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ELECTED VERSUS APPOINTED POLICYMAKERS: EVIDENCE FROM CITY TREASURERS

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Elected Versus Appointed Policymakers

Were all California cities with elected treasurers to replace them with appointed ones, they could save more than \$20 million collectively.

How cities pick their treasurers whether, by elections or through appointment, can have an impact on their cost of borrowing. In California, cities that appoint treasurers spend 13 to 23 percent less in borrowing costs than comparable cities with elected treasurers, according to Elected Versus Appointed Policymakers: Evidence from City Treasurers (NBER Working Paper No. 15643). Were all California cities with elected treasurers to replace them with appointed ones, they could save more than \$20 million collectively, author Alexander Whalley estimates.

Previous studies of whether elected officials or bureaucrats do a better job at controlling borrowing costs have produced mixed results. In fact, the most compelling empirical research suggests that elected electricity regulators choose lower prices than appointed regulators, and that elected judges are more encouraging toward employment-discrimination lawsuits than appointed judges. But using real-world data to prove a causal relationship is difficult, because so many other factors beyond the method of selecting public officials may play a role.

In this study's sample of 203 California cities between 1995 and 2006, for example, it is relatively easy to see that cities with elected treasurers paid 15 percent more to borrow money than cities with appointed treasurers. But the cities with elected treasurers also had more debt on average, were more likely to have directly elected mayors and clerks, and had lower per capita income and a less educated population, all of which could also influence borrowing costs. Controlling for those factors, cities with appointed treasurers paid 13 percent less to borrow than cities with elected treasurers.

To get at the issue of causality, this study further refines the sample to examine the 31 cities that held a referendum during the period to replace an elected treasurer with an appointed one. Ten cities approved such a change. Whalley argues that cities where the referendums either

succeeded or failed by a very narrow margin were likely to be quite similar, on average. From that core sample, he finds that cities spend 23 percent less to borrow money if they have an appointed treasurer.

City treasurers can affect a city's borrowing costs in two ways: by issuing new debt and by refinancing existing debt. This study finds little difference between appointed and elected treasurers on the costs of issuing new debt. But on refinancing existing debt, an activity that requires significant skill and expertise, the appointed city treasurers were much better at getting lower interest rates. One reason may be that appointed treasurers often have higher levels of education (often an MBA or MPP degree) than elected treasurers do.

The findings suggest that in some cases, there may be benefits, such as reduced borrowing costs in this case, associated with assigning technical policy-making tasks to appointed officials with specialized expertise. The results of this study also have broader implications for the organization of public good provision, Whalley concludes. Efforts to improve governance in developing countries may well be enhanced by emulating the division of policymaking tasks in advanced democracies.

-- Laurent Belsie

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Elected Versus Appointed Policymakers: Evidence from City Treasurers*

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Abstract

This paper investigates whether methods of public official selection affect policymaking in cities. I draw on the unique characteristics of California's city referendum process to identify the causal effect of city treasurers' method of selection on their cities' debt management policies. I utilize a regression discontinuity strategy based on the effect of narrowly-passing appointive city treasurer referendums on city borrowing costs. The results indicate that appointive treasurers reduce a city's cost of borrowing by 13% to 23%. The results imply that if all cities in California with elected treasurers were to appoint them, total borrowing expenditures would be reduced by more than \$20 million per year. Appointive city treasurers appear to reduce borrowing costs primarily through the refinancing of expensive debt at lower interest rates.

Keywords: Institutions, Policy Choices, Local Government.

JEL Classifications: D7, H1, H7.

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1. Introduction

Understanding how political institutions affect policy outcomes has long been a central issue in economics.¹ Many differences in political institutions center on how leaders are selected and held accountable. In this paper, I compare the policy choices of public officials who are selected and held accountable in very different ways - bureaucrats and politicians - for the case of city treasurers.

The case of case of city treasurers is particularly compelling to examine for two reasons. First, as both elected and appointed city treasurers' are responsible for debt management policy, the differences in costs cities pay to borrow provide a clear measure of different debt management policy choices.² Second, quasi-experimental variation in the method of selection for city treasurers is available from votes on city referendums, allowing for concerns about the endogeneity of institutions to be effectively addressed. The context of city treasurers provides a rare opportunity to examine whether differences public official selection methods actually cause meaningful differences in policymaking.

Methods of public official selection can affect policymaking through the competence and effort policymakers supply to the policy task (e.g. Besley, 2006). Recent theoretical contributions argue that bureaucrats have a comparative advantage in technical policymaking (see for example Maskin and Tirole, 2004; Alesina and Tabellini, 2007, 2008; and Vlaicu, 2008). Empirically the most compelling evidence that shows politicians choose policies more favorable towards voters. Besley and Coate (2003) find that elected electricity rate regulators choose lower prices than appointed regulators. Besley and Payne (2006) find that more employment discrimination lawsuits are submitted when a judge is elected rather than appointed.³

¹Besley and Case (2003) provide an excellent survey of the literature.

²Treasurers are also allocated the primary tasks of the receipt and safekeeping of public money coming into the Treasury, compliance with laws governing the deposit and paying out of public money, and submission to the city council of a monthly report accounting for all revenue, expenses, and balances in city accounts. Treasurers may also be in charge of collection of city revenues, as well as, cash flow and investment policy for city funds. See CMTA (2001) for further details. City treasurers are not responsible for city fiscal policy or the decision to borrow however.

³There is large literature on the effects of appointed and elected regulators that has generally found mixed results. The mixed findings in the earlier literature are likely due to not addressing the institutional endogeneity issue as carefully as the more recent studies. See Besley and Coate (2003) for an excellent survey of the prior literature.

Whether the empirical evidence squares with the theoretical arguments remains an open question. It is unclear, for example, whether evidence from electricity rate regulation and judicial decision making outcomes primarily reflects differences in public official policy performance or differences in pandering to public opinion. In this paper I study the appointive effect for a different technical policy issue, one where policy choices affect the cost of a specific government service and closely reflect public official policy performance. The results of my analysis inform the debate about whether the theoretical predictions of recent models line up closely with the empirical evidence.

Beside an obvious interest for political economists, the existence of policy outcome and performance differentials between elected and appointed officials has tremendous practical relevance. In particular, with many cities spending substantial sums to service their debt every year, improved debt management policy could well increase government efficiency. As Arthur Levitt, the former chairman of the Securities and Exchange Commission, writes:

“State, county and municipal entities across the nation enjoy a privileged position in the debt markets – the interest they pay is often tax-free and their market is lightly regulated. So, how is it possible that local entities frequently pay too much to borrow money?... Many elected and appointed officials simply don’t care... Taxpayers should be irate.” Levitt (2009).

The key empirical challenge faced in estimating the appointive effect is to disentangle the effect of city treasurer selection methods on policy choices from other determinants of city borrowing costs. One central factor that affects city borrowing costs is default risk. Factors that affect default risk, such as the level of city debt or the economic conditions of the local economy, may well affect the choice of city treasurer selection methods. For example, as the benefits of effective debt management policy are especially large for cities with large amounts of debt these cities may be more likely to choose city treasurer selection methods resulting in lower borrowing costs. As cities with large levels of debt are subject to more default risk, these cities are likely to pay higher borrowing costs anyway. Furthermore, as many components of default risk are frequently unobserved, city treasurer selection methods are likely to be endogenously related to the unobserved default risk of a city.⁴

The central contribution of this paper is to estimate the causal effect of bureaucratic ver-

⁴See Besley and Case (2003) and Acemoglu (2005) for discussions on the implications of endogenous institutions for estimating the causal effect of institutions. Aghion, Alesina, and Trebbi (2004) and Acemoglu, Ticchi and Vindigni (2006) introduce theoretical models of the endogenous determination of institutions.

sus political selection methods on policymaking. I implement a regression discontinuity (RD) research design to estimate the causal effect of city treasurer selection methods on city borrowing costs.⁵ The design takes advantage of the unique characteristics of California's local referendum process to isolate quasi-experimental variation in city treasurer selection methods. Many differences in city treasurer selection methods across cities are likely due to differences in unobserved city institutions, resident preferences, or special interest strength that also likely affect borrowing costs. Cities where residents vote to pass an appointive city treasurer referendum may well differ on both observable and unobservable dimensions from those that do not. However, cities in which an appointive treasurer referendum passes by a *very narrow margin* are likely to be quite similar on average to cities where an appointive treasurer referendum fails by a very narrow margin. Taking advantage of this feature, I use a regression discontinuity research design to identify the causal effect of city treasurer selection methods on city borrowing costs. I apply this estimator to a newly available rich data set from cities in California combining over a decade of information on local referendums with annual measures of city borrowing costs.

My main results concern the treasurer appointive effect on city borrowing costs. I find that having an appointed rather than elected city treasurer reduces city borrowing costs by between 13% and 23%. The results imply that if all cities in California with elected treasurers were to appoint them, total borrowing expenditures would be reduced by \$20 million per year. These effects do not appear to be driven by other unobserved determinants of borrowing costs discontinuously changing at the threshold of referendum passage. In addition, I find little evidence that either fiscal policy or other costs of borrowing are affected by the method of selection for the city treasurer.

I also examine whether the appointive effect is primarily due to differences in *debt restructuring* policy or *debt issuance* policy choices. Reducing borrowing costs with debt restructuring policies requires active monitoring of public debt markets, as well as, the expertise to evaluate likely changes future market interest rates. Reducing borrowing costs with debt issuance policies, in contrast, requires the use of competitive debt sales that reduce payments to (potentially politically connected) financial intermediaries. As debt restructuring policies require more treasurer effort and expertise than debt issuance policies, understanding the channel for the appointive effect provides evidence on the importance of treasurer supplied inputs in explaining

⁵Several previous papers have used elections as sources of identification in regression discontinuity models. See for example, Lee (2008), Ferreira and Gyourko (2009), and Cellini, Ferreira, and Rothstein (2008). Lee and Lemeux (2008) and Angrist and Pischke (2009) provide excellent surveys of the regression discontinuity research design.

the central findings.

I separately measure debt restructuring and debt issuance policy choice differences by using cross-sectional debt issue level data containing information on the date of the debt issue, in addition to, the interest rate. Information on the date that the debt is issued is useful in disentangling the two types of policies, as debt issuance policies are chosen by the treasurer in office at the time the debt is issued. Thus, differences in debt restructuring policies can be measured by differences in borrowing costs for debt issued *before* the referendum as high interest rate debt issues are selectively retired. Similarly, differences in debt issuance policies can be measured by the appointive effect on debt issued *after* the referendum. The evidence indicates that the appointive effect is primarily due to debt restructuring, but not debt issuing policies.

More broadly this paper also contributes to recent literature on the effects of electoral accountability and leader selection on policymaking and public goods provision. Recent empirical work has demonstrated the important role for electoral accountability in policymaking and the provision of public goods in a variety of contexts (Besley and Case, 1995; List and Sturm, 2006; and Ferraz and Finan, 2007, 2008). It has also been shown that leadership change has an important effect on institutional and economic performance (Jones and Olken, 2005).

My results provide clear evidence that bureaucratic control of city debt management policy reduces city borrowing costs. Caution is required, however, in attempting to generalize these results beyond my context and sample. Even within my sample, effects may differ for cities not at the margin in passing an appointive treasurer referendum. Nevertheless, finding that bureaucratic control of debt management policy reduces city borrowing costs is an important result that provides empirical support for recent theoretical work emphasizing the comparative advantage of bureaucratic control for technical policymaking (Maskin and Tirole, 2004; Alesina and Tabellini, 2007, 2008; and Vlaicu, 2008).

The remainder of the paper is organized as follows: Section 2 describes the context and empirical approach. Section 3 describes the data and descriptive statistics. Section 4 presents the main results and validates the research design. Section 5 examines evidence of differences in debt restructuring and issuance policy choices between politician and bureaucrat treasurers. Section 6 concludes.

2 Context and Empirical Approach

2.1 City Treasurers, Policy Choices, and City Borrowing Costs

In this section I provide a brief overview of the operation of public debt markets, the role of city treasurers in city government, and debt management policies which affect borrowing costs.

