

# Elected Versus Appointed Policymakers: Evidence from City Treasurers\*

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## **Abstract**

This paper investigates whether public official selection methods affect policy-making. 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 analysis draws on rich data from California to examine whether cities with appointed or elected city treasurers pay lower costs to borrow. The results demonstrate that appointive treasurers reduce a city's cost of borrowing by 17% to 31%. Further results indicate that appointive city treasurers reduce borrowing costs primarily through the refinancing of expensive debt at lower interest rates.

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JEL Classifications: D7, H1, H7.

# 1 Introduction

Do political institutions affect policymaking? The answer to the question depends crucially on whether policy outcomes are primarily due to the design of institutions (Besley, 2006), capture by special interests (Grossman and Helpman, 2001), or voter preferences (Downs, 1957). Despite the central importance of policymaking in political economics, our knowledge of the responsiveness of policymaking to alternative political institutions remains limited.<sup>1</sup>

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. Bureaucrats and politicians face quite different incentives to exert effort. Politicians are directly accountable to voters, and exert effort to gain reelection. In contrast, top-level bureaucrats are accountable to their professional peers, and exert effort to gain future job postings or professional recognition. Whether politicians or bureaucrats exert more effort, or simply pander to voters and special interests, depends crucially on the strength of the implicit incentives that reelection and career concerns provide. As the importance of these implicit incentives is unclear, the effect of delegating policy to a bureaucrat or politician remains an open question.<sup>2</sup>

I examine the effect of political versus bureaucratic control of policy for the case of city treasurers for several reasons. First, city treasurers' policy choices can be easily observed in the costs cities pay to borrow. Debt management policy is a particularly compelling government service to examine as costs are observable, in the expenditure cities pay to borrow, and attributes of public debt other than interest rates are likely to be less important to constituents than in other contexts.<sup>3</sup> In addition, the complexity

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<sup>1</sup>Besley and Case (2003) provide an excellent survey of the literature. Seminal contributions include Barro (1973) and Ferejohn (1986).

<sup>2</sup>A significant theoretical literature has addressed this question. See Besley and Coate (2003), Maskin and Tirole (2004), Alesina and Tabellini (2007, 2008), and Vlaicu (2008). Much of the existing empirical work has focused on the related question of whether politicians are more likely than bureaucrats to choose policies that appeal to voters. See Besley and Coate (2003), Besley and Payne (2006), and Vlaicu (2008). For recent evidence on career concerns for bureaucrats see Boylan and Long (2005) and Phillips and Grace (2008).

<sup>3</sup>There are, of course, many reasons for differences in the cost of borrowing across cities. The majority

of debt management policy allows for a close match between theory and empirics as 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)<sup>4</sup>.

Second, the context of city treasurers seems to match typically considered accountability structures that emphasize effort and expertise effects very well. In terms of effort, elected and appointed treasurers likely face very different career concerns 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 infer talent in the policy performance of the politician and how the city treasurer profession infers talent from policy performance by the appointed treasurer. 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 and do not need to be a city resident. The context of city treasurers provides a rare opportunity to examine whether differences in the implicit incentives bureaucrats and politicians face do indeed translate into meaningful differences in policy performance.

Last, quasi-experimental variation in the treasurers' appointive status is available. As many have argued that political institutions may be endogenously chosen to reflect the policy preferences of special interests or voters, disentangling the effects of government form from other unobserved factors may not be straightforward.<sup>5</sup> However, as special interests and voters may face substantial costs in influencing the choice of political insti-

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of the variation in borrowing costs across cities is due to differences in default risk. However, some of the differences in borrowing costs across cities may reflect differences in attributes of the debt that city residents value, such as variability in future interest rates. I discuss debt attributes more fully in section 6 below.

<sup>4</sup>While debt management is a complex policy task is unlikely to be so complex that highly competent and specialized public officials are unable to understand debt management, as Baye and Wright (2010) find for generalist judges in anti-trust cases.

<sup>5</sup>See Besley and Case (2003) and Acemoglu (2005) for discussion 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.

tutions the endogeneity of institutions may not be a substantive concern in practice. The case of city treasurers provides a chance to examine whether addressing the endogeneity of political institutions substantively alters conclusions about institutional effects on policymaking.

Before introducing the empirical approaches it is worthwhile to explain how city borrowing costs measure debt management policy choices. The cost that a city pays to finance a debt issue is composed primarily of two factors, the perceived default risk of a city and the debt management policy choices of a city treasurer. The default risk of a city reflects any factors that predict default and thus require a premium from bond investors, from the size and diversity of the tax base, to whether city expenditure policies are sustainable, etc. Two debt management policies chosen by the treasurer also affect borrowing costs: *debt refinancing* and *debt issuance* policy. Reducing borrowing costs with debt refinancing policies requires active monitoring of public debt markets, as well as, the expertise to evaluate likely changes in 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. The challenge for the empirical analysis is to control for city default risk so that the remaining differences in borrowing costs between cities with differing treasurer selection methods reflect debt management policy choices alone.

