants from state and federal sources often provide the additional liquidity required to navigate economic downturns for these cities. Thus, short-sighted budgeting by municipalities potentially places a significant fiscal externality on other governments.

Appendix

A OLS specification

The bias of OLS estimates depends on the correlation between updated assessments of permanent income and changes in current revenues. If this correlation is positive (negative), then OLS estimates will be biased upward (downward). Campbell and Mankiw, in their original application to individual consumption behavior, hypothesize that this correlation will be positive; increases in current income are associated with increases in permanent income. This seems most likely for individual decision makers; current income that is allocated to savings represents an increase in permanent income.

However, it is not necessarily the case that future permanent income and changes in current revenues are positively correlated when considering government budgets. In response to negative shocks to current revenues, governments will either be forced to cut spending, increase taxes, borrow, or utilize savings. If governments cut spending or raise taxes during economic downturns, outmigration may occur and property values may fall. These effects represent decreases in permanent resources. Borrowing is costly, resulting in higher future taxes and decreases in permanent resources, for the same reasons. Thus, when faced with transitory shocks, cities that are unable to utilize savings anticipate lower permanent resources.

This hypothesis implicitly assumes that governments fail to accumulate and utilize savings because they are constrained or undisciplined, not because they are irrational. They update forecasts of permanent resources based on current fiscal decisions. If they did not change forecasts, then the OLS estimates would not be biased; changes in current resources would be orthogonal to the error term.

The intuition outlined above can be formalized in a decomposed version of equation (3). A transitory economic shock, in the absence of government fiscal response, leads to a change in current revenues. However, governments respond by raising or lowering taxes, expenditures, debt, or savings. So, the full value of the shock will not be represented in a change in current revenues; the change in revenues captures a combination of the economic shock and fiscal responses. Considering these government responses to a shock, changes in current revenues and expenditures can be decomposed into the following variables.

$$\Delta lnR_t = s_t + t_t + w_t + d_t$$

$$\Delta lnG_t = G_{t-1} + g_t$$

s_t is the effect of the economic shock on revenues, t_t is the government's initiatied change in tax revenue, w_t is the government's utilization of savings, d_t is the government's issuance or retirement of any debt, and g_t is any adjustment to current spending that the government makes in response to the economic shock. Based on the preceding discussion, I make the following assumptions.

$$cov(s_t, \epsilon_t) > 0$$

$$cov(t_t, \epsilon_t) < 0$$

$$cov(w_t, \epsilon_t) = 0$$

$$cov(d_t, \epsilon_t) < 0$$

$$cov(g_t, \epsilon_t) > 0$$

According to the properties of covariance,

$$cov(\Delta lnR_t, \epsilon_t) = cov(S_t, \epsilon_t) + cov(t_t, \epsilon_t) + cov(W_t, \epsilon_t) + cov(D_t, \epsilon_t) + cov(g_t, \epsilon_t)$$

Since the magnitudes of these covariances are unknown, it is impossible to sign $cov(\Delta lnR_t, \epsilon_t)$, and thus the bias of the OLS estimates. OLS estimates of λ will be downward biased if economic shocks are met largely with changes in taxes and debt, upward biased if the government takes no fiscal action in response to shocks or if spending changes in response to shocks, and unbiased if only savings are used in response to shocks. Without a direct measure of the ex-ante level of the shock, it is difficult to determine the mixture of fiscal responses that governments utilize. Thus, by instrumenting for current year economic shocks, the bias of estimates is eliminated, although the direction of the bias is unclear.

Table 13: **OLS estimation of equation (4) by access to initiative, recall, and implementation of term limits.**