There are three sets of participants in public debt markets: city issuers, financial intermediaries, and investors. The debt issuing process begins with the city deciding that a capital project (or other need) requires financing, financial intermediaries are then hired, the issue is then presented to the debt rating agencies, and finally the issue is sold to investors.⁶

City Treasurers City treasurers are delegated the tasks of managing public funds in accordance with the law (CMTA, 2001). They are primarily delegated the tasks of developing debt management, accounting, and cash management policies to implement legislative policy. The city treasurer influences city borrowing costs by their choice of debt management policies.⁷

Appointed and elected treasurers are likely to differ in the level of expertise and effort they supply to the policy task. In terms of expertise, the only requirements for a person to be an elected city treasurer is that they are a resident of the city, and usually, do not have a criminal conviction. In contrast, appointed city treasurers can be selected for their expertise in managing public debt. This difference often results in appointed treasurers having higher levels of education (often an MBA or MPP degree) than elected treasurers.⁸ In terms of effort, elected and appointed treasurers face very different returns to effort as the effect of policy performance on reelection for the politician and promotion for the bureaucrat are likely to be quite different. These effort differences crucially hinge on how well voters can observe policy performance by the politician and how city managers use policy performance to hire, fire, and promote bureaucrats

⁶For a more detailed discussion of the issuance laws, rules, and regulations which govern the process of public debt for cities in California see CDIAC (2006). For a more general discussion of public debt issuance and management by local governments see Joseph (1994) and Leonard (2004). See Feldstein and Fabozzi (2008) for an excellent and very thorough overview of the municipal debt market.

⁷These policies are sometimes chosen in a committee rather than by the city treasurer alone, but the city treasurer often plays a large role in setting the policy agenda regardless.

⁸I have attempted to directly collect information on individual city treasurer education levels from cities themselves under a Public Records Act request. Unfortunately, many cities only keep very limited records on the educational qualifications of their treasurers, and the resulting data set is too incomplete to be useful.

(Alesina and Tabellini, 2007).

City Borrowing Costs The cost that a city ultimately pays to finance a debt issue is composed of two factors: (1) the interest rate that investors demand to hold the debt and; (2) the spread between the interest rate spot price investors demand and what the city pays. The interest rate that investors require to hold the city debt depends principally on the default risk of a city.⁹ Aspects of the local economy and city fiscal policy affect default risk, and are reflected in the city debt rating. These factors can range from the diversity of the tax base, to whether city expenditure policies are sustainable, etc. In addition, other aspects of a debt issue such as the term length of the issue, and the principal amount affect borrowing costs. The characteristics of the debt issue are often determined by the nature of the project to be funded. However, the spread that a city pays above the spot market price for a debt issue depends on two debt management policies: debt issuance and debt restructuring.

Debt Issuance Policies Debt issuance policies primarily affect the spread that intermediaries receive. The key debt issuance policy choice is whether to sell the debt in a competitive auction, where sealed bids are submitted by underwriters and the lowest bid is chosen, or as a negotiated sale with a single underwriter chosen in advance of pricing. Competitive sales lead to lower borrowing costs for the issuer, but likely a smaller spread for the underwriter.¹⁰ Other debt issuance policy choices include when to issue the debt, and the characteristics of the debt issue such as term length etc.

Debt Restructuring Policies Debt restructuring policies are the second type of policies that can influence borrowing costs. City debt, much like conventional mortgages, can be refinanced. As market interest rates fluctuate according to many factors, refinancing when it is worthwhile to do so can result in lower average borrowing costs on city debt. The potential benefit of

⁹Municipal defaults are rare, but not unheard of. There were over 2000 defaults by cities and incorporated townships in the United States between 1939 and 1969 (Spiotto, 2008). Historically defaults have been due to provision of nonessential services, fraud and mismanagement, adverse local economic conditions, and natural or man-made disasters (Spiotto, 2008).

¹⁰The evidence is very clear on this difference in borrowing costs. See Robbins and Simonson (2007) and Simonson, Robbins and Helgeson (2001) for recent studies showing that competitive sales reduce borrowing costs for cities. Levitt (2009) criticizes non-competitive sales as 'pay to play' rewards for the campaign finance provided to elected officials by financial intermediaries

refinancing a debt issue is expressed nicely in the municipal finance saying: "While doctors bury their mistakes, in municipal financing, they are refunded [refinanced]." Spiotto, 2008 p. 707.

As Figure 1 demonstrates there is significant variability in the market interest rate for municipal securities. There are predictable patterns of demand for an issue depending on the timing other issues from the U.S. Treasury and other large issuers, national holidays, seasonal demand from investors, etc. Unexpected changes in monetary or tax policy may also affect the cost of borrowing.

A debt refinancing policy of responding to favorable market conditions can significantly reduce average borrowing costs. For example, in a typical year a city treasurer who sells a revenue anticipation note on a week in the top 10% of the interest rate distribution will pay a 22 percent higher interest rate than one who sells the note in the bottom 10%. While it is possible that refinancing may result in some characteristics of the debt issue the residents may value changing, the fact refinancing opportunities are primarily driven by the variation in market interest rates *over time* that all treasurers face suggests that changes in characteristics are likely to be second order.

2.2 Empirical Approach

The fundamental identification problem in generating unbiased estimates of a pure appointive effect on policy outcomes is that cities do not choose political institutions randomly. For a start, cities may well choose political institutions to maximize local social welfare. For example, as the benefit of a low cost debt management policy increases with the city debt level, cities with higher levels of debt may be more likely to choose treasurer selection methods that lead to the lower costs of borrowing. However, the size of the city debt is also likely to affect city borrowing costs anyway. In addition, many factors that influence both borrowing costs and the choice of political institutions are frequently unobserved. Therefore, a naive comparison of borrowing costs between cities with and without appointed treasurers may well lead to biased estimates of the appointive effect. Credible estimates require variation in the method of selection for city treasurers which is independent of other unobserved factors that affect city policy outcomes.

Ordinary Least Squares To provide a benchmark for the regression discontinuity (RD) analysis that addresses the issue of institutional endogeneity I first estimate the appointive

effect by ordinary least squares (OLS). The simple model I estimate can be expressed as,

$$(1) \quad Y_{it} = \delta \text{appointed}_{it} + X_i \beta + \omega_t + \epsilon_{it},$$

where i indexes the referendum and t indexes the year. The variable appointed_{it} is a dummy variable that takes a value of one if the treasurer is appointed and value zero otherwise. X_i is a vector of covariates from the 1990 census in the city where the referendum occurs, ω_t is a set of year fixed effects, and ϵ_{it} is the error term. The variables I control for in the X_i vector to capture variation in default risk across cities are: Income Per Capita, Population Size, Percentage White, Percentage Black, Percentage Less Than Seventeen, Percentage Greater Than Sixty-Five, Percentage College Graduate, Percentage High School Graduate, Mayor-Council Form of Government, and Employment-Population Ratio. The conditional mean difference in policy outcomes between appointed and elected treasurers is reflected by δ .

While it is possible to control for many easily measured city characteristics in the X_i vector there are many unobserved components of borrowing costs. For example, cities may differ in their budgetary institutions that can affect the level of debt and probability of default. Poterba (1994) provides evidence that budgetary institutions affect debt levels and the adjustment to fiscal shocks for US states. Other factors such as the strategic use of debt by politicians (Alesina and Tabellini, 1990) or high spending city legislative institutions (Coate and Knight, 2009) may lead to substantial debt accumulation and high borrowing costs. Differences in cities in these factors may well influence treasurer selection method choices in a city. In addition, poor performance by an elected treasurer may well lead city residents to push for a change in the treasurer selection method. To address concerns about correlations of treasurer selection methods with important unobservables I also estimate a regression discontinuity specification.

Regression Discontinuity Estimation Strategy To address the concern that treasurer method of selection may be endogenous related to other determinants of city borrowing costs I use a regression discontinuity (RD) strategy to compare cities where an appointive treasurer referendum *barely passed* to cities where an appointive treasurer referendum *barely failed*.

To understand how this strategy addresses concerns about the endogeneity of political institutions consider a city where residents vote on treasurer appointive referendum i , and the referendum receives vote share s_i . A referendum passes if it receives a vote share of greater than 0.5 so an indicator variable for the referendum passage can be expressed as: $\text{pass}_i = 1(s_i > 0.5)$.

We can then express a post-referendum city policy outcome (Y_i) as,

$$(2) \quad Y_i = \alpha + \delta pass_i + \epsilon_i,$$

where δ is the causal effect of an appointed treasurer relative to an elected treasurer on the policy outcome, and ϵ_i represents all other determinants of Y_i . This simple model would yield an unbiased estimate of the appointive effect if the referendum outcome, $pass_i$, was uncorrelated with the other determinants of policy, ϵ_i . However, as many of the unobserved determinants of borrowing costs are likely reflected in voter preferences there are good reasons to suspect that the referendum outcome may well be correlated with other determinants of borrowing costs. If so, a simple regression of model (2) will yield a biased estimate of δ . However, one can identify the causal effect of referendum passage by using the referendums that are barely rejected to form a counterfactual for the referendums that barely pass. Lee (2008) demonstrates that this strategy provides quasi-random variation in referendum outcomes, because for narrowly decided referendums, whether the referendum passes is likely to be determined by pure chance as long as there is some unpredictable component of the ultimate vote.

The implementation of the RD strategy I focus on here uses all of the referendums but controls the variation coming from non-close referendums using flexible controls for the vote share.¹¹ Assuming that the conditional expectation of the unobserved determinants of Y_i given the realized vote share, is continuous, we can approximate it by a polynomial of order g .

To implement my regression discontinuity approach I estimate models of the following form for each policy outcome, Y_{it} ,

$$(3) \quad Y_{it} = \delta pass_{it} + P_g(s_i) + X_i\beta + \omega_t + \epsilon_{it},$$

where i indexes the focal referenda and t indexes the year. The variable $pass_{it}$ is a dummy variable that takes a value of one in every year after the referendum passes and value zero otherwise. $P_g(s_i)$ is a polynomial function of the referenda vote share, s_i , of order g . X_i is a vector of covariates from the 1990 census in the city where the referendum occurs, ω_t is a set of year fixed effects, and ϵ_{it} is the error term. Again, the variables I control for in the X_i vector to capture variation in default risk across cities are: Income Per Capita, Population Size, Percentage White, Percentage Black, Percentage Less Than Seventeen, Percentage Greater Than Sixty-Five, Percentage College Graduate, Percentage High School Graduate, Mayor-Council Form of

¹¹The RD design can be estimated parametrically or non-parametrically, focusing on only close elections or the larger sample of all elections. I follow a parametric approach using all referendums because it allows straightforward hypothesis testing, and precise estimates. See Lemeux and Lee (2008) for a detailed comparison of alternative approaches to estimating RD models.

Government, and Employment-Population Ratio. Thus, the appointive effect, δ , is estimated controlling for the percent voting in favor with a flexible polynomial functional form.

The central identifying assumption for δ to estimate the causal effect of treasurer method of selection on policy outcomes is that all relevant factors *vary smoothly* at the referendum passage threshold. We need this assumption for city policy outcomes after a narrowly failing referendum to form an adequate counterfactual for city policy outcomes after a referendum narrowly passes. While this assumption is not testable directly, I examine whether city policy outcomes are smooth, conditional on the polynomial in vote share, in the years prior to referenda to assess its plausibility.

To estimate (3) I use all the post referendum data for a focal referendum in a given city. Observations are uniquely identified by the city and the date of the referendum. For the cities that have more than one referendum the same calendar year observation is used more than once.¹² To address this issue I follow Cellini, Ferreira, and Rothstein (2009) and cluster the standard errors at the city level to account for the dependence created by multiple city-year observations and any serial correlation in the error term due to persistent debt management policy.

A few other estimation details are worth noting. First, as Porter (2003) argues that odd polynomial orders have better econometric properties, and the preferred order of the polynomial regression is still open to debate in the RD literature, I present results with both linear and cubic vote share polynomials. Second, I code the timing of the change in city treasurer selection method as occurring two years after the referendum, as the referendum to change the city treasurer selection method takes effect after the incumbent elected treasurer completes their two year term.