The central contribution of this paper is to estimate the effect of bureaucratic versus political control on policymaking. Two alternative and complementary approaches are used to measure the appointive effect. First, I present an ordinary least squares (OLS) regression analysis to compare borrowing costs in cities with elected and with appointed city treasurers. By using richly detailed data for a large number of cities I am able to control for a multitude of factors that affect default risk, and thus, borrowing costs which might also be related to the choice of institutions. To the extent that city economic, demographic, and fiscal policy characteristics are sufficient to capture the relationship between default risk and city treasurer appointive status the OLS regressions will yield an estimate with a casual interpretation. Of course, as the choice of political institutions may be related to unobservable determinants of policy choice, the OLS results may be driven by the selective adoption of political institutions.

In my second approach I implement a regression discontinuity (RD) research design to address concerns about unobservable determinants of the city borrowing costs being correlated with political institutions. 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 group 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 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.<sup>6</sup>

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 17% to 31% in the OLS analysis. The results of the RD analysis indicate that having an appointed rather than elected city treasurer reduces city borrowing costs by 20% to 29%. The similarity of the results suggests that unobservable determinants of borrowing costs are not strongly related to the choice of political institutions. 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 next strive to understand the sources of the appointive effect. To do so I examine whether the appointive effect is primarily due to differences in *debt refinancing* or *debt issuance* policy choices. The evidence indicates that the appointive effect is primarily due to debt restructuring, but not debt issuing policies. As the reduction in borrowing costs from the restructuring of city debt requires significant expertise and effort the results suggest that the appointive effect is due to differences in treasurer supplied inputs rather

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<sup>6</sup>Lee and Lemeux (2008) provide an excellent survey of the regression discontinuity research design.

interest group influence.

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 as city debt management is a highly technical policy. 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). Empirically, the results contrast Besley and Coate (2003) who find that elected electricity rate regulators choose lower prices than appointed regulators.<sup>7</sup>

The remainder of the paper is organized as follows: Section 2 describes the conceptual framework and context. Section 3 describes the empirical approach. Section 4 discusses the data and descriptive statistics. Section 5 presents the main results. Section 6 examines evidence of differences in debt restructuring and issuance policy choices between politician and bureaucrat treasurers. Section 7 concludes.

## 2 Conceptual Framework, City Treasurers, and Debt Management Policy

### 2.1 Conceptual Framework

**Political Agency.** In this section I discuss a simple framework to understand the effects of treasurer appointive status on the policy outcome, city borrowing costs. I discuss these effects in a simple agency model of treasurer performance based on Alesina and Tabellini (2007).<sup>8</sup> In the model there are two treasurer supplied inputs that affect policy performance: treasurer effort and ability. The choice of treasurer selection methods affects

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<sup>7</sup>There is large literature on the effects of appointed and elected regulators that has generally found mixed results. See Besley and Coate (2003) for an excellent survey of the prior literature.

<sup>8</sup>A simple formal political agency model is presented in the appendix which more fully illustrates the roles of incentives and selection in policy performance that I discuss here.

the policy outcome through differences between bureaucrats and politicians in terms of effort, the *effort* effect, or in terms of expertise, the *expertise* effect.

In general, the sign of the appointive effect for debt management policy outcomes is ambiguous. It hinges crucially on the sign and magnitude of both the effort and the expertise effects of bureaucratic versus political control. In principle, either effect could go in either direction. If the reelection of a politician city treasurer depends crucially on government borrowing performance, but future employers of a bureaucrat place little value on prior government borrowing performance the effort effect would favor the politician. Alternatively, if constituents face significant difficulty in observing policy performance by politicians, but future employers of bureaucrats highly value prior policy performance the effort effect would favor the bureaucrat. The sign of the expertise effect is also ambiguous. For example, bureaucrats might be more skilled than politician on average if bureaucrats are selected based on prior policy performance or education. Alternatively if city treasurer positions are allocated to bureaucrats on a patronage basis bureaucrats may be less skilled.

The sign and magnitude of the effort and expertise effects may be influenced pressure from strong interest groups. While the presence of a special interest group is not explicitly incorporated into the simple political agency framework, it is straightforward to think about special interests seeking to influence the effort and ability supplied by a treasurer. For example, financial intermediaries may make campaign contributions to elected treasurers or provide kickbacks to appointed treasurers to influence policy choices. Of course, whether special interests prefer an appointed or elected city treasurer likely depends on the relative costs of capturing the treasurer under alternative political institutions.

**City Treasurers.** The context of city treasurers seems to match the conceptual framework of Alesina and Tabellini (2007) very well. Appointed and elected treasurers are likely to differ in the level of expertise and effort the 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 and do not need to be a city resident. This difference often results in appointed treasurers



having higher levels of education (often an MBA or MPP degree) than elected treasurers.<sup>9</sup>

In terms of effort, elected and appointed treasurers likely face very different career concerns 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 infer talent in the policy performance of the politician and how the city treasurer profession infers talent from policy performance by the appointed treasurer.