	All	Initiative[1]	No Initiative	Recall[1]	No Recall	Pre Term Limits	Post Term Limits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔlnR_t, λ	0.451	0.453	0.414	0.503	0.340	0.428	0.458
	(0.039)**	(0.069)**	(0.047)**	(0.063)**	(0.035)**	(0.090)**	(0.077)**
ΔlnK_t	-0.007	-0.008	-0.004	-0.007	-0.006	0.001	-0.011
	(0.002)**	(0.004)*	(0.003)	(0.004)	(0.003)*	(0.010)	(0.011)
ΔlnT_t	0.056	0.056	0.045	0.059	0.040	0.036	0.049
	(0.006)**	(0.009)**	(0.009)**	(0.009)**	(0.008)**	(0.018)*	(0.021)*
r	-0.000	-0.001	-0.001	-0.001	-0.000	-0.000	0.007
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.009)	(0.009)
Constant	0.013	0.015	0.018	0.014	0.018	0.008	-0.011
	(0.004)**	(0.006)*	(0.007)**	(0.006)*	(0.006)**	(0.028)	(0.023)
Observations	11737	5365	4060	5494	3931	234	300
Newey-West Standard errors in parentheses							

1. Excluding California cities.

B Instrumental variables estimation

Table 14: **IV estimation of equation (4) by access to initiative, recall, and implementation of term limits.**

	All	Initiative[1]	No Initiative	Recall[1]	No Recall	Pre Term Limits	Post Term Limits
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔlnR_t, λ	0.597	0.399	0.271	0.415	0.242	0.339	0.621
	(0.130)**	(0.107)**	(0.209)	(0.129)**	(0.163)	(0.148)*	(0.144)**
ΔlnK_t	-0.013	-0.008	-0.002	-0.004	-0.006	0.003	-0.012
	(0.004)**	(0.005)	(0.005)	(0.005)	(0.004)	(0.010)	(0.012)
ΔlnT_t	0.067	0.083	0.056	0.085	0.054	0.061	0.027
	(0.010)**	(0.020)**	(0.012)**	(0.021)**	(0.011)**	(0.034)	(0.023)
r	0.002	0.004	0.005	0.004	0.005	0.007	0.009
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.011)	(0.009)
Constant	-0.001	-0.003	-0.001	-0.004	0.001	-0.012	-0.022
	(0.004)	(0.005)	(0.007)	(0.006)	(0.006)	(0.035)	(0.022)
Observations	9511	4329	3278	4435	3172	213	272
Anderson (p-value)	.000	.000	.058	.000	.014	.064	.001
Hansen (p-value)	.004	.003	.478	.126	.637	.238	.401

Newey-West standard errors in parentheses

* significant at 5%; ** significant at 1%

1. Excluding California cities.

Three through six year lags of ΔlnR_t, ΔlnG_t, ΔlnK_t, ΔlnT_t, and the real interest rate, are included as instruments for ΔlnR_t.

C Calculation of political score[29]

Most analyses of party alignment use voting records as a measure of the strength of party preference.[30] However, the vote share for a particular candidate does not necessarily reflect the jurisdiction's preference for the candidate's party.

A single vote for a candidate might represent a preference for the platform of the candidate's party, a preference for that candidate's personal characteristics relative to her opposition, or both. An aggregated vote share is influenced by preference for particular party platforms as well as individual candidate error terms. Thus, vote share is a noisy measure of the jurisdiction's preferred party platform. In this section, I present a simple model of voter behavior to motivate an empirical measure of underlying political party preferences derived from demographic data.

Suppose political party platforms, P, are represented by a point on a policy spectrum.[31] P reflects, for example, the tendency of Democrats to favor social insurance programs and income redistribution and the tendency of Republicans to favor socially conservative policies, smaller government, and lower taxes. A voter chooses to align with either party and further considers personal characteristics when voting. Relevant candidate characteristics might include incumbency effects, charisma of candidates, deviations from the party platform, campaign efforts, or popularity shocks.

Consider an election between two candidates, each aligned with a political party platform $P = D, R$. Candidates, c, are endowed with a popularity shock (or other individual characteristic that might influence voters) at the beginning of the election, v. Popularity shocks for each candidate, e_{Pcv}, are independently and identically distributed. Thus, candidates are represented by their policy platforms plus any personal deviations:

$$P + e_{Pcv}$$

Constituents vote for the candidate who provides them the greatest utility. A voter has a political preference, e_i toward D. $e_i = 0$ represents a voter who does not prefer either party's platform and votes based on politician characteristics, e_{Dcv} and e_{Rcv}, alone. A large value of e_i represents a voter who is firmly committed to vote Democrat, a "core"

[29]The political score used in the analysis is derived in Donovan (2009b). This appendix describes the derivation of this score. For more detail, see Donovan (2009b).