It is important to be clear on how my main outcome of interest, the average interest rate paid on the stock of city debt after the referendum, measures city debt management policy choices. Because debt restructuring policies affect the interest rate on the stock of city debt and the debt issuance policies affect the interest rate on the flow of city debt, the dynamics of the effect of treasurer selection methods on the average interest rate depends on which policy choice is primarily affected. As I have relatively few years of data after a number of the referendums my analysis likely captures the short run effect of a change in treasurer selection methods. This

¹²There are four cities that put multiple referendums to the voters, two of which fail the first time and subsequently pass, in the sample period.

short run effect may well differ from a longer-run effect depending on which policy choice changes with treasurer selection method. Debt restructuring policies are likely to result in a larger short than long run effect, as an effective treasurer who inherits a stock of expensive debt from less effective treasurer could reduce average interest rates quite quickly by refinancing the debt. In contrast, as the average debt issue matures over 20 years after the debt is issued, and new debt is typically issued every other year, the effect of a change of debt issuance policy may only have a small effect on the average interest rate on the stock of city debt in the short run. In any case, changes in the average interest rate on the stock of city debt are informative about the short run effect of treasurer selection methods on debt management policies.

2.3 Treasurer Appointive Referendums

Historically, all general law cities in California began with elected treasurers. The State Code allows for a general law city to make the treasurer position appointive if city residents vote for this change, and many cities have done so. Referendums for the appointment of city treasurers are placed on the ballot by city councils in accord with procedures outlined in the California State Code.¹³ The exact text of the measure is given in the California State code as, “*Shall the office of city treasurer be appointive?*”, with the words “*yes*” and “*no*” following the question. If greater than 50% of the voters choose “*yes*” the referendum is carried. If the referendum is carried, the elected position is filled with a candidate of the city council’s choosing on the expiration of the current treasurer’s term.

I list the treasurer referendums in Table A1.¹⁴ During my sample period there are 36 appointive treasurer referendums, occurring in over 20% of the general law cities in California. Ten of the appointive treasurer referendums pass. Many referendums fail, likely because many

¹³Local ballot measures mostly concern local issues of land use, governance, and safety (Gordon, 2004). As Gordon (2004) notes, the California local Initiative and Referendum process was enacted in 1911 in response to the perceived influence of special interests, in particular the railroads. Currently the initiative process is available in all of California’s 475 cities. All general law cities follow the procedures outlined in the state code for an initiative to become eligible for the ballot. While charter law cities have the option of having their own specific set of requirements that do not directly contravene the state code, most follow the set of requirements outlined in the state code. About 17% of local initiatives cover local government structure and organization. The local initiative process is more common in large, growing and economically diverse cities with larger public sectors; however, local income and other local political institutions play little role. Local initiatives can appear on the ballot in state and local (concurrent) or local-only (non-concurrent) elections. For an overview of the local initiative process in cities throughout the United States, see Matsusaka (2003).

¹⁴I discuss details of the exact data source and sample selection below in the data section.

Americans view bureaucrats negatively, as Wilson (1989) and others have noted. The distribution of the vote shares is plotted in Figure 1. This distribution indicates that many of the referendums are quite close to the passing threshold, which is particularly useful for my RD estimation strategy to be compelling.

Whether a referendum that would be *very close* to passing does, indeed, barely pass (or conversely, barely fail), is unlikely to be driven by changes in the performance of the local economy, fiscal policy, other city institutions, or pressure from interest groups. Referendums may pass or fail for a variety of reasons. The work of Bowler and Donovan (1998), Gerber (1999), and Nicholson (2005) has shown that many features of ballot measures unrelated to the actual policy issue being voted on can affect their passage. For example, the salience of an issue on the national or state agenda, or cues in the question text, can determine the passage of a ballot measure. Marginal referendums may pass or fail depending on the public's view of bureaucrats at the time of the election, how other issues in the election crowd-in or crowd-out attention to the measure, the views of the swing voters who turn out to vote, etc. Exogenous shocks to who turns out to vote (perhaps due to weather conditions), or media coverage of national political or bureaucratic corruption scandals, could easily tip the balance.

3 Data

The empirical analysis in this paper studies the effect of treasurer appointive referendum passage on borrowing costs. In general, data on vote shares and the question text for local referendums are not compiled by state agencies. Fortunately, the newly available *California Elections Data Archive* (CEDA) database compiled by the Center for California Studies in cooperation with the California Secretary of State contains the key information required for this study. This database contains information on the date, content, and vote outcomes for all local initiatives and referendums appearing on the ballot in California cities from 1995 until 2006.¹⁵ I obtain my measure of whether an appointive city treasurer referendum passes and the vote share from this source.

I match the referendum data to data on city borrowing costs obtained from the *City Financial Transactions Report* (CFTR) data for fiscal years 1992 to 2008 for each city. The CFTR

¹⁵As the treasurer selection methods change two years after the referendum is voted on I am unable to use any of the post 2006 referendums.

data comprise a uniquely detailed database with extensive coverage of many financial variables collected annually by the California State Controller for each city. All of the current 475 cities in California are required to file a report. More than 99% of cities file a financial transactions report in any given year.¹⁶ I use variables on total interest expenditure, total debt outstanding, total expenditures, and total local source revenues, all from the CFTR data. I construct my measure of city borrowing costs, the average interest rate paid on the stock of city debt, by dividing total interest expenditure by total debt outstanding. Importantly, this interest rate measures the borrowing costs that cities actually pay, including both the gross margin financial intermediaries charge, as well as, payments to investors. To match both data sources together I code the passage of a referendum based on the fiscal year as defined in the CFTR data.¹⁷

Together the data from CFTR and CEDA form the panel of California cities that I use in my central analysis. I match in data from two further sources. First, I match in data from the 1990 Decennial Census to measure population, demographic, and economic characteristics of each city. Second, I match in data from the 1992 Census of Governments on city government organization. I use this dataset to measure the baseline method of selection of the city treasurer position.

For the last section of the paper I also use data on individual debt issues matched to cities from the CFTR data in 2008. This data includes the interest payments, outstanding balance, type of debt, and term for each issue. I am only able to analyze the issue data for the 2008 cross-section as data from earlier years contain many missing values for the interest expenses.¹⁸

I construct my analysis sample in the following way. I first use California cities that appear in the 1992 Census of Governments as my base population of cities, which is 456 cities. The few cities that incorporate after 1992 are not in the sample. I also drop charter cities, as the referendum process and question as specified applies only to general law cities, and charter cities are free to redefine the duties and role for the city treasurer.¹⁹ This gives a sample of 361 cities. I then match these data to 1990 Decennial Census, CFTR, and CEDA data by city. I drop the

¹⁶Government Code section 12463 directs the California State Controller to annually compile and report to the public the financial transactions of all California cities.

¹⁷The city fiscal year runs from July to July, so for example I code referendums occurring between July of 1995 and the end of June 1996 as occurring in the 1996 fiscal year. I use the 1996 fiscal year as the first year of the sample as I do not have referendum data for the entire 1995 fiscal year.

¹⁸Interest expenses are missing for over 65 percent of the pre-2008 issue observations, but only 5 percent for the 2008 issue observations.

¹⁹General law cities follow the local government procedures and policies in the California State Code, whereas charter cities may adopt any policies and procedures which do not directly contradict state law.

cities without positive reported interest costs in each year, as a number of smaller cities do not have outstanding debt in every year. I also drop the few observations with an interest rate of greater than 25%; as observed interest rates on debt issues are never above 18% these are likely due to coding errors. My final sample analysis sample contains 203 cities for my full sample of cities, with the 36 treasurer appointive referendums taking place in 31 referendum cities for the referendum sample.

Table 1 presents descriptive statistics for all of the cities in my sample. Columns (1) and (2) show the means and standard deviations computed over all city-year observations dividing cities by treasurer method of selection.²⁰ The comparison yields a number of interesting results. First, cities with elected treasurers pay higher borrowing costs. The borrowing costs in cities with elected treasurers more than 15% greater than those in cities with appointed treasurers. Cities that have elected and appointed treasurers are also quite different in other ways. Cities with elected treasurers tend to have more debt, and are more likely to have directly elected mayors and clerks. Cities with elected treasurers also have lower levels of income per capita, and a less educated population. Many of these differences are likely to make cities with elected treasurers subject to more default risk, leading to higher borrowing costs for elected treasurer cities regardless of treasurer method of selection. Thus, the descriptive statistics indicate that cities with appointed treasurers pay lower borrowing costs, but differ on key observable measures which are likely to affect borrowing costs independently of treasurers' method of selection.

As my main analysis focuses on the referendum cities sample that have elected treasurers in 1992 and subsequently vote on a treasurer appointive referendum a natural question to ask is whether it is representative of cities with elected treasurers. I present descriptive statistics comparing cities with an elected treasurer in 1992 that do and do not subsequently vote on a referendum in Table A2. By and large the cities are very similar, though the cities that vote on a referendum have somewhat lower levels of economic activity.

²⁰Treasurer method of selection in a given year is defined from the baseline method of selection variable and any subsequent passing treasurer appointive referendums.

4 Results

4.1 Main Results

Column (1) of Table 2 presents the baseline OLS estimates of δ from fitting equation (1). This estimate indicates a negative relationship between having an appointed city treasurer and borrowing costs. Conditional on city government and economic characteristics, an appointed city treasurer is associated with a reduction in borrowing costs of 13% of the baseline. As there are significant concerns about whether cross-sectional differences in, or even changes in, city treasurer method of selection are independent of unobserved determinants of borrowing costs, I next present estimates using the variation in city treasurer method of selection due to narrowly passing referendums.

In columns (2) and (3) of Table 2, I present the estimates of δ from fitting two versions of equation (3). In column (2) I present the RD estimates with a linear polynomial in vote share. This estimate indicates that appointed city treasurers do indeed cause city borrowing costs to be lower. The effect appears to be both statistically and economically significant. The passage of an appointive treasurer referendum reduces borrowing costs by 19% of baseline. The effect is also economically large. As interest payments in cities with elected treasurers account for nearly \$43 per capita, appointive treasurers reduce interest expenditure by more than \$8 per capita or \$250,000 for the city in total. Thus, the results imply that if all 137 cities with elected treasurers in the baseline period were to switch to appointed city treasurers total borrowing expenditures would fall by over \$30 million.

The baseline RD results in column (2) of Table 2 are depicted visually in Figure 3. The graph clearly shows the positive vote share-interest rate gradient, and the discontinuous drop in interest rates that occurs at the referendum passage threshold.²¹

While the linear vote share specification seems to capture the distribution of borrowing costs fairly well, one might still be concerned that non-linearities in the interest rate-vote share relationship might be driving the result in column (2) of Table 2. To address this potential concern I also estimate RD models with the more flexible cubic vote share polynomial. I present

²¹In Figure 3 I plot the raw data of the interest rates observations and the RD linear model without any controls for transparency. The comparable regression results for the linear RD model without the 1990 census controls are very similar to those in Table 2. The coefficient on the pass dummy is -0.89, with a city-clustered standard error of 0.34.

the results of the cubic polynomial RD model in column (3) of Table 2. The estimated effect is slightly larger than in column (2) translating into an effect of 23% of baseline, but remains statistically significantly different from zero at the 5% level.

The fact the RD estimates imply that borrowing costs are more responsive to treasurer method of selection than the OLS estimates could indicate that cities with higher levels of unobservable default risk are more likely to have appointed treasurers. Another possibility is that unobserved default risk has little relationship with treasurer method of selection, but the short run effect of an appointive city treasurer is larger than the effects in the longer run. The short run effect of switching treasurer appointive methods may be larger than the long run effect if elected treasurers fail to take advantage refinancing opportunities so that the scope for refinancing to affect borrowing costs on average is especially large in the short run.