Anecdotal evidence suggests that the incentives to exert effort for an appointed treasurer may well be larger than for an elected treasurer. The city treasurer occupation is highly professionalized with a number of active associations and well developed notions of professional performance. For example, the code of ethics for California Public Treasurers contains the clauses: “To promote principles of good government. To be dedicated to the concepts of effective and efficient local government service being provided by elected and appointed Treasurers.” and “To observe the profession’s technical standards and continually strive to improve the Treasurers level of competence.” (CMTA, 2001) Similar clauses are contained in the Association of Public Treasurers of the United States and Canada.<sup>10</sup> Many job advertisements for appointed treasurer positions require evidence that best practices in public finance were used in prior positions, etc. In contrast, elected city treasurers may have less incentive to exert effort in the face of a public that has little knowledge of debt management policy.

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<sup>9</sup>I 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.

<sup>10</sup>The code of ethics for states that: “A member shall adhere to concepts of effective and efficient local government service being provided by elected and appointed Public Treasurers. A member shall conduct himself or herself at all times in a manner which serves the public interest and maintains the good reputation of the profession.” and “A member shall observe professional technical standards and continually strive to acquire knowledge and improve levels of competence in Treasury management.” (APTUSC, 2005)

## 2.2 City Borrowing Costs and Debt Management Policy

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.<sup>11</sup>

**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.<sup>12</sup> Aspects of the local economy and city fiscal policy affect default risk, and are reflected in the city debt rating. These factors range from the diversity of the tax base, to whether city expenditure policies are sustainable, etc. In addition, other aspects of a debt issue often determined by the nature of the project to be funded such as the term length of the issue, and the principal amount can affect borrowing costs. 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

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<sup>11</sup>For 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.

<sup>12</sup>Municipal 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).

for the underwriter.<sup>13</sup> Levitt (2009) criticizes non-competitive sales as ‘pay to play’ rewards for the campaign finance provided to elected officials by financial intermediaries. Thus, the choice of competitive or non-competitive sales methods is likely to be related to the influence of special interests. Other debt issuance policy choices include when to issue the debt, and the characteristics of the debt issue such as term length, etc.

**Debt Refinancing Policies.** Debt refinancing policies are the second type of policies that can influence borrowing costs. A city debt issue, much like conventional mortgages, can be refinanced. As market interest rates fluctuate according to many factors, refinancing an issue when it is worthwhile to do so can result in lower average borrowing costs on city debt. The potential benefit of 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. Market interest rate fluctuations reflect predictable patterns of demand such as the timing of 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 market interest rates.

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%. Of course refinancing a debt issue is not without transactions costs, however these are relatively low. The Government Finance Officers Association’s “Best Practice” recommends that refinancing an issue is cost-effective if the net present value of the interest savings from refinancing are on the order of 3 to 5 percent (GFOA, 1995).

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<sup>13</sup>The evidence is very clear on this difference in borrowing costs. See Robbins and Simonson (2007) and Simonson, Robbins and Helgerson (2001) for recent studies showing that competitive sales reduce borrowing costs for cities.

### 3 Empirical Approaches

The analysis employs two complementary empirical approaches. I first conduct an OLS analysis that uses a large sample of cities and richly detailed data on city economic, population, and financial characteristics to control for default risk. While the OLS analysis does not address concerns about correlation between unobservable default risk and political institutions, it does use a large sample of cities to measure the effect. In contrast, the RD analysis exploits quasi-experimental variation in political institutions to effectively address concerns about institutional endogeneity. However, the RD analysis is necessarily conducted on the smaller sample of cities that put a treasurer appointive referendum to the voters during the sample period.<sup>14</sup> Thus, one particularly attractive feature of the city treasurer context is that both approaches, that have quite different sets of assumptions, are possible and comparable.

#### 3.1 Ordinary Least Squares

My first approach is to 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 city and  $t$  indexes the year. The variable  $\text{appointed}_{it}$  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,

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<sup>14</sup>The lack of large numbers of changes in political institutions is well known in the literature. In fact, because important political institutions change so infrequently (and any changes are likely endogenous), prior researchers in this area have necessarily focused on either cross-sectional analysis alone or within-state analysis, with very few ‘experiments’ at all. A few examples from recent studies: Besley and Case (2003) note that some of the major state-level changes in electoral rules in the United States from 1950 to the 1990’s, such as literacy tests or poll taxes, affected only 15% of states. In addition, the highly influential study of Besley and Coate (2003) has only 3 states that switch (from 1960-1997) from which to estimate the effect of the electoral status of regulators on electricity prices. Many other careful and important studies are limited to the analysis of cross-sectional data alone; see Persson and Tabellini (2003) and Aghion, Alesina, and Trebbi (2004) for recent examples of this work.

$\omega_t$  is a set of year fixed effects, and  $\epsilon_{it}$  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  $\delta$ . I cluster the standard errors at the city level to account for the fact that borrowing costs are correlated within a city and changes in treasurer appointive status are persistent (Bertrand, Duflo, and Mullainathan, 2004).