[30]See, for example, Grossman (1994) , Levitt and Snyder (1995). [18, 24]

[31]This spectrum can be thought of as ranging from left-wing to right-wing policies, for example.

supporter. A highly negative value of e_i represents a "core" Republican supporter who strongly opposes the Democrat platform. Thus, a Democratic voter has preferences:

$$U(D + e_i) > U(R)$$

A constituent will vote for D if,

$$U(D + e_{Dcv} + e_i) > U(R + e_{Rcv})$$

and will vote for R if,

$$U(R + e_{Rcv}) > U(D + e_{Dcv} + e_i).$$

For example, a voter with a positive bias toward D, ($e_i > 0$), will vote for the Republican candidate if the Republican candidate is endowed with a large enough popularity shock and/or the Democratic candidate receives an especially poor popularity shock.

An important assumption is that popularity shocks, e_{Pcv}, for N candidates in V elections, are normal, independent and identically distributed, so that

$$\sum_{v=1}^{V} \sum_{c=1}^{N} e_{Pcv} = 0.$$

Further, individual preferences can be aggregated to a jurisdiction, J, level, so that the mean Democratic bias for a jurisdiction is given by,

$$\bar{e_J} = \frac{1}{I} \sum_{i=1}^{I \in J} e_{iJ}$$

C.1 Measuring political preferences

C.1.1 Demographic specification

Previous studies simply use voting data to infer political preferences for various geographic divisions.[32] However, as described above, personal characteristics of candidates are independent of voter political preferences and may sway a constituent to vote against her preferred party platform. Although the actual vote might be an appropriate measure for some analyses, a measure of the underlying preference for party platforms is better suited for others. In this section, I present an econometric specification to predict these party preferences for any geographic level for which demographic data are available. Specifically, I predict political preferences for 435 cities in the United States.

[32] See, for example, Grossman (1994) , Levitt and Snyder (1995), Ansolebehre (2008). [18, 24, 4]

A person's stated party affiliation (e_i), and the party of the candidate for whom she votes are strongly correlated. According to the National Elections Study (NES) for periods 1968-1996, constituents vote according to their stated party affiliation most of the time. Of the respondents who identified as Democrats, 81% and 77% voted for the Democratic candidate in the previous gubernatorial and presidential races, respectively. For identified Republicans, 84% and 86% voted for the Republican candidate in the previous gubernatorial and presidential races, respectively.[33]

In order to derive political biases from voting data, either candidate popularity shocks or voter party preferences must be identified. Currently, there are no comprehensive data available for popularity, charisma, or campaign efforts for candidates. Further these variables might have heterogenous effects on different types of voters. On the other hand, there are several sources of data on voter characteristics. Voter registration is the best available measure of party preferences although it is not widely available at sub-state geographic levels in the United States. I argue that these political preferences can be accurately inferred using demographic data, a source that is widely available from the decennial census,and every five years from the City and County Data Books.

Political scientists have provided some evidence that demographics are correlated with voter preferences. Aldrich et. al (2007) find that a demographic model performs similarly well to a model that utilizes voter opinions on issues to predict the party of a representative [1]. I provide further evidence that demographics are sufficient to infer party preference using the NES for various years between 1968 and 1996.

The National Election Study is conducted biennially since 1948. The universe for the election year studies is all United States citizens of voting age on or before election day of the same year. Potential voters are randomly selected for telephone interviews. The data include "a series of demographic variables and measures of social structure, partisanship, candidate evaluation, reptrospective and incumbent presidential evaluation, public opinion, ideological support for the political system, mass media usage, and equalitarianism and post-materialism. Additional items provide measures of political activity, participation, and involvement, and voting behavior and registration..." [35]

[33]Respondents rated their party affiliation on a scale of 1 to 7, where 1 corresponds to a strong Democrat and 7 corresponds to a strong Republican. 3 is a Democrat-leaning Independent, 4, a true Independent, and 5, a Republican-leaning Independent. The percentages reported here were derived from respondents who rated themselves as 1 or 2 for Democrats and 6 or 7 for Republicans. Note that the correlation is smaller when leaning Independents are included.