4.2 Robustness Tests

Identification of the effects of institutions is challenging because any factor that affects both the cost of borrowing and institutional reform in a city may induce spurious correlation. For example, one might be concerned that cities with particularly ineffective elected treasurers implement policies that lead to a city paying higher borrowing costs. If having an ineffective elected treasurer leads citizens to narrowly pass an appointive treasurer referendum, we will estimate a negative effect where none exists. In this context, however, I believe that this issue is unlikely to be a serious problem. First, the passage of the referendum by a small margin is unlikely to be affected by significant differences in the effectiveness of a given treasurer, city fiscal policy, or other institutional characteristics. More importantly the central identification assumption is based on the smoothness of other confounding factors at the referendum passage threshold. Mean differences between cities that do and do not pass a referendum in their economic fundamentals, government, and other institutions do not contribute to identification. One might still be concerned that the narrow passage of an appointive treasurer referendum may be related to discontinuous changes in other key confounding variables. I next estimate a number of models to address this potential concern.

City Policy Outcomes Prior to Referendum Voting One advantage of having panel data on city policy outcomes is that I can test for a discontinuous jump in city borrowing costs and other policy outcomes at the referendum passage threshold before the referendum

is actually voted on. If the narrow passage of a city treasurer referendum simply reflects a discontinuity in unobservable determinants of city borrowing costs my identification strategy could be threatened.

In Table 3 I estimate versions of equation (3) using only pre-referendum data on city financial outcomes. In the first panel of Table 3 I present the results for borrowing costs prior to the referendum vote. The results indicate that no statistically significant discontinuity exists in borrowing costs at the threshold of passage of the appointive referendum prior to the vote. The relationship between prior interest rates and future referendum vote share and passage is shown graphically in Figure 4. The figure shows clearly the lack of a significant discontinuity in interest rates prior to voting on the referendum.²² The fact that there is little evidence of a significant discontinuity in the main outcome prior to the referendum lends credence to the identification assumption.

I next examine whether there is any evidence for a discontinuity in city fiscal policy outcomes before the referendum is voted on in Panels B to D in Table 3. In general, the results of these regressions show little statistically significant relationship between pre-referendum fiscal policy outcomes and future referendum outcomes. Moreover, even the sign of the point estimates are highly sensitive to the order of the vote share polynomial. The one exception is that the coefficient on future referendum passage on Total Local Source Revenue is statistically significant at the 10% level with a linear polynomial in vote share. However, this discontinuity is highly non-robust, as the estimate switches sign and is no longer statistically significant when higher order polynomial terms are included. The results of this exercise suggest there is little reason to be concerned the main results above are driven by pre-existing non-linearities in city debt management or fiscal policy.²³

Other City Institutional Changes The next threat to identification I consider is that the narrow passage of a treasurer referendum is tied to other institutional changes or reforms that might affect a city's cost of borrowing. As noted by Acemoglu and Robinson (2005), institutional

²²The visual depiction of the data in Figure 3 does demonstrate that some extreme outlier interest rate observations do exist. In an unreported analysis I have also estimated the models in Table 3, Panel A excluding interest rates greater than 8%. The coefficient and standard error on the pass dummy from this analysis are nearly identical to those reported in Table 3.

²³I also show in Table A3 that there is little evidence for a discontinuity in the control variables in the X_t matrix from the 1990 census at the referendum passage threshold. As less than 10% of the coefficients are statistically significant at the 10% significance level the results reflect what we would expect from pure chance.

changes are often bundled together. As a city council decides whether to place an appointive treasurer referendum before voters, this decision may be correlated with changes other institutions that might also affect the costs of borrowing such as city budgetary institutions. An example of this would be the adoption of balanced-budget rules. In this case, the estimated treasurer appointive effect may simply reflect other correlated changes in city government budgetary institutions, rather than changes in the method of selection of the city treasurer.

To shed light on this issue I examine whether the narrow passage of an appointive *clerk* referendum has a similar effect to narrow passage of the treasurer appointive referendum. This specification is informative for two reasons. First, as shown in Table 2 cities with elected treasurers are far more likely to have elected clerks. This fact suggests that unobservable factors that lead to the adoption of an appointive treasurer institution may also lead to the adoption of an appointive clerk institution. Indeed, many cities place appointment referendums for city clerk and city treasurer positions on the ballot simultaneously and even structure the referendum as vote to make both positions appointive. Second, as city clerks have little to do with debt management policies we would expect to see little change in borrowing costs if the treasurer appointive effect actually reflects differences in debt management policy choices by city treasurers. For these reasons clerk appointive referendum make a compelling test of whether other unobserved institutional changes are responsible for the main results.

I present the results of the appointed clerk models in Table 4. I first estimate a model similar to (1) by OLS where the appointed treasurer dummy variable is replaced by the appointed clerk variable. I then estimate an RD model similar to (3) where the referendum pass and vote share variables are from the appointive clerk referendum rather than the appointive treasurer referendum. The sample for the RD models includes all post-referendum data for the 20 cities that put an appointive city clerk referendum to the voters.²⁴

In Table 4 I present OLS and RD estimates the appointive clerk effect on borrowing costs. In the first column of the table we see that the OLS estimate of the coefficient is negative, but not statistically significant at conventional significance levels. The point estimates switches sign in the linear RD model in the second column of Table 4, but remains statistically insignificant.

²⁴A number of cities either put a treasurer appointive referendum and a clerk appointive referendum to the voters in the same election, or put a single referendum question to the voters that would switch both positions to be appointed. To the extent that appointive clerk and treasurer referendum are voted on jointly we would expect that the passage of a treasurer or clerk referendum would have the same measured impact. However, as the list of treasurer referendums in Table A1 shows, in practice only a few of the treasurer appointive referendums are also clerk appointive referendums.

In the last column of Table 4 we see that the point estimate again switches sign, and again remains statistically insignificant. The fact the point estimates are not statistically significant provides further reassurance that the city treasurer appointive effect in Table 2 is causal and is not simply due to other unobservable institutional changes.

Narrow Margin of Victory Sample The central identification assumption in my RD analysis is that borrowing costs in cities that narrowly fail to pass a referendum form a valid counterfactual for those that narrowly do pass a referendum. Thus far I have used all the referendums available to conduct my analysis and relied on the vote share polynomial to smooth other unobserved determinants of borrowing costs. While the tests for smoothness prior to the referendum support this assumption, it is natural to ask whether the appointive effect is similar in the smaller sample of referendums that barely pass and barely fail. This sample is often termed the 'RD sample' (Angrist and Pischke, 2009). I estimate my models on this RD sample next.

In Table 5 I present estimates of RD specifications for the sample of twenty referendums that either pass or fail by less than 10 percent. The first column in Table 5 shows that the passage of treasurer appointive referendum in the linear RD specification reduces the city borrowing costs. The point estimate is substantially larger than the models in Table 2. However as the baseline level of borrowing costs is also higher in the narrow margin of victory sample the implied effect is about 28% of baseline, only slightly larger than found in the full referendum sample. The fact that the effect size is similar in the RD sample to the effect in the full referendum sample in Table 2 is comforting. The second column of Table 5 presents the results of the RD specification with a cubic polynomial estimated with the narrow margin of victory sample. Again, the results reveal that treasurer appointive referendum passage leads to lower borrowing costs. The magnitude of the estimate is even larger than that in column (1) of Table 5.

4.3 Other Policy Outcomes

Fiscal Policy Outcomes The results thus far have indicated that appointed city treasurers implement debt management policies that result in lower borrowing costs. I next turn to the question of whether treasurer selection method also indirectly affects fiscal policy. These effects are of interest for two reasons. First, it is interesting to know whether narrowly focused changes in political institutions have broader effects across multiple policy areas, as policymakers controlling different policy levers may respond to one another. Second, voters might hold treasurers

accountable by observing the indirect effects of treasurer policy choices through changes in fiscal policy, rather than debt management policy per se. For this form of indirect accountability (with appropriate attribution) to be effective fiscal policy would need to respond to changes in debt management policy.

In Table 6 I present RD estimates the appointive treasurer effect for fiscal policy. In the first panel of the table I examine the appointive effect on total city expenditure. In this panel we see that the while point estimate of the coefficient is positive, it is not statistically significant at conventional significance levels in both RD specifications. That there is little evidence of an effect on total expenditure likely indicates the reduction in borrowing costs is spent on other public goods. In the second panel of Table 6 I look at the appointive effect on locally generated revenue. In the first column the estimate is positive and statistically significant at the 10% level. In the second column the point estimate is similar, but no longer statistically significant. The lack of a negative effect suggests that the reduction in borrowing costs is not passed on to taxpayers. The weak evidence of an increase in revenue could be due to an effect of appointive city treasurers on tax collection rather than fiscal policy per se. The last panel in Table 6 examines the appointive effect for the stock of total debt outstanding. Again, the results show little evidence of an appointive city treasurer effect on total debt outstanding.

Debt Issuance and Management Costs In this section I investigate whether other borrowing or administrative costs are affected by treasurer method of selection, as interest rates are not the only cost cities pay to borrow. I examine whether a city treasurers' method of selection also affects other costs directly related to the issuance of debt. Whether other costs of debt issuance respond to treasurer method of selection is important for understanding the effect of treasurer method of selection on *total* borrowing costs.

I consider two additional costs. I first examine whether cities with appointed treasurers spend more or less on per issue fees to private sector debt issuance consultants. The effect could plausibly go in either direction. Effective city treasurers may be able to substitute their effort and expertise for that of a consultant, reducing city expenditure on advisory services. On the other hand, effective city treasurers may be effective because they select higher quality financial advisors whose services may be more costly. It is also possible that private sector debt consultants are able to extract rents from ineffective city treasurers who are not aware of the services they really need or the price they should be willing to pay.

In the first panel of Table 7 in columns (1) and (2), I present the results of estimating a RD model similar to equation (3) with the outcome of debt issuance consultant expenditure. The results clearly show little statistically significant effect of appointive treasurer referendum passage on expenditure on debt consultants.

Second, I also examine whether the cost of government administration in the city treasurer department increases with an appointed city treasurer. Department costs might change if the appointive city treasurers' effect is due to hiring more or better quality staff in the finance department, or spending more on the city treasurer position itself. In practice, the effect on spending from reallocating the city treasurer position from a politician to a bureaucrat may well be small. Many small cities reallocate the debt management task to the finance director in response to the ballot measure passing, and do not create additional positions.

In the first panel of Table 7 in columns (1) and (2), I present the results of estimating a RD model similar to equation (3) with the outcome of total expenditure on general government administration. The results are mixed. The linear RD estimate in column (1) indicates that appointive city treasurers do significantly increase general government administration expenditure. However, the results with a cubic polynomial in column (2) indicate that the impact of an appointive treasurer on government administration is not statistically significant. Thus, the results in Table 7 provide little consistent evidence of strong appointive city treasurer effect on the cost of government administration.

5 Debt Restructuring or New Debt Issuance Policies?

The results presented thus far indicate that appointed treasurers reduce city borrowing costs. What explains these findings? In this section, I utilize highly detailed issue level data from the 2008 cross-section to examine evidence on whether the appointive effect is primarily due to debt restructuring or debt issuance policies. I first examine whether appointed and elected city treasurers' choose different debt restructuring policies. I then consider whether appointed and elected treasurers choose different debt issuance policies.

Understanding the channel for the appointive effect allows us to assess whether the effect can be interpreted as reflecting public official performance. As both treasurer effort and expertise are required to restructure existing debt at a lower cost, debt restructuring is closely related

to treasurer effort and skill inputs. It is possible that refinancing may result in some characteristics of the debt issue the residents may value changing, the fact refinancing opportunities are primarily driven by the variation in market interest rates *over time* that all treasurers face suggests that changes in characteristics are likely to be second order. Thus, an interpretation of borrowing cost reductions due to refinancing as public official performance seems warranted. In contrast, the central policy choice affecting the cost of borrowing for new debt is the method of sale, which seems more closely related to catering to special interest lobbying than to the inputs supplied by a city treasurer.