The outcome variable for the main analysis,  $Y_{it}$ , measures the borrowing costs in city in a given year by the average interest rate paid on the stock of city debt. It is important to be clear on how this measure of borrowing costs measures city debt management policy choices. The interest rate on the stock of city debt will be affected by either debt issuance or restructuring choices. For the analysis that examines changes in the treasurer position within a city my analysis likely captures the short run effect of a change in treasurer selection methods. This 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. Of course, if the OLS estimates yield similar estimates to the models based on changes in the treasurer position within a city the short and long-run effects are unlikely to differ substantially.

While it is possible to control for many easily measured city characteristics in the  $X_i$  vector there could be important unobserved components of borrowing costs that are correlated with political institutions. 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 Tabelinni, 1990) or high spending city legislative institutions (Coate and Knight, 2009) may lead to substantial debt accumulation and high borrowing costs. As the importance of the endogeneity of political institutions for policymaking is unclear, whether the rich set of city measures here captures effectively address selection issues is

an open question.

### 3.2 Regression Discontinuity

To address potential concerns that treasurer method of selection may be endogenously related to other determinants of city borrowing costs I also 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 implement my regression discontinuity approach I estimate models of the following form for each policy outcome,  $Y_{jt}$ ,

$$(2) \quad Y_{jt} = \delta pass_{jt} + \gamma(s_j) + X_j\beta + \omega_t + \epsilon_{jt},$$

where  $j$  indexes the focal referenda and  $t$  indexes the year. The variable  $pass_{jt}$  is a dummy variable that takes a value of one in every year after the referendum passes and value zero otherwise. The vote share is denoted  $s_j$ ,  $\omega_t$  is a set of year fixed effects, and  $\epsilon_{jt}$  is the error term. Thus, the appointive effect,  $\delta$ , is estimated controlling for the percent voting in favor with a flexible polynomial functional form. This simple model will yield an unbiased estimate of the appointive effect if the referendum outcome,  $pass_j$ , was uncorrelated with the other determinants of policy,  $\epsilon_{jt}$  conditional on the vote share,  $s_{jt}$ .

Lee (2008) demonstrates that the RD 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 central identifying assumption for  $\delta$  to estimate the causal effect of treasurer selection method 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 referendum vote share, in the years prior to referenda to assess its plausibility.<sup>15</sup>

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<sup>15</sup>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 controls for the vote share. The RD design can be estimated parametrically or non-parametrically, focusing on only close elections or the larger sample

To estimate (2) 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.<sup>16</sup> 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. 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.

**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.<sup>17</sup> 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

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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.

<sup>16</sup>There are four cities that put multiple referendums to the voters, two of which fail the first time and subsequently pass, in the sample period. I examine the robustness of my results to the exclusion of these cities as a sensitivity analysis.

<sup>17</sup>Local 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).

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.

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.

I list the treasurer referendums in Table A1.<sup>18</sup> 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 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 the RD estimation strategy to be compelling.

As my RD 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 these cities are 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 income per capita and skill.

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<sup>18</sup>I discuss details of the exact data source and sample selection below in the data section.



## 4 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.<sup>19</sup> 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 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.<sup>20</sup> 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.<sup>21</sup>

Together the data from CFTR and CEDA form the panel of California cities that I

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<sup>19</sup>As the treasurer selection methods change two years after the referendum is voted on I am unable to use any of the post 2006 referendums.

<sup>20</sup>Government Code section 12463 directs the California State Controller to annually compile and report to the public the financial transactions of all California cities.

<sup>21</sup>The 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.

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.<sup>22</sup>

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.<sup>23</sup> 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 cities without reported interest costs in each year, as a number of smaller cities do not have outstanding debt in every year. My final sample analysis sample contains 301 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.<sup>24</sup> 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 are more than 25% (50 basis

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<sup>22</sup>Interest expenses are missing for over 65 percent of the pre-2008 issue observations, but only 5 percent for the 2008 issue observations.

<sup>23</sup>General 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.

<sup>24</sup>Treasurer method of selection in a given year is defined from the baseline method of selection variable and any subsequent passing treasurer appointive referendums.

points) greater than those in cities with appointed treasurers. Cities that have elected and appointed treasurers are also somewhat 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. These differences could well 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.

## 5 Results

### 5.1 Main Results

Table 2 presents OLS estimates of  $\delta$  from fitting various version of equation (1). In column (1) of the Table 2 we can see that cities with appointed city treasurers pay less to borrow. The results in column (2) show that the estimate changes little when the baseline variables measuring economic and demographic characteristics of cities are included. As city fiscal policy is likely related to borrowing costs, and may be related to the choice of an appointed city treasurer, the specifications in column (3) of Table 2 add controls for city spending, debt, and local taxes. The results change little with the inclusion of the fiscal policy controls. The appointive effect remains negative and statistically significant. Lastly, a city fixed effects specification is estimated by estimating equation (1) without the time-invariant controls, but with city fixed effects. This model is identified from variation in the political institutions within a city, and thus all time-invariant city characteristics are effectively controlled for. Again, the results in column (4) of Table 2 reveal a negative and statistically significant relationship between having an appointed city treasurer and city borrowing costs.