In the proceeding analysis, I utilize voters' stated party affiliation and various demographic variables from the National Elections Studies. Respondents rate their party affiliation on a scale of 1 to 7, where 1 corresponds to a strong Democrat and 7 corresponds to a strong Republican. An ordered probit regression of a person's stated political number on a vector of demographic characteristics, $\mathbf{X_i}$, indicates that several characteristics are significant in predicting a persons political number. The following model is estimated for several years between 1968 and 1996.

$$Number_i = \mathbf{B} * \mathbf{X_i} + \epsilon_\mathbf{i}$$

The ordered probit specifications are reported in Table C.1.1 for a selected set of the most statistically significant coefficients.[34] The signs on the coefficients are of the expected sign; richer people tend to identify as Republicans and blacks and hispanics identify as Democrats, as do Jewish people. Urban location is also a significant predictor of Democratic leaning. Interestingly, men are more likely to identify as Republicans. The party affiliation of parents is one of the strongest predictors of a persons political preference.

[34]The complete list of variables used in the specification is: Male, Black, Hispanic, Race: other, Occupation: Professional, Occupation: Clerical, Occupation: Service, Occupation: Labor, Occupation: Farm, Occupation: Homemaker, Age: 17 to 24, Age: 25 to 34, Age: 35 to 44, Age: 45 to 54, Age: 55 to 64, Age: 65 to 74, Age: 75 to 99, Education: Grade school, Education: HS dropout, Education: HS graduate, Education: some college, Education: Bachelor's degree, Urban, Rural, Working class, Middle class, Upper Class, Region: Northeast, Region: South, Region: West, Religion: Protestant, Religion: Catholic, Religion: Jewish, Income: 0-20 %tile, Income: 21-40 %tile, Income: 41-60 %tile, Income: 61-80 %tile, Income: 81-100 %tile, Democrat Father, Republican Father, Democrat Mother, and Republican Mother.

Table 15: **Ordered Probit: Self-Reported Political Number**[a]

	1968	1972	1974	1980	1988	1992	1994	1996
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male	0.066	0.038	0.114	-0.010	0.177	0.237	0.254	0.315
	(0.90)	(0.72)	(2.19)*	(0.16)	(3.27)**	(4.86)**	(4.55)**	(5.52)**
Age: 65 to 74	-0.152	0.393	-0.693	-1.164	0.303	-0.112	-0.399	-0.049
	(0.31)	(1.31)	(3.11)**	(1.41)	(0.41)	(1.13)	(3.37)**	(0.08)
Black	-1.256	-0.710	-0.776	-0.705	-0.686	-0.702	-0.978	-0.892
	(11.13)**	(9.05)**	(9.23)**	(7.02)**	(8.23)**	(9.42)**	(10.73)**	(9.78)**
Hispanic				-0.137	-0.209	-0.038	-0.289	-0.265
				(0.93)	(2.20)*	(0.46)	(2.97)**	(2.73)**
Urban	-0.057	-0.062	-0.256	-0.183	-0.127	-0.286	-0.076	-0.165
	(0.75)	(1.09)	(4.54)**	(2.55)*	(1.96)	(5.11)**	(1.18)	(2.42)*
Top Income	-0.027	0.433	0.527	0.530	0.695	0.238	0.349	0.422
	(0.13)	(2.88)**	(3.49)**	(3.44)**	(3.70)**	(1.80)	(2.03)*	(2.68)**
Professional	0.403	0.239	-0.113	0.175	0.151	-0.437	0.002	-0.075
	(2.35)*	(1.90)	(0.90)	(1.28)	(1.02)	(3.83)**	(0.01)	(0.46)
Clerical	0.479	0.206	0.074	0.033	0.229	-0.315	0.166	0.055
	(2.74)**	(1.64)	(0.59)	(0.24)	(1.57)	(2.78)**	(1.18)	(0.34)
Upper Class	0.207	0.263	0.311	0.129	0.240	0.127	0.288	
	(2.06)*	(3.31)**	(3.73)**	(1.28)	(2.69)**	(1.57)	(3.08)**	
Protestant	0.270	0.227	0.218	0.067	0.174	0.284	0.401	0.362
	(2.11)*	(2.46)*	(2.59)**	(0.74)	(2.05)*	(4.52)**	(5.53)**	(4.72)**
Jewish	-0.697	-0.527	-0.657	-0.721	-0.279	-0.610	-0.733	-0.557
	(3.29)**	(3.20)**	(4.10)**	(3.91)**	(1.35)	(3.56)**	(3.76)**	(2.68)**
Democrat Father	-0.340	-0.387		-0.342	-0.218	-0.377		
	(3.97)**	(5.90)**		(4.18)**	(2.62)**	(5.26)**		
Observations	1531	2656	2433	1577	1999	2447	1772	1694