5.1 Measuring Debt Restructuring and Debt Issuance Policies

To separately measure debt restructuring and new debt issuance policies one would ideally use data on the flow of debt issues over time. Unfortunately, I only have access to a single cross section of debt-issue level data that reports interest rates reliably. The debt issue level data available as prior to the 2008 cross-section does not record an interest rate for the majority of the records.²⁵

I am able to separately measure debt restructuring and debt issuance policies indirectly by examining how the appointive effect depends on when the debt was issued. The test is based on the idea that a city treasurer's debt issuance policies can only affect the interest rates on the debt issued after they take office. Debt issuance policies will then only affect the interest rate on debt issued *after* the referendum. In contrast, the appointive effect on debt issued prior to the referendum must be due to the selective retiring of expensive debt through refinancing. Debt restructuring policies will then only affect the interest rate on debt issued *before* the referendum.

I again use my regression discontinuity approach to estimate the effect of treasurer methods of selection on debt issuance and debt restructuring policies. I estimate models of the following form for the interest rate, I_{ij} ,

$$(4) \quad I_{ij} = \delta pass_{ij} + P_g(s_i) + X_i\beta + \epsilon_{ij},$$

where i indexes the referendum and j indexes the issue. The variable $pass_i$ is a dummy variable that takes a value of one if a referendum passes in a prior year and value zero otherwise. $P_g(s_i)$ is a polynomial function of the referenda vote share, s_i , of order g . X_i is the same vector of

²⁵As the debt issue data is for the fiscal year 2008 it ends on June 30 2008, so the interest rates on these issues are not affected by the subsequent financial crisis in fall 2008.

covariates from the 1990 census in the city where the referendum occurs as in model (1), and ϵ_{it} is the error term. Thus, the appointive effect, δ , is estimated controlling for the percent voting in favor with a flexible polynomial functional form, as usual.

5.2 Results

The results of estimating (4) are reported in Table 8. I first present the results for all debt issues, and then turn to estimating on the models on the samples with: (1) all pre-referendum debt issues, to measure the debt refinancing policy response and (2) all post-referendum debt issues, to measure the debt issuing policy response. I also estimate models where I control for the type term length of the debt issue in columns (2), (4) and (6). The results in columns (1) and (2) demonstrate a similar appointive effect as noted above, about 25% of the baseline interest rate, though they are less precisely estimated.

Debt Restructuring The results from estimating equation (4) for the debt issued *before* the referendum are presented in columns (3) and (4) of Table 8. These results indicate that differences in city treasurer methods of selection cause differences in debt restructuring policies. The coefficient estimates are quite large and statistically significantly different from zero at the 5% level. The magnitude as a percentage of baseline is substantial, representing an effect of over 40% of baseline.²⁶

It is also important to note that the results hardly differ when debt characteristics are included as controls. This is important as attributes of a public debt issue other than the interest rate may affect resident welfare. For example, long-term debt with a fixed interest rate could be more valuable to risk averse city residents than short-term or variable interest rate debt. Thus far I have discussed borrowing costs as if lower borrowing costs are a public good, however if borrowing cost reducing are simply due to changes in other debt attributes residents dislike welfare interpretations are difficult. The fact that the results are so similar with and without debt characteristic controls indicates that the appointive effect is not primarily due to changes in debt attributes residents may value. Thus these results suggest that lower borrowing costs from refinancing can be interpreted as welfare improving. In any case, the debt restructuring

²⁶The baseline level of interest rates may not be the relevant counterfactual as the highest interest debt is most likely to be refinanced. The effects are still quite large compared the maximum level of interest rates in the sample, representing a response of over 20%.

response is substantial and provides support for a public official performance interpretation of the appointive effect.

Debt Issuance The results of estimating equation (4) for the debt issued *after* the referendum are presented in columns (5) and (6) of Table 8. These results provide little support for debt issuance response. The point estimates in both columns are actually positive and not statistically significantly different from zero. Thus, there is little evidence that appointed treasurers choose a different debt issuance policy.

In sum, the results shed some light on how differences in policy choices between elected and appointed treasurers account for the appointive effect. Appointed treasurers are more likely to restructure expensive debt, but not issue new debt at lower cost. The fact that the primary debt management policy difference between appointed and elected treasurers is debt restructuring lends further credence to the results above. As the median term length of a debt issue in the sample is 21 years there is little scope for debt issuance policy to affect the average interest rates on the stock of city debt in the short run. In contrast, the scope for the refinancing of expensive debt to have an especially large effect on average interest rates on the stock of city debt shortly after the method of selection of the treasurer switches is far greater.

6 Conclusion

This paper has documented that there are substantial effects from bureaucratic control of city debt policy: assigning the debt task to a bureaucrat reduces city borrowing costs. The results indicate that appointive treasurers reduce a city's cost of borrowing by 13 % to 23%. The appointive effect is also economically significant. The results imply that if all cities in California with elected treasurers were to appoint them, total borrowing expenditures would be reduced by more than \$20 million per year. Thus, this paper has demonstrated that alternative political institutions do indeed cause meaningful differences in policymaking.

I also present evidence that the appointive effect is primarily due to different debt restructuring, not different debt issuance, policy choices. As both treasurer effort and expertise are required to restructure existing debt at a lower cost, the evidence for a debt restructuring channel indicates an important role for treasurer supplied inputs in explaining the findings. In addition, as the appointive effect seems to have little to do with changes in attributes of the debt issue

interpreting the appointive effect as reflecting better public official performance seems to be warranted.

There are several implications for future research. First, it would be interesting to examine whether similar differences in policymaking are found for less technical policy choices. The theoretical arguments noted above indicate politicians likely have a comparative advantage in less technical policy areas. It would be interesting to examine empirical evidence on whether or not this is the case. Second, as highly detailed data on policy maker expertise are unavailable for city treasurers, it would be interesting to examine how differences in expertise affect policy choices and performance in a context where such measures are available.

The results of this study also have broader implications for the organization of public good provision. They suggest, for example, that the division of policymaking tasks typical in many advanced democracies, with appointed officials conducting the more-technical monetary and regulatory policy, and elected officials conducting the less-technical fiscal policy, is appropriate. Efforts to improve governance in developing countries may well be enhanced by emulating the division of policymaking tasks in advanced democracies.

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TABLE 1: Descriptive Statistics, by City Treasurer Appointive Status

	Treasurer Position Appointed (1)	Treasurer Position Elected (2)	(1)-(2) t-stat [p-value] (3)
<u>(1) Outcome:</u>			
Interest Rate Paid on Total Outstanding Debt (%)	2.17 (1.85)	2.57 (1.93)	-3.03 [0.003]
<u>(2) City Finances (2000 \$):</u>			
Total Local Source Revenue per capita	637 (1225)	646 (1075)	-0.07 [0.944]
Total Expenditure per capita	1317 (1789)	1561 (2557)	-1.10 [0.273]
Total Debt Outstanding per capita	1178 (1813)	1691 (3189)	-2.01 [0.046]
<u>(3) City Government Characteristics:</u>			
Mayor-Council Form of Government	0.09 (0.29)	0.21 (0.41)	-2.72 [0.007]
Appointed City Clerk	0.91 (0.29)	0.12 (0.33)	21.88 [0.000]
Fraction with Full-Time Elected Officials	0.03 (0.18)	0.01 (0.08)	1.63 [0.104]
<u>(4) City Economic Characteristics:</u>			
City Population	34416 (38509)	31104 (32416)	0.75 [0.453]
Per Capita Income (1990 \$)	17521 (10133)	14519 (6835)	2.98 [0.003]
Fraction College Graduate	0.23 (0.17)	0.17 (0.11)	3.67 [0.000]
Employment-Population Ratio	0.46 (0.09)	0.44 (0.08)	1.85 [0.065]
Number of Observations	2228	1894	

Notes: Source: Author's calculations using data from California CFTR, and CEDA data from 1992 to 2008, 1990 Decennial Census, and 1992 Census of

Government data. The unit of observation is city-year. The main entries in column (1) present the mean of the selected variables for cities with appointed treasurers. The main entries in column (2) present the mean of the selected variables for cities with elected treasurers. The standard deviations of the selected variable are presented in parenthesis in columns (1) and (2). The main entries in column (3) present the test statistics for a test of differences in means between column (1) and (2), with the p-value of the test presented in square brackets. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments and positive interest expenses in each year. All monetary values are expressed in 2000 \$ unless noted otherwise. The variables *Interest Rate Paid on Total Outstanding Debt*, *Total Local Source Revenue per capita*, *Total Expenditure per capita*, and *Total Debt Outstanding per capita* are based on data from the California City Financial Transactions Reports (CFTR). The variables *Mayor-Council Form of Government*, *Appointed City Treasurer*, *Appointed City Clerk*, and *Full-Time Elected Officials* are based on data from the 1992 Census of Governments. The variable *City Population* is based on data from the California E-1 Population Tables. The variables *Per Capita Income*, *Fraction College Graduate*, and *Employment-Population Ratio* are based on data from the 1990 Decennial Census. Definition of the variables: *Interest Rate Paid on Total Outstanding Debt (%)* is the ratio of total interest expenses divided by total debt outstanding; *Total Expenditure* is the total expenditure to deliver city services; *Total Local Source Revenue* is total city revenue net of transfers from the Federal, State, and County governments; *Total Debt Outstanding* is the total stock of debt outstanding issued by the city; *Mayor-Council Form of Government* indicates whether or not the city has a directly elected mayor; *Full-Time Elected Officials* indicates whether or not the elected officials serve in a full-time capacity; *City Population* is the city population annually estimated by the California state Department of Finance; *Per Capita Income* is the per capita income in the city; *Fraction College Graduate* is the fraction of the population with a college degree in the city; and *Employment-Population Ratio* is the ratio of employment to population in the city.

TABLE 2: The Effect of Appointive City Treasurers on City Borrowing Costs

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	OLS (1)	RD linear (2)	RD cubic (3)
Treasurer Appointed	-0.34** (0.13)		
Treasurer Appointive Referendum Pass		-0.68** (0.31)	-0.81** (0.38)
Number of Observations	4122	254	254
1990 Census controls	Yes	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	2.64 [2.43]	3.52 [2.11]	3.52 [2.11]
Sample	All Cities	Referendum Cities	

Notes: Source: Author's calculations using data from California City Financial Transactions Reports, and CEDA data from 1992 to 2008, 1992 Census of Government data and 1990 Decennial Census data. The sample in column (1) includes all general law cities in California with a municipal government in the 1992 Census of Governments and positive interest expenses in each year. The sample in column (2) and (3) includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a prior year. The variable *Treasurer Appointed* takes a value of one if the treasurer is appointed in city *i* in year *t*. The variable *Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed in a prior year in city *i* in year *t*. The unit of observation is referendum-year. Each column presents the results from one regression specification. The main entries are coefficient estimates. The entries in parentheses are standard errors clustered at the city level. All models include city characteristics from the 1990 Census: *Population Size, Percentage White, Percentage Black, Percentage Less Than Seventeen, Percentage Greater Than Sixty-Five, Percentage College Graduate, Percentage High School Graduate, Mayor-Council Form of Government, and Employment-Population Ratio*. The models in columns (2) and (3) also control for a linear and cubic polynomial in vote share respectively. * indicates significantly different from zero at the 10% level of significance; ** indicates significantly different from zero at the 5% level of significance; *** indicates significantly different from zero at 1% level of significance.