The results in Table 2 also reveal that the magnitude of the appointive effect varies

little across the alternative specifications. Relative to the baseline level of borrowing costs the estimates range from 17% to 31%. Thus, the appointive effects in Table 2 also appear to be economically significant. As interest payments in cities with elected treasurers account for nearly \$43 per capita, the median estimates imply that appointive treasurers reduce annual interest expenditure by more than \$10 per capita or \$300,000 for the city in total. If we make the plausible assumption that that there are no general equilibrium effects of treasurer appointive status we can get a sense of the overall effects that the OLS estimates imply. The results imply that if all 133 cities with elected treasurers in the baseline period were to switch to appointed city treasurers total annual borrowing expenditures in would fall by over \$40 million.

Before turning to the RD estimates I conduct one further exercise to examine whether the OLS specifications in Table 2 are likely to suffer from omitted variable bias. To shed light on this question I estimate similar OLS models to those in Table 2, but replace the appointed *treasurer* variable with an appointed *clerk* variable in Table 3. As cities that have appointed treasurers are also very likely to have appointed clerks any unobservable determinants of the adoption of an appointive city treasurer are also likely to be related to the adoption of an appointive city clerk. However, because city clerks are not involved in debt management policy we would expect that conditional on the control variables there is no appointive effect for city clerks.

I present the results from an OLS model of the appointive city clerk effect in Table 3. These results show that while the city clerk effect is statistically significant without any control variables in column (1) of Table 3, adding the control variables or the city fixed effects leads the city clerk effect to become statistically insignificant. Thus, the appointive clerk effect estimates suggest that the OLS estimates of the treasurer appointive effect is not likely to be confounded by omitted variable bias. Of course, as these results are only suggestive, further investigation is warranted.

I next present the RD estimates in Table 4. I present the estimates of  $\delta$  from fitting two versions of equation (2) with, and without, baseline city characteristic controls. In column (1) I present the RD estimates with a vote share control only. This estimate indicates that appointed city treasurers do indeed cause city borrowing costs to be lower. Again, the effect appears to be both statistically and economically significant. The passage of an

appointive treasurer referendum reduces borrowing costs by 29% of baseline. Similarly the results in column (2) of Table 4 show that adding city controls does little to alter the statistical significance of the effect. In addition the magnitude of the effect remains above 20% of baseline. The baseline RD results in column (1) of Table 4 are depicted visually in Figure 3. The graph clearly shows the positive vote share-interest rate gradient, and the discontinuous drop in borrowing costs that occurs at the referendum passage threshold.

The fact that the OLS and RD appointive effect estimates are quite comparable is interesting. This finding is particularly notable as the different analyses address institutional endogeneity in very different ways and are necessarily conducted on quite different samples of cities. The similarity of the estimates provides reassurance that the RD estimates are not local to only the set of cities that put an appointive treasurer referendum to the voters. Furthermore, the similarity of the estimates across the two approaches also suggests that any endogeneity of treasurer selection method adoption has little meaningful effect on the coefficient estimates. Even though the estimates appear to be very similar I next present a series of robustness checks to probe the validity of the RD estimates.

## 5.2 Robustness Tests

While 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 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 in Tables 5 and 6 to address these potential concerns.

**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 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 the RD identification strategy would be threatened.

In the top panel of Table 5 I estimate versions of equation (2) using only pre-referendum data on city borrowing costs. 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. 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.

**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 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. 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 again examine whether the narrow passage of an appointive *clerk* referendum has a similar effect to narrow passage of the treasurer appointive referendum as a falsification check. I present the results of the appointed clerk RD models in the second panel of Table 5. I then estimate an RD model similar to (2) where the referendum pass and vote share variables are from the appointive clerk referendum rather than the appointive treasurer referendum.<sup>25</sup> The results in both columns reveal little statistically significant effect of appointed clerks on city borrowing costs. The fact the point estimates are not statistically significant provides further reassurance that the city

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<sup>25</sup>A 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.

treasurer appointive effect in Table 4 is not simply due to other unobservable institutional changes.

**Alternative Specifications and Samples.** In Table 6 I examine whether the RD results are robust across alternative samples and specifications. I first consider whether the results differ in the sample of referendums in the neighborhood of passage. 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 control to smooth other unobserved determinants of borrowing costs. It is natural to ask whether the appointive effect is similar in the smaller sample of referendums that barely pass and barely fail. I estimate my models on this ‘RD sample’ next. In Table 6 I present estimates of RD specifications for the sample of twenty referendums that either pass or fail by less than 10 percent in the top panel. The first column in Table 6 shows that the passage of treasurer appointive referendum in the RD specification again reduces the city borrowing costs.<sup>26</sup>

I next examine whether the results are robust to including city fixed effects in the RD model in panel B of Table 6. Again, I obtain similar point estimates to those above and the results are statistically significant at the five percent level. The third and fourth panels of Table 6 examine whether the results are sensitive to excluding extreme values of the borrowing costs and cities with multiple referendums from the sample. In column (1) we see that the effects remain statistically significant at the 10% level for each of the smaller samples. In column (2) we see that while the estimates are not statistically significant when the census controls are added and the cities with multiple referendums are dropped, they remain statistically significant when the outliers are dropped. In addition, as the point estimates in Table 6 are generally very close to those in Table 4, any differences in statistical significance are likely due to a loss of precision from the smaller sample rather than substantively different estimates. In sum, the results in Table 6 demonstrate that the RD results are quite robust to alternative samples and specifications.