Absolute value of z statistics in parentheses

* significant at 5%; ** significant at 1%

[a]Self reported number: 1=Strong Democrat, 7=Strong Republican

The model predicts a persons stated political affiliation relatively well. Table 16 reports the average predicted probabilities for each number of the 1 to 7 scale. The first panel represents the predictions for 1968, using the coefficients from the 1968 regression coefficients. A strong Democrat is predicted to be a strong Democrat with an average probability of .35 and is either a strong or moderate Democrat with average probability of .64. A strong Republican is predicted to be a strong Republican with an average probability of .2 and a moderate Republican with average probability .23. The self-reported number is, or is within one unit of, the *most likely* predicted number.

Table 16: **Political number predicted probabilities: 1968 Ordered probit coefficients, 1968 data**[bc]

	1	2	3	4	5	6	7
1	**0.35**	0.29	0.09	0.09	0.06	0.08	0.04
2	0.26	**0.29**	0.11	0.11	0.08	0.10	0.05
3	0.19	0.27	**0.11**	0.12	0.09	0.14	0.08
4	0.16	0.26	0.11	**0.13**	0.10	0.15	0.08
5	0.11	0.23	0.11	0.13	**0.11**	0.19	0.13
6	0.08	0.18	0.10	0.12	0.12	**0.22**	0.19
7	0.07	0.17	0.09	0.12	0.12	0.23	**0.20**

[b]Political number: 1=Strong Democrat, 7=Strong Republican.

[c]Rows: self-reported political numbers.

Columns: mean predicted probabilities.

Table 17: **Political number predicted probabilities: 1968 ordered probit coefficients, 1980 data**[bc]

	1	2	3	4	5	6	7
1	**0.34**	0.29	0.10	0.09	0.07	0.08	0.04
2	0.25	**0.28**	0.10	0.11	0.08	0.12	0.06
3	0.19	0.27	**0.11**	0.12	0.10	0.14	0.08
4	0.16	0.25	0.11	**0.12**	0.10	0.16	0.10
5	0.11	0.21	0.10	0.12	**0.11**	0.20	0.15
6	0.08	0.18	0.10	0.12	0.12	**0.22**	0.19
7	0.09	0.17	0.09	0.12	0.11	0.22	**0.20**

[b]Political number: 1=Strong Democrat, 7=Strong Republican.

[c]Rows: self-reported political numbers.

Columns: mean predicted probabilities.

In order to make meaningful predictions of political behavior based on demographic characteristics, it is important that the relationship between demographics and politics be relatively stable over time. In order to test this, I use the 1968 regression coefficients to predict political preferences for demographics and stated preferences in 1972 and 1980 (Tables **??** and 17).[35] The predictions are remarkably consistent from 1968 to 1980. Indeed, the coefficients are just as powerful in prediction 12 years later. This persistence in the relationship between demographics and political preferences validates the use of demographic data to predict political alignment of individuals over time.

C.1.2 Political score for US cities

The argument made above is that demographic variables are sufficient to calculate an individual's bias toward the Democratic party, but not to predict how she will actually vote, though the two are highly correlated. In this section, I use similar demographic variables as those included in the NES to estimate $\bar{e_J}$ for 435 cities in the United States.[36]

For any election outcome, a Democratic candidate's victory can be attributed to either a positive popularity shock for the Democratic candidate ($e_{Dcv} > 0$), a negative popularity shock for the Republican candidate ($e_{Rcv} < 0$), and/or strong preferences for the Democratic platform ($\bar{e_J} > 0$). For a sufficiently large number of elections, $\bar{e_J}$ is easily recovered since $\sum_{v=1}^{V} \sum_{c=1}^{N} e_{Pcv} = 0$.