TABLE 3: City Financial Outcomes Prior to Treasurer Appointive Measure Vote

Model =	RD linear (1)	RD cubic (2)
<i>A. Dependent Variable = Interest Rate on Total Outstanding Debt</i>		
Future Treasurer Appointive Referendum Pass	-0.59 (0.45)	-0.17 (0.64)
Number of Observations	327	327
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	3.52 [2.11]	3.52 [2.11]
<i>B. Dependent Variable = Real Total Expenditure per capita</i>		
Future Treasurer Appointive Referendum Pass	2.58 (161.21)	-197.29 (235.03)
Number of Observations	327	327
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	868.86 [346.87]	868.86 [346.87]
<i>C. Dependent Variable = Real Total Local Source Revenue per capita</i>		
Future Treasurer Appointive Referendum Pass	40.76* (20.18)	-9.24 (22.56)
Number of Observations	327	327
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	368.53 [116.52]	368.53 [116.52]
<i>D. Dependent Variable = Real Total Outstanding Debt per capita</i>		
Future Treasurer Appointive Referendum Pass	-86.67 (380.79)	-400.94 (519.76)
Number of Observations	327	327
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	855.76 [743.56]	855.76 [743.56]
Sample	Referendum Cities	

Notes: Source: Author's calculations using data from California City Financial Transactions Reports, and CEDA data from 1992 to 2008, 1992 Census of Government data and 1990 Decennial Census data. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters

in a future year. The variable *Future Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed in a future year in city *i* in year *t*. The unit of observation is referendum-year. Each column presents the results from one regression specification. The main entries are coefficient estimates. The entries in parentheses are standard errors clustered at the city level. All models include city characteristics from the 1990 Census: *Population Size, Percentage White, Percentage Black, Percentage Less Than Seventeen, Percentage Greater Than Sixty-Five, Percentage College Graduate, Percentage High School Graduate, Mayor-Council Form of Government, and Employment-Population Ratio*. The models in columns (1) and (2) also control for a linear and cubic polynomial in vote share respectively. * indicates significantly different from zero at the 10% level of significance; ** indicates significantly different from zero at the 5% level of significance; *** indicates significantly different from zero at 1% level of significance.

TABLE 4: The Effect of Appointive City Clerks on City Borrowing Costs

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	OLS (1)	RD linear (2)	RD cubic (3)
Clerk Appointed	-0.12 (0.13)		
Clerk Appointive Ballot Pass		0.78 (0.55)	-0.93 (1.03)
Number of Observations	4122	155	155
1990 Census controls	Yes	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	2.64 [2.43]	3.43 [2.15]	3.43 [2.15]
Sample	All Cities	Referendum Cities	

Notes: Source: Author's calculations using data from California City Financial Transactions Reports, and CEDA data from 1992 to 2008, 1992 Census of Government data and 1990 Decennial Census data. The sample in column (1) includes all general law cities in California with a municipal government in the 1992 Census of Governments and positive interest expenses in each year. The sample in column (2) and (3) includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive clerk referendum put to voters in a prior year. The variable *Clerk Appointed* takes a value of one if the clerk is appointed in city *i* in year *t*. The variable *Clerk Appointive Referendum Pass* takes a value of one if a local clerk appointive referendum passed in a prior year in city *i* in year *t*. The unit of observation is referendum-year. Each column presents the results from one regression specification. The main entries are coefficient estimates. The entries in parentheses are standard errors clustered at the city level. All models include city characteristics from the 1990 Census: *Population Size*, *Percentage White*, *Percentage Black*, *Percentage Less Than Seventeen*, *Percentage Greater Than Sixty-Five*, *Percentage College Graduate*, *Percentage High School Graduate*, *Mayor-Council Form of Government*, and *Employment-Population Ratio*. The models in columns (2) and (3) also control for a linear and cubic polynomial in vote share respectively. * indicates significantly different from zero at the 10% level of significance; ** indicates significantly different from zero at the 5% level of significance; *** indicates significantly different from zero at 1% level of significance.

TABLE 5: The Effect of Appointive City Treasurers on City Borrowing Costs: Narrow Margin of Victory Sample

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	RD linear (1)	RD cubic (2)
Treasurer Appointive Referendum Pass	-1.13*** (0.23)	-2.02*** (0.53)
Number of Observations	160	160
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	4.01 [1.76]	4.01 [1.76]
Sample	Referendum Cities, Margin of Victory > -0.1 & < 0.1	

Notes: Source: Author's calculations using data from California City Financial Transactions Reports, and CEDA data from 1992 to 2008, 1992 Census of Government data and 1990 Decennial Census data. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a prior year with a vote share of between 0.4 and 0.6. The variable *Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed in a prior year in city *i* in year *t*. The unit of observation is referendum-year. Each column presents the results from one regression specification. The main entries are coefficient estimates. The entries in parentheses are standard errors clustered at the city level. All models include city characteristics from the 1990 Census: *Population Size*, *Percentage White*, *Percentage Black*, *Percentage Less Than Seventeen*, *Percentage Greater Than Sixty-Five*, *Percentage College Graduate*, *Percentage High School Graduate*, *Mayor-Council Form of Government*, and *Employment-Population Ratio*. The models in columns (1) and (2) also control for a linear and cubic polynomial in vote share respectively. * indicates significantly different from zero at the 10% level of significance; ** indicates significantly different from zero at the 5% level of significance; *** indicates significantly different from zero at 1% level of significance.

TABLE 6: The Effect of Appointive City Treasurers on City Fiscal Policy Outcomes

Model =	RD linear (1)	RD cubic (2)
<i>A. Dependent Variable = Real Total Expenditure per capita</i>		
Treasurer Appointive Referendum Pass	140.79 (227.64)	166.04 (270.29)
Number of Observations	254	254
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	868.86 [346.87]	868.86 [346.87]
<i>B. Dependent Variable = Real Total Local Source Revenue per capita</i>		
Treasurer Appointive Referendum Pass	108.57* (60.00)	86.19 (74.02)
Number of Observations	254	254
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	368.53 [116.52]	368.53 [116.52]
<i>C. Dependent Variable = Real Total Outstanding Debt per capita</i>		
Treasurer Appointive Referendum Pass	-139.17 (542.71)	-23.48 (624.41)
Number of Observations	254	254
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	855.76 [743.56]	855.76 [743.56]

Sample

Referendum Cities

Notes: Source: Author's calculations using data from California City Financial Transactions Reports, and CEDA data from 1992 to 2008, 1992 Census of Government data and 1990 Decennial Census data. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a future year. The variable *Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed in a prior year in city *i* in year *t*. The unit of observation is referendum-year. Each column presents the results from one regression specification. The main entries are coefficient estimates. The entries in parentheses are standard errors clustered at the city level. All models include city characteristics from the 1990 Census: *Population Size, Percentage White, Percentage Black, Percentage Less Than Seventeen, Percentage Greater Than Sixty-Five, Percentage College Graduate, Percentage High School Graduate, Mayor-Council Form of Government, and Employment-Population Ratio*. The models in columns (1) and (2) also control for a linear and cubic polynomial in vote share respectively. * indicates significantly different from zero at the 10% level of significance; ** indicates significantly different from zero at the 5% level of significance; *** indicates significantly different from zero at 1% level of significance.

TABLE 7: The Effect of Appointive City Treasurers on Other City Borrowing Costs

Model =	RD linear (1)	RD cubic (2)
<i>A. Dependent Variable = Expenditure on Debt Consultants per capita</i>		
Treasurer Appointive Referendum Pass	-17.41 (40.48)	-48.96 (68.46)
Number of Observations	254	254
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	28.00 [71.43]	28.00 [71.43]
<i>B. Dependent Variable = Expenditure on Management and Support Department per capita</i>		
Treasurer Appointive Referendum Pass	65.56** (25.17)	47.42 (33.45)
Number of Observations	254	254
1990 Census Controls	Yes	Yes
Baseline Dependent Variable Mean [Standard Deviation]	63.47 [44.58]	63.47 [44.58]

Sample

Referendum Cities

Notes: Source: Author's calculations using data from California City Financial Transactions Reports, and CEDA data from 1992 to 2006, 1992 Census of Government data and 1990 Decennial Census data. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a future year. The variable *Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed in a prior year in city *i* in year *t*. The unit of observation is referendum-year. Each column presents the results from one regression specification. The main entries are coefficient estimates. The entries in parentheses are standard errors clustered at the city level. All models include city characteristics from the 1990 Census: *Population Size, Percentage White, Percentage Black, Percentage Less Than Seventeen, Percentage Greater Than Sixty-Five, Percentage College Graduate, Percentage High School Graduate, Mayor-Council Form of Government, and Employment-Population Ratio*. The models in columns (1) and (2) also control for a linear and cubic polynomial in vote share respectively. * indicates significantly different from zero at the 10% level of significance; ** indicates significantly different from zero at the 5% level of significance; *** indicates significantly different from zero at 1% level of significance.

TABLE 8: The Effect of Appointive City Treasurers on City Borrowing Costs: FY2008 Debt Issue Sample, by Debt Issue Date

Dependent Variable = Interest Rate on Outstanding Debt Issue

Model =	All Debt Issues		Debt Issued Before Referendum		Debt Issued After Referendum	
	RD cubic (1)	RD cubic (2)	RD cubic (3)	RD cubic (4)	RD cubic (5)	RD cubic (6)
Treasurer Appointive Referendum Pass	-1.28* (0.68)	-1.33** (0.67)	-2.32** (0.91)	-2.54*** (0.91)	1.17 (1.10)	1.36 (1.04)
Number of Observations	205	205	129	129	76	76
1990 Census Controls	Yes	Yes	Yes	Yes	Yes	Yes
Debt Issue Characteristic Controls	No	Yes	No	Yes	No	Yes
Baseline Dependent Variable:						
Mean	5.06	5.00	5.33	5.33	4.45	4.45
[Standard Deviation]	[1.38]	[1.36]	[1.41]	[1.41]	[1.08]	[1.08]
{Min, Max}	{0.92, 9.31}	{0.92, 9.31}	{0.92, 9.31}	{0.92, 9.31}	{1.75, 6.86}	{1.75, 6.86}

Sample

Referendum Cities

Referendum Cities

Referendum Cities

Notes: Source: Author's calculations using data from California City Financial Transactions Reports from 2008, and CEDA data from 1995 to 2006, 1992 Census of Government data and 1990 Decennial Census data. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a prior year. The variable *Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed in a prior year in city *i* in a prior year. The unit of observation is debt issue. Each column presents the results from one regression specification. The main entries are coefficient estimates. The entries in parentheses are standard errors clustered at the city level. All models include a cubic polynomial in vote share and city characteristics from the 1990 Census: *Population Size*, *Percentage White*, *Percentage Black*, *Percentage Less Than Seventeen*, *Percentage Greater Than Sixty-Five*, *Percentage College Graduate*, *Percentage High School Graduate*, *Mayor-Council Form of Government*, and *Employment-Population Ratio*. The models in columns (2), (4) and (6) also include controls for the type and term of the debt issue. * indicates significantly different from zero at the 10% level of significance; ** indicates significantly different from zero at the 5% level of significance; *** indicates significantly different from zero at 1% level of significance.

TABLE A1: List of City Treasurer Appointive Referenda

Date	City	Vote Yes	Vote No	Total Votes	Percent in Favor	Referendum Pass	Joint Referendum with Appointive Clerk
7-Nov-95	Ontario	1836	6846	8682	0.21	No	
26-Mar-96	Dunsmuir	214	415	629	0.34	No	
26-Mar-96	Reedley	1701	1379	3080	0.55	Yes	
26-Mar-96	Ukiah	1651	1933	3584	0.46	No	
26-Mar-96	Orange	8730	12428	21158	0.41	No	
26-Mar-96	San Clemente	3495	6874	10369	0.34	No	
26-Mar-96	Brea	3092	3878	6970	0.44	No	
5-Nov-96	Live Oak	559	514	1073	0.52	Yes	Yes
5-Nov-96	Livermore	8557	14374	22931	0.37	No	
5-Nov-96	Auburn	1951	3226	5177	0.38	No	
5-Nov-96	Waterford	602	897	1499	0.40	No	
5-Nov-96	Santa Paula	2850	3553	6403	0.45	No	Yes
3-Nov-98	San Juan Bautista	166	346	512	0.32	No	
3-Nov-98	Paso Robles	2386	3486	5872	0.41	No	
3-Nov-98	Gonzales	474	447	921	0.51	Yes	
3-Nov-98	Benicia	2700	5943	8643	0.31	No	
3-Nov-98	Arroyo Grande	2486	3615	6101	0.41	No	Yes
8-Jun-99	Manteca	1967	1812	3779	0.52	Yes	
2-Nov-99	Livermore	7842	5698	13540	0.58	Yes	
7-Mar-00	Waterford	544	619	1163	0.47	No	
7-Mar-00	Santa Maria	6348	7086	13434	0.47	No	Yes
7-Nov-00	Tehachapi	889	1173	2062	0.43	No	
7-Nov-00	Woodland	7715	7123	14838	0.52	Yes	
7-Nov-00	Portola	298	459	757	0.39	No	
5-Nov-02	Turlock	5881	6077	11958	0.49	No	
5-Nov-02	Signal Hill	685	1125	1810	0.38	No	

5-Nov-02	Portola	162	390	552	0.29	No
5-Nov-02	Barstow	1028	2427	3455	0.30	No
5-Nov-02	Firebaugh	265	306	571	0.46	No
5-Nov-02	Paso Robles	2606	3824	6430	0.41	No
5-Nov-02	Wasco	952	1096	2048	0.46	No
28-Jan-03	South Gate	2537	5409	7946	0.32	No
4-Nov-03	Fontana	1931	1234	3165	0.61	Yes
2-Mar-04	Calistoga	607	590	1197	0.51	Yes
2-Nov-04	Waterford	990	779	1769	0.56	Yes
8-Nov-05	Pittsburg	4319	6016	10335	0.42	Yes

Notes: California Local Elections (CEDA) data from 1992 to 2006.