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<sup>26</sup>The minimum number of clusters necessary for asymptotic distributions to be appropriate is still under discussion in the literature. Recent Monte Carlo evidence in Hansen (2007) indicates that asymptotic distributions for clustered standard errors perform well with as few as 10 clusters.

### 5.3 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 reducing the number of elected officials in a city affects fiscal policy. Of course, as the city treasurer is not part of the city council we might expect the effects to be less significant than Berry and Gersen (2009) find for electoral density in general. 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 7 I present OLS and RD estimates of the appointive treasurer effect for fiscal policy. In the first panel of the table I examine the appointive effect for total city expenditure. In this panel we see that there is little evidence of an effect on total expenditure that likely indicates the reduction in borrowing costs is spent on other public goods or tax revenue reduction. In the second panel of Table 6 I look at the treasurer appointive effect for locally generated revenue. In the second column the estimate is positive and statistically significant at the 10% level. The estimates in the first and third columns are not statistically significant however. The lack of a clear negative effect suggests that the reduction in borrowing costs is not passed on to taxpayers. The last panel in Table 7 examines the appointive effect for the stock of total debt outstanding. Again, the results show little consistent evidence of an appointive city treasurer effect on total debt outstanding.

**Debt Issuance and Management Costs.** I next turn to the question of whether other costs related to debt management are related to treasurer method of selection, as interest rates are not the only cost cities pay to borrow. 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 that form an important component of transactions costs. 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 addition, rents could be provided to advisors if appointed or elected city treasurers are captured by financial intermediaries.

In the first panel of Table 8, I present the results of estimating OLS and RD models with the outcome of debt issuance consultant expenditure, similar to equations (1) and (2) respectively. The results clearly show little statistically significant effect of appointive treasurer referendum passage on expenditure on debt consultants regardless of the precise specification.<sup>27</sup>

Second, I 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 the second panel of Table 8, I present the results of estimating OLS and RD models with the outcome of total expenditure on general government administration, similar to equations (1) and (2) respectively. The results are mixed. The OLS and unconditional RD estimates in columns (1) and (2) are not statistically significant. However, the conditional RD estimate in column (3) indicates that appointive city treasurers do increase general government administration expenditure. The results in Table 8 do not provide consistent evidence of strong appointive city treasurer effect on the cost of government administration.

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<sup>27</sup>Unfortunately, the data used in this study does not provide information on the identity of the financial advisor so I am unable to explore the nature and impact of advisor-treasurer connections in more detail.

## 6 Debt Refinancing 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 consistent with differing debt refinancing or debt issuance policies. I first examine whether appointed and elected city treasurers' choose different debt refinancing policies. I then consider whether appointed and elected treasurers choose different debt issuance policies.

Beyond understanding the source of the effect above understanding the channel for the appointive effect also allows us to assess whether interpreting the appointive effect as reflecting public official performance is warranted. As both treasurer effort and expertise are required to refinance an existing debt issue at a lower cost, debt refinancing is closely related to treasurer effort and skill inputs. 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.

### 6.1 Measuring Debt Refinancing 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.<sup>28</sup> 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 idea is that a city treasurers new debt issuance policies can only affect the interest rates on the debt issued after they take office. New debt issuance policies can then only affect the interest rate on debt issued *after* the referendum. In contrast, the appointive effect on debt issued prior to the referendum can only be due to the selective retiring of expensive debt through refinancing. Debt restructuring policies will then only affect the interest rate on debt

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<sup>28</sup>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. As the debt issue data is for the fiscal year 2008 ending June 30 2008, interest rates on these issues are not affected by the subsequent financial crisis in fall 2008.

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,  $I_{ij}$ ,

$$(3) \quad I_{ij} = \delta pass_{ij} + P_3(s_i) + \epsilon_{ij},$$

where  $i$  indexes the appointive treasurer referendum and  $j$  indexes the issue. The variable  $pass_i$  is a dummy variable that takes a value is a referendum passes in a prior year and value zero otherwise.  $P_3(s_i)$  is a third order polynomial function of the referenda vote share,  $s_i$ , and  $\epsilon_{it}$  is the error term. I estimate models both with and with out debt characteristic controls. The models including debt characteristic controls include variables for the term length of the debt as well as an indicator variable for four types of debt (dedicated funding bonds, non-dedicated funding bonds, short term notes and loans, and other).<sup>29</sup>

## 6.2 Results

The results of estimating (3) are reported in Table 9. 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 and term length of the debt issue in columns (2), (4) and (6).