One option is to calculate the average Democrat vote share for a given jurisdiction over a sufficiently long time period. Ansolebehre and Snyder [4] use an eight year average of the Democratic vote share in a given jurisdiction to measure local preferences. Levitt and Snyder [24] use the average of three presidential election vote shares. Although these measures fare better than a single vote, the samples are not large enough to completely isolate preferences from popularity shocks.

Instead of calculting an average of relatively few votes within a county, vote returns are projected onto demographic variables for all counties in the United States. This projection isolates the party bias from the effect of candidate characteristics within counties, as long as candidate error terms are normal i.i.d. and demographics sufficiently

[35] These years were chosen because they have exactly the same set of demographic variables available.

[36] Demographic data are from City and County Data Books, published every five years. Of the 506 cities in the Annual Survey of Government Finances sample, 435 appear in the City and County Data Books. County vote returns for senate, gubernatorial, and presidential elections are available from 1970 to 2004 [21, 23].

predict party preferences. Then,$\bar{e_J}$ is imputed for 435 cities using the coefficients from the county regression. One benefit from this approach is that, by estimating the preference coefficients for demographics, preferences can be imputed for jurisdictions where vote records are not consistently available. [37] [38]

The precise specification for this imputation is presented in equations 6 and 7. Senate, gubernatorial, and presidential election vote returns v, for all counties y, in time periods t,are projected onto county demographic characteristics, $\mathbf{X}$.[39] $\boldsymbol{\beta}$,the vector of coefficients for a vector of demographic characteristics, is recovered from this regression. $\%P$ represents the county vote share for $P = D, R$. In addition to demographic characteristics, there might be variation in $\bar{e_J}$ across states due to regional economic variations or regional social conditions, for example.[40] These variations in $\bar{e_J}$ are incorporated into the regressions by including State-Year fixed effects.

$$\%D_{ytv} - \%R_{ytv} = \alpha + \beta * X_{yt} + \gamma * (State\_Year) + e_{Dcv} + e_{Rcv} + \epsilon_{ytv} \tag{6}$$

Substituting $\sum_{v=1}^{V} \sum_{c=1}^{N} e_{Pcv} = 0$, the specification becomes,

$$\bar{e_{yt}} = \alpha + \beta * X_{yt} + \gamma * (State\_Year) + \epsilon_{yt}$$

I then make the following imputation for cities, J:

$$\bar{e_{Jt}} = \alpha + \beta * X_{Jt} + \gamma * (State\_Year) \tag{7}$$

The coefficients, $\boldsymbol{\beta}$,are reported in table 18, for five time intervals. The coefficients are consistent with the self-reported political score model presented in section C.1.1 that utilizes National Elections Studies. The ordered probit model revealed that people in the top income quintile and people who consider themselves in the upper class are more likely to identify as Republicans. The negative coefficient on % Owner Occupied Housing– a

[37] Presidential, senatorial, and gubernatorial voting records are available at the county level from ICPSR study 13 and uselectionatlas.org. Demographic data are available at both the county and city level from the City and County Data Books, with supplements from the deci-annual Census.

[38] Indeed, the same calculation can be made for census tracts, school districts, or any jurisdiction for which demographics are aggregated. This is a potentially valuable statistic for researches and politicians alike.

[39] The time periods are determined by the City and County Data Book, published every five years.

[40] For example, Southern whites who were historically Democrat turned largely Republican after the signing of the Civil Rights Act of 1964.

measure of wealth– represents a preference for the Republican party platform.[41] Per capita income has a negative coefficient through 1988 and after 1988, the coefficient for median income is negative and statistically significant. This is consistent with the notion that richer people prefer Republicans, ceteris paribus.

The ordered probit model reveals that urban status is associated with being Democrat. Population density in table 18 is highly correlated with Democratic preferences. As expected, Democratic support is increasing in the percentage of the population that is black and Hispanic. Also consistent with the ordered probit model, Republican support is increasing in the percent male within a county. The percent Jewish within a county population is the strongest predictor of Democratic support.[42] These coefficients are used to predict political party preferences for 435 cities in 48 states.