TABLE A2: Descriptive Statistics in 1992, by City Treasurer Appointive Referendum Status

	Treasurer Elected in 1992:		(1)-(2)
	Appointive Referendum Voted On (1)	Appointive Referendum Not Voted On (2)	t-stat [p-value] (3)
<u>(1) Outcome:</u>			
Interest Rate Paid on Total Outstanding Debt (%)	2.54 (2.43)	1.95 (2.38)	1.19 [0.236]
<u>(2) City Finances (2000 \$):</u>			
Total Debt Outstanding per capita	830 (805)	1021 (1660)	-0.88 [0.379]
Total Expenditure per capita	934 (393)	1022 (816)	-0.83 [0.407]
Total Local Source Revenue per capita	402 (167)	536 (724)	-1.74 [0.084]
<u>(3) City Government Characteristics:</u>			
Mayor-Council Form of Government	0.19 (0.40)	0.24 (0.43)	-0.51 [0.610]
Elected City Clerk	0.10 (0.30)	0.11 (0.32)	-0.27 [0.791]
Fraction with Full-Time Elected Officials	0 (0)	0.01 (0.10)	-1.00 [0.320]
<u>(4) City Economic Characteristics:</u>			
City Population	31614 (33826)	29035 (31026)	0.38 [0.703]
Per Capita Income (1990 \$)	13166 (4543)	15628 (8810)	-2.09 [0.039]
Fraction College Graduate	0.15 (0.09)	0.19 (0.13)	-1.85 [0.066]
Employment-Population Ratio	0.43 (0.07)	0.45 (0.09)	-0.83 [0.405]
Number of Observations	31	106	

Notes: Source: Author's calculations using data from California CFTR, and CEDA data from 1992 to 2008, 1990 Decennial Census, and 1992 Census of

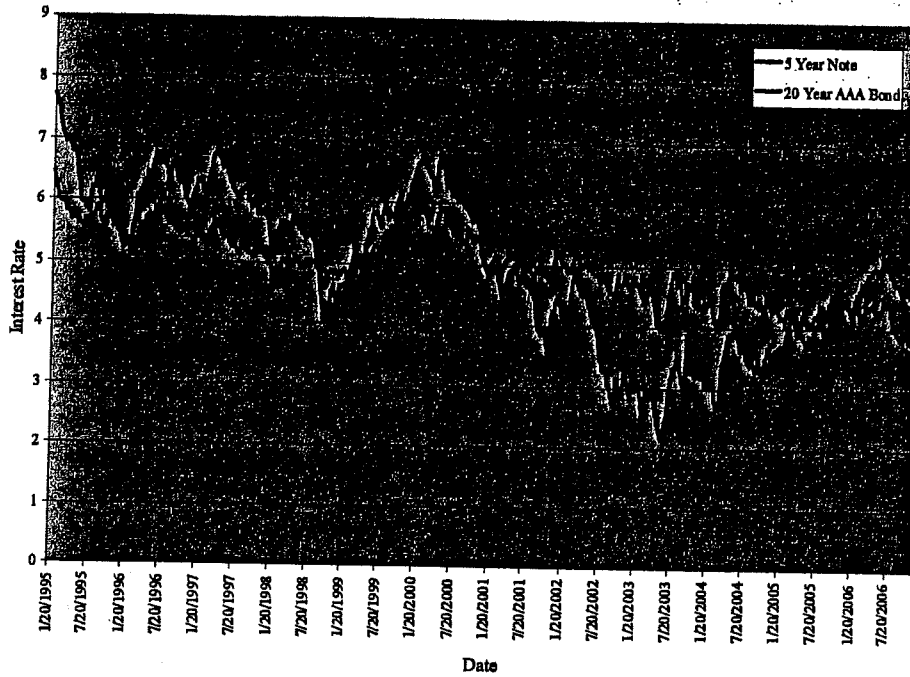
Government data. The unit of observation is city. The main entries in column (1) present the mean of the selected variables for cities with elected treasurers and a referendum voted on. The main entries in column (2) present the mean of the selected variables for cities with elected treasurers and a referendum not voted on. The standard deviations of the selected variable are presented in parenthesis in columns (1) and (2). The main entries in column (3) present the test statistics for a test of differences in means between column (1) and (2), with the p-value of the test presented in square brackets. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments and positive interest expenses in each year. All monetary values are expressed in 2000 \$ unless noted otherwise. The variables *Interest Rate Paid on Total Outstanding Debt*, *Total Local Source Revenue per capita*, *Total Expenditure per capita*, and *Total Debt Outstanding per capita* are based on data from the California City Financial Transactions Reports (CFTR). The variables *Mayor-Council Form of Government*, *Appointed City Clerk*, and *Full-Time Elected Officials* are based on data from the 1992 Census of Governments. The variable *City Population* is based on data from the California E-1 Population Tables. The variables *Per Capita Income*, *Fraction College Graduate*, and *Employment-Population Ratio* are based on data from the 1990 Decennial Census. Definition of the variables: *Interest Rate Paid on Total Outstanding Debt (%)* is the ratio of total interest expenses divided by total debt outstanding; *Total Expenditure* is the total expenditure to deliver city services; *Total Local Source Revenue* is total city revenue net of transfers from the Federal, State, and County governments; *Total Debt Outstanding* is the total stock of debt outstanding issued by the city; *Mayor-Council Form of Government* indicates whether or not the city has a directly elected mayor; *Full-Time Elected Officials* indicates whether or not the elected officials serve in a full-time capacity; *City Population* is the city population annually estimated by the California state Department of Finance; *Per Capita Income* is the per capita income in the city; *Fraction College Graduate* is the fraction of the population with a college degree in the city; and *Employment-Population Ratio* is the ratio of employment to population in the city.

TABLE A3: Baseline City Characteristics and Post-1992 Treasurer Appointive Referendums Outcomes

	Mean [Standard Deviation]	RD linear	RD cubic
	(1)	(2)	(3)
Per Capita Income	13166 (4543)	-255 (2471)	1138 (3347)
Employment-Population Ratio	0.43 (0.07)	-0.01 (0.04)	-0.01 (0.06)
Percent White	0.73 (0.19)	-0.02 (0.11)	0.03 (0.14)
Percent Black	0.03 (0.04)	0.00 (0.02)	-0.03 (0.03)
Percent Less Than 17	0.28 (0.05)	0.00 (0.03)	-0.05 (0.04)
Percent Greater Than 65	0.12 (0.05)	0.00 (0.03)	0.05 (0.04)
Percent College Graduate	0.15 (0.09)	-0.02 (0.05)	0.01 (0.07)
Percent High School Graduate	0.70 (0.16)	-0.02 (0.09)	-0.01 (0.11)
Population	31614 (33827)	12925 (18898)	-38903* (21106)
Mayor-Council Government	0.16 (0.40)	-0.01 (0.24)	0.04 (0.32)
Elected Clerk	0.90 (0.30)	-0.30* (0.17)	-0.30 (0.23)

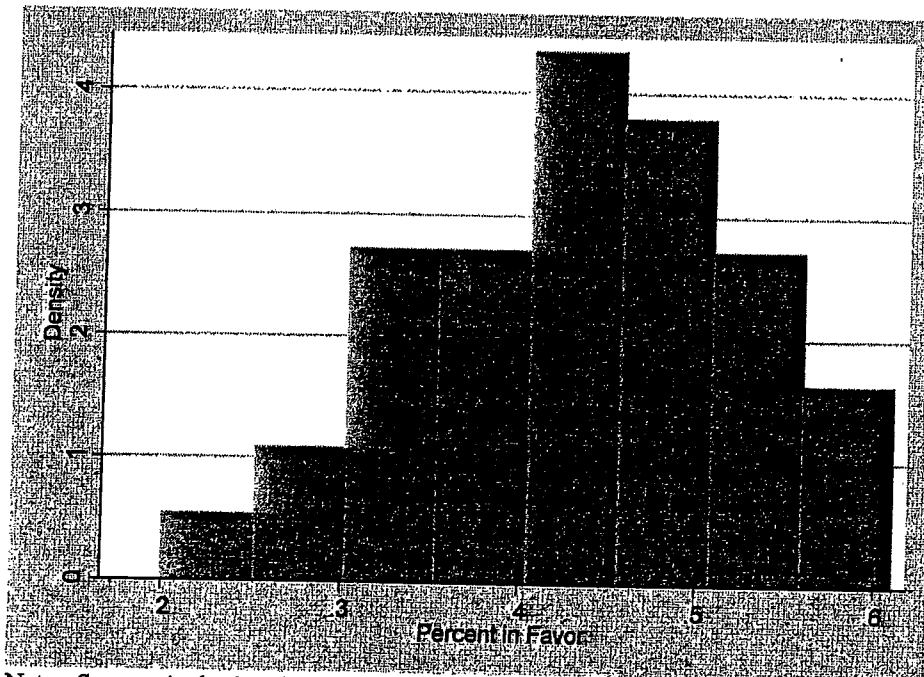
Notes: Source: Author's calculations using data CEDA data from 1992 to 2006, 1990 Decennial Census data, and 1992 Census of Governments data.. The sample includes all general law cites in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a future year. The variable *Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed in a future year in city *i*. The unit of observation is city (n=31). Each entry in columns (2) and (3) presents the results from one regression specification. The main entries are coefficient estimates. The entries in parentheses are standard errors. The models in columns (2) and (3) control for a linear and cubic polynomial in vote share respectively.* indicates significantly different from zero at the 10% level of significance; ** indicates significantly different from zero at the 5% level of significance; *** indicates significantly different from zero at 1% level of significance.

FIGURE 1: Weekly Municipal Bond and Note Market Interest Rates, 1996-2006



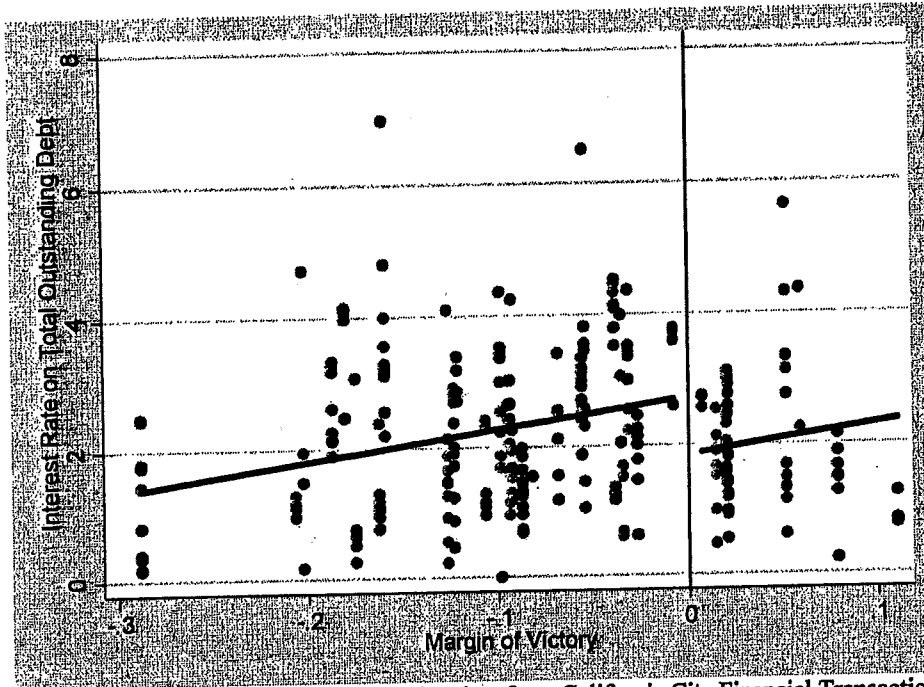
Notes: Source: Author's calculations using data from Global Financial Database – US Municipal Government Bond Yields.