**Debt Refinancing.** The results from estimating equation (3) for the debt issued *before* the referendum are presented in columns (3) and (4) of Table 9. These results indicate that differences in city treasurer methods of selection cause differences in debt refinancing

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<sup>29</sup>The debt types are defined in the following way. The dedicated funding bond debt types are 1911 Act Bonds, 1915 Act Bonds, Community Facility District, Mark-Roos and Mello-Roos bonds. The non-dedicated funding bond debt types are General Obligation and Revenue bonds. The short term notes and loans debt types are Notes and Loans. The other debt types include all remaining debt types.

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 25% to 29% of baseline.<sup>30</sup> Importantly, this effect is an order of magnitude larger than the 3% to 5% borrowing costs savings needed to recover any costs associated with the refinancing transaction (GFOA, 1995).

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

**Debt Issuance.** The results of estimating equation (3) for the debt issued *after* the referendum are presented in columns (5) and (6) of Table 9. These results provide little support for an appointive effect on debt issuance policies. 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

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<sup>30</sup>The 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 14%.

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.

## 7 Conclusion

This paper has documented that there are meaningful effects from bureaucratic control of city debt policy: assigning the debt task to a bureaucrat reduces city borrowing costs. The results demonstrate that appointive treasurers reduce a city's cost of borrowing. 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 a figure on the order of \$40 million per year. Thus, this paper has demonstrated that alternative political institutions do indeed cause meaningful differences in policymaking.

Importantly, the results from the two empirical approaches are very similar. The fact that the linear regression and the regression discontinuity approaches yield such similar results indicates that institutional choices are not strongly related to unobservable determinants of policymaking in the context of city treasurers. Of course, the endogenous choice of institutions may be a more substantial concern in other contexts, especially where policymaking has a strong distributional component. A more definite conclusion awaits further evidence from other contexts.

I also present evidence that the appointive effect is primarily due to different debt refinancing, not different debt issuance, policy choices. As both treasurer effort and expertise are required to refinance existing debt at a lower cost, the evidence for a debt refinancing channel indicates an important role for treasurer supplied inputs in explaining the findings.

There are several promising directions for future research. First, it would be interest-

ing to examine whether similar differences in policymaking are found for less technical policy choices. As 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 policymaker 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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## 9 Appendix A: A Simple Model of Performance by Bureaucrat and Politician Treasurers

To guide the empirical analysis, this appendix outlines a simple model of the effect on debt management policy of assigning the city treasurer task to a bureaucrat or politician. The model applies the agency framework developed by Alesina and Tabellini (2007) to the context of city treasurers and debt management policy.

The performance of a city treasurer is measured by the interest rate that a city pays on outstanding debt. City residents prefer to pay lower interest rates so that their tax burden is reduced or other public services they value are provided. The policy outcome – the interest rate paid on city debt,  $i$  – is observable but not contractible, so society cannot write the optimal performance contract with the city treasurer to achieve the first-best effort level. Society can, however, choose whether to allocate the city treasurer task to a bureaucrat or politician.<sup>31</sup>

The effort and ability of a city treasurer affect the interest rate that a city pays to borrow. In addition, the effort and ability of a treasurer determine borrowing costs through information collection and utilization in debt policy making, as well as through improved financial management and the hiring of appropriate financial advisors. Borrowing costs of a city are also determined by the exogenous default risk of a city. Default risk is determined by the level and stability of the tax base, as well as the level of debt and public spending of a city.

**Environment.** Consider the policy outcome,  $i$ , the interest rate paid on city debt, which depends on the effort level chosen by the treasurer,  $a$ , and their ability,  $\theta$ , and the exogenous default risk of a city,  $D$ . Citizens prefer to have less spent on debt interest, and obtain utility from policy performance according to  $u = U(i) = -i$ . Policy performance is expressed in the following way:

$$i = D - \theta - a,$$

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<sup>31</sup>It is also assumed that the participation constraint for both types of policymakers is satisfied, but need not bind. Alesina and Tabellini (2007) note that since an optimal contract is not being considered policymakers could be earning additional rents in equilibrium.

where  $\theta$  is the random ability of the treasurer to execute the policy task, and  $\theta$  is uniformly distributed over  $[\bar{\theta} - \frac{x}{2}, \bar{\theta} + \frac{x}{2}]$ . The interest rate on city debt falls with the ability and effort supplied by the treasurer, and increases with the default risk of the city. The complexity of the debt management task is reflected by  $x$ , the more complex the task the greater the uncertainty policy makers face about their ability to execute the task. Treasurers decide how much effort,  $a$ , to supply by maximizing their utility,

$$V(a) = R(a) - C(a),$$

where  $R(a)$  is the reward function for effort, and  $C(a)$  is the cost of effort. The reward function for effort differs between politician treasurers,  $R^P(a)$ , whose goal is to be reelected, and bureaucrat treasurers,  $R^B(a)$ , whose goal is to increase future employers perception of their talent,  $\theta$ . The cost of effort function is the same for both types of treasurers,  $C(a) = a^2$ .