[41]Note that in Section C.1.1 Democratic preferences were represented by smaller numbers on the 1 to 7 scale. For the remainder of the appendix, however, a smaller number represents a more Republican bias. This is because the bias is derived from the difference of votes: $\%Dem - \%Rep$.

[42]Data on religious affiliation were collected from The Association of Religion Data Archives, http://www.theARDA.com. The number of Jewish adherents is available at the county level for, 1980, 1990, and 2000.[13, 31, 30] The number of Jewish adherents in 1980 is appended to county demographic variables from the 1977 City and County Data Book. The number of Jewish adherents in 1990 is appended to county demographic variables from the 1983, 1988, and 1994 City and County Data Books. Jewish adherent data from 2000 is appended to county demographic variables from the 2000 City and County Data Book.

Table 18: **OLS estimation of equation 6. County Vote Returns (%Dem-%Rep)**

	1970-1977	1978-1983	1984-1988	1989-1994	1995-2004
	(1)	(2)	(3)	(4)	(5)
Population/ mile^2	0.000	0.001	0.000	0.000	0.000
	(2.77)**	(4.90)**	(2.16)*	(2.54)*	(0.17)
Population	0.000	0.000	-0.000	0.000	0.000
	(1.45)	(0.40)	(0.47)	(0.52)	(1.42)
Birth Rate	-0.037	-1.021	-0.839	-0.792	-0.268
	(0.55)	(11.66)**	(9.60)**	(8.01)**	(4.36)**
Income Per Capita	-0.002	-0.002	-0.001	0.001	0.002
	(5.25)**	(3.41)**	(5.53)**	(4.65)**	(14.91)**
Median Income	0.002	0.000	0.001	-0.001	-0.001
	(7.57)**	(2.50)*	(7.66)**	(8.10)**	(24.59)**
% Owner Occupied	-0.356	-0.121	-0.316	0.020	0.167
	(11.33)**	(3.75)**	(7.68)**	(0.46)	(5.54)**
% Over 65	0.674	-0.536	0.081	-0.391	-0.853
	(8.57)**	(5.50)**	(0.83)	(4.53)**	(13.53)**
% White	-0.337	-0.378	-0.012	-0.385	-0.486
	(8.90)**	(9.52)**	(0.76)	(10.32)**	(20.03)**
% Black	-0.085	0.129	0.623	0.239	0.345
	(2.13)*	(3.01)**	(24.43)**	(6.03)**	(13.16)**
% Hispanic	0.275	0.090	0.375	0.207	0.372
	(13.30)**	(3.32)**	(12.69)**	(7.89)**	(19.13)**
% HS Graduates	-0.116	-0.407	-0.479	-0.153	0.318
	(2.80)**	(7.85)**	(9.07)**	(2.52)*	(6.67)**
% College	-0.539	-0.624	-0.555	-0.140	0.269
	(13.84)**	(15.01)**	(12.88)**	(2.99)**	(5.67)**
% Male	0.458	-0.632	-0.641	-0.833	-0.520
	(5.23)**	(3.83)**	(4.03)**	(5.79)**	(6.41)**
% Jewish	3.940	5.620	7.412	1.353	1.686
	(3.65)**	(4.46)**	(5.39)**	(6.50)**	(11.33)**
Constant	16.778	115.265	31.674	182.547	89.426
	(2.39)*	(11.12)**	(3.43)**	(18.32)**	(15.48)**
Observations	21967	13622	15267	13815	26657
R^2	0.28	0.39	0.30	0.33	0.38
State x Year FE	Y	Y	Y	Y	Y

Absolute value of t statistics in parentheses
* significant at 5%; ** significant at 1%

The analysis relies on the correct assignment of political preferences to cities. Thus it is important that the predicted political score, derived from demographic characteristics, is consistent with other available measures and that coefficients used in the prediction are not spuriously estimated across periods.