FIGURE 2: Appointive Treasurer Referendum Vote Distribution



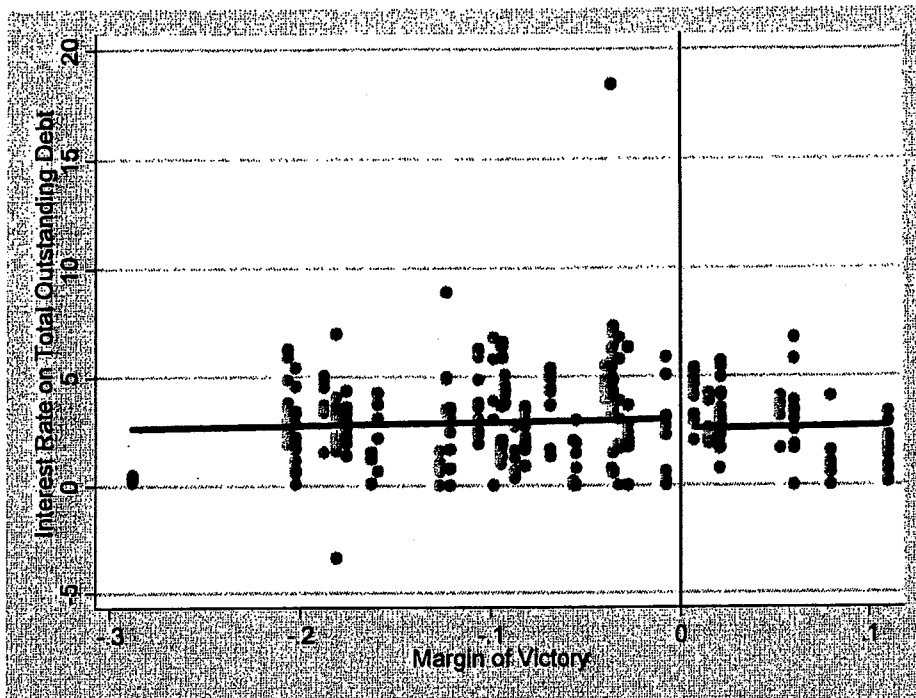
Notes: Source: Author's calculations using data from California Elections Data Archive (CEDA) data from 1995 to 2006.

FIGURE 3: Treasurer Appointive Effect



Notes: Source: Author's calculations using data from California City Financial Transactions Report, and CEDA data from 1992 to 2008. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a prior year. The figure plots the Interest Rate Paid on Total Outstanding Debt by the margin of victory in the treasurer appointive referendum. The line plots the predicted values of a regression of the interest rate on a referendum pass indicator, and a linear polynomial in referendum vote share.

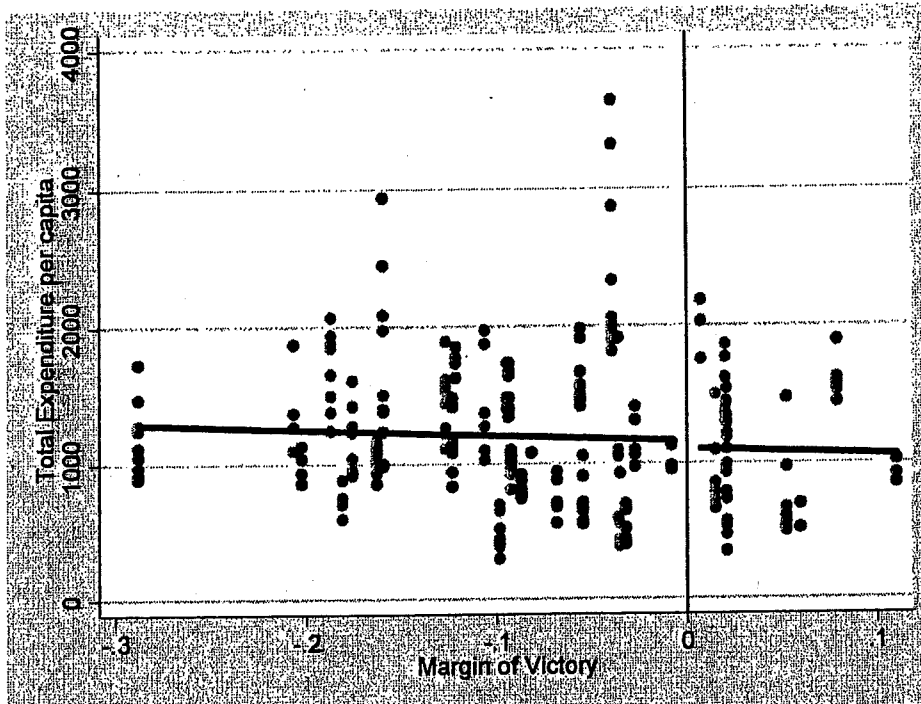
FIGURE 4: Treasurer Appointive Effect Prior to Treasurer Appointive Measure Vote



Notes: Source: Author's calculations using data from California City Financial Transactions Report, and CEDA data from 1992 to 2008. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a future year. The figure plots the Interest Rate Paid on Total Outstanding Debt by the margin of victory in the future treasurer appointive referendum. The line plots the predicted values of a regression of the interest rate on a future referendum pass indicator, and a linear polynomial in referendum vote share.

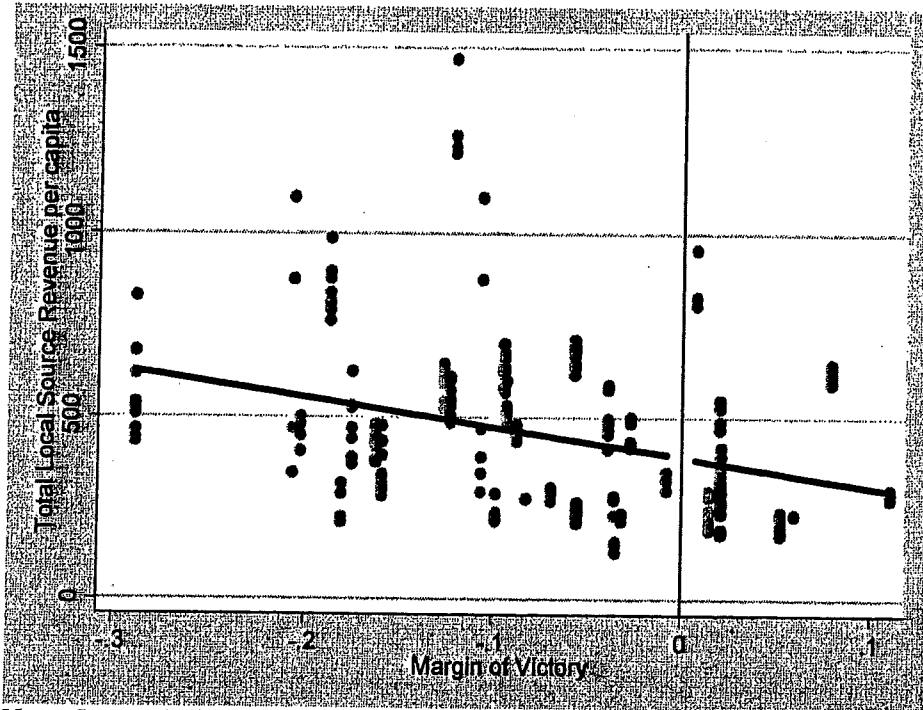
FIGURE 5: Appointive Effect on Fiscal Policy

(a) Total Expenditure



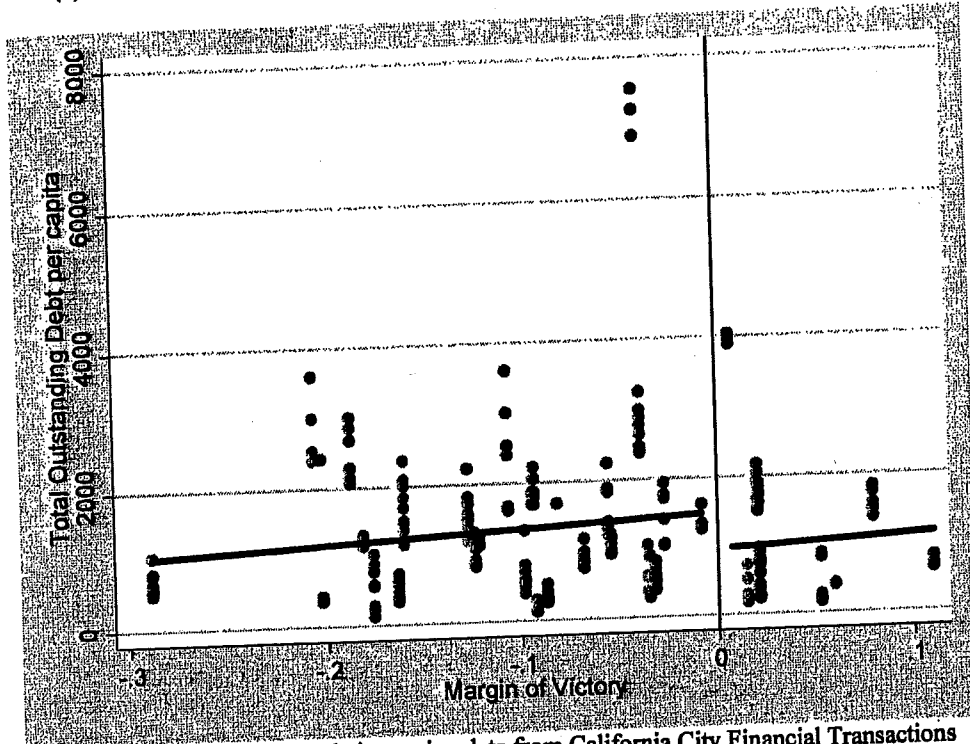
Notes: Source: Author's calculations using data from California City Financial Transactions Report, and CEDA data from 1992 to 2008. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a prior year. The figure plots the Total Expenditure per capita by the margin of victory in the treasurer appointive referendum. The line plots the predicted values of a regression of the Total Expenditure per capita on a referendum pass indicator, and a linear polynomial in referendum vote share.

(b) Total Local Source Revenue



Notes: Source: Author's calculations using data from California City Financial Transactions Report, and CEDA data from 1992 to 2008. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a prior year. The figure plots the Total Local Source Revenue per capita by the margin of victory in the treasurer appointive referendum. The line plots the predicted values of a regression of the Total Local Source Revenue per capita on a referendum pass indicator, and a linear polynomial in referendum vote share.

(c) Total Debt Outstanding



Notes: Source: Author's calculations using data from California City Financial Transactions Report, and CEDA data from 1992 to 2008. The sample includes all general law cities in California with a municipal government in the 1992 Census of Governments, positive interest expenses in each year, and an appointive treasurer referendum put to voters in a prior year. The figure plots the Total Debt Outstanding per capita by the margin of victory in the treasurer appointive referendum. The line plots the predicted values of a regression of the Total Debt Outstanding per capita on a referendum pass indicator, and a linear polynomial in referendum vote share.