C.1.3 Political score and voter registration

Voter registration is a good measure of a person's preferred party platform.[43] Voters register as a Democrats, Republicans, various third party options, or decline to state their party affiliations. Those with strong preferences for Democrats will register Democrat, those with strong Republican preferences will register Republican, and those with a small e_i or $e_i = 0$ will register some other option. Within a municipality, the difference between registered Democrats and Republicans translates well into a measure of the aggregated municipal preference for the Democratic party, the measure derived from the county vote share regressions on demographics.

Figure 7 plots voter registration against the imputed political score for the 66 California cities in the sample.[44] The correlation is .58 between voter registration and political score. The outliers below the 45-degree line are Newport Beach. Excluding Newport Beach, the correlation increases to .63. The strong correlation between the two measures additionally validates the use of demographic measures to infer political bias.

C.1.4 Stability of coefficients over time

Coefficients estimated using county vote data should consistently estimate political party preferences for time periods in the future. In order to test this, I use coefficients from the 1978-83 regression to estimate the political score for cities in all time periods.[45] Metropolitan populations tend to be highly mobile, thus we might expect some changes in demographic composition and political biases in these areas. But, to the extent that these changes happen slowly over time, the estimated bias of a city should be correlated with the prediction for subsequent time periods. Table 19 presents these correlations. Coefficients from 1978-1983 consistently predict political biases through 2004.

[43] Of course, there are reasons why a person might not register sincerely, or would decline to declare a party. Short of interviewing all residents in a county, registration is the best available measure to party preference.

[44] Voter registration data was collected from the state reports of registration for years 1970, 1976, 1980, 1985, 1990, 1992, 1995, 1998, 2000, 2001, and 2002. Reports are available for the state of California from the California Secretary of State and at http://www.sos.ca.gov.

[45] The income variable for 1970-1977 is median family income; after 1977 median household income is available. The 1978-83 coefficients are used for this exercise so that the variables used for estimation are consistent with the variables available in subsequent time periods.

Figure 7: **Voter registration and political score, corr=.58.**

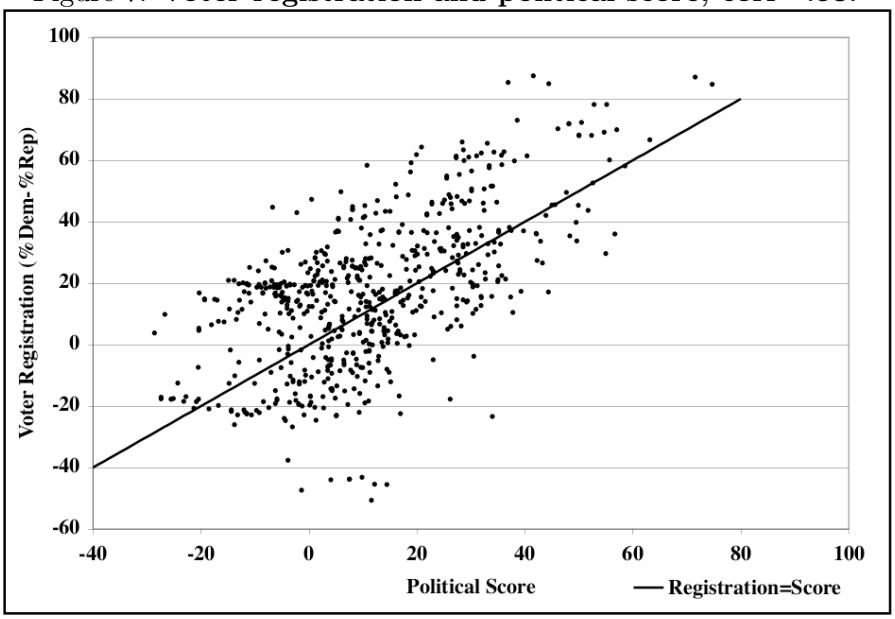


Table 19: **Correlation, Predicted political biases for municipalities using 1978-1983 regression coefficients**

	Score 1978-83	Score 1984-88	Score 1989-94	Score 1995-04
Score 1978-83	1			
Score 1984-88	0.93	1		
Score 1989-94	0.82	0.86	1	
Score 1995-04	0.65	0.72	0.78	1

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