The timing of events is that, first the policy is assigned to either a politician or bureaucrat treasurer. Next, the treasurer chooses effort,  $a$ , before knowing their ability,  $\theta$ . Lastly, nature chooses  $\theta$  and policy outcomes are observed and the rewards are paid.

**The Bureaucrat Treasurer.** The reward function for bureaucrat treasurers is based on expected talent conditional on performance and the market value of ability. The reward function for bureaucrat treasurers is  $R^B(a) = \alpha E(E(\theta|i))$ , where  $E$  denotes the unconditional expectations over the random variable  $i$ , and  $E$  denotes the conditional expectations over  $\theta$  conditional on the realization of performance,  $i$ . The parameter  $\alpha$  captures the valuation bureaucrats place on their performance either due to the value that the current or future employers place on performance, or due to intrinsic motivation that they have for perceived performance. Denoting the public's perception of effort as  $a_e^B$  we have  $R^B(a) = \alpha E(-i - a_e^B + D) = \alpha E(\theta + a - a_e^B)$ .

We can now compute the equilibrium level of effort for bureaucrat treasurers. Taking the first order condition of  $V^B(a)$  with respect to effort, and then imposing the equilibrium condition  $a = a_e$  we obtain equilibrium effort for bureaucrat treasurers,  $a^B = \frac{\alpha}{2}$ . Thus, the equilibrium interest rate paid on city debt when the treasurer is a bureaucrat is given by,

$$(4) \quad i^B = D - \bar{\theta}^B - \frac{\alpha}{2},$$

where  $D$  is the default risk of the city,  $\bar{\theta}^B$  is the average ability of bureaucratic treasurers, and  $\frac{\alpha}{2}$  is the equilibrium effort of bureaucratic treasurers.

**The Politician Treasurer.** The reward function for politician treasurers is based on the value of the office and probability of reelection, so that  $R^P(a) = \beta \Pr(u \geq W)$ , where  $\Pr(u \geq W)$  is the probability of reelection. The parameter  $\beta$  reflects how valuable the office is to politicians due to ego rents that they receive. Politicians are reelected when their constituents' utility,  $u = -i$ , under the politician is greater than utility from the policy performance of the average politician exerting equilibrium effort, so that  $W = u(i^P) = -D + \bar{\theta} + a_e^P$ . Thus,  $R^P(a) = \beta[1 - \Pr(\theta \geq W - a + D)] = \beta[1 - \Pr(\theta \geq \bar{\theta} + a_e^P - a)]$ . Using the uniform distribution of ability, above, we obtain  $R^P(a) = \beta[\frac{\bar{\theta} + a_e^P - a}{x}]$ . Like bureaucrats, politicians also choose effort before observing their talent.

We can now compute the equilibrium level of effort for politician treasurers. Taking the first order condition of  $V^P(a)$  with respect to effort, and then imposing the equilibrium condition,  $a = a_e^P$ , we obtain equilibrium effort for elected treasurers,  $a^P = \frac{\beta}{2x}$ . Thus, equilibrium effort for politician treasurers increases with the value of the office,  $\beta$ , and falls with the complexity of the debt management task,  $x$ . The equilibrium interest rate paid on city debt when treasurers are politicians is then given by,

$$(5) \quad i^P = D - \bar{\theta}^P - \frac{\beta}{2x},$$

where  $D$  is the default risk of the city,  $\bar{\theta}^P$  is the average ability of politician treasurers, and  $\frac{\beta}{2x}$  is the equilibrium effort of politician treasurers.

**Appointive Effect.** We can obtain an expression for the appointive effect for a city by taking the difference between (1) and (2). Doing so we obtain,

$$(6) \quad \Delta i = i^B - i^P = - \left( \bar{\theta}^B - \bar{\theta}^P + \frac{\alpha}{2} - \frac{\beta}{2x} \right).$$

Equation (3) demonstrates that the sign of the appointive effect is ambiguous. The appointive effect depends crucially on the sign and magnitude of both the expertise effect,  $\bar{\theta}^B - \bar{\theta}^P$ , and the effort effect,  $\frac{\alpha}{2} - \frac{\beta}{2x}$ , of bureaucratic versus political control. The central goal of the empirical analysis is to establish the sign and magnitude of  $\Delta i$ .

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 (%)	1.64 (1.86)	2.13 (2.21)	-3.28 [0.001]
<u>(2) City Finances (2000 \$):</u>			
Total Expenditure per capita	1248 (1757)	1328 (1088)	-0.56 [0.579]
Total Debt Outstanding per capita	1035 (1851)	1315 (1446)	-1.86 [0.063]
Total Local Source Revenue per capita	627 (1201)	604 (782)	0.21 [0.833]
<u>(3) City Government Characteristics:</u>			
Mayor-Council Form of Government	0.08 (0.26)	0.20 (0.40)	-3.14 [0.040]
Appointed City Clerk	0.91 (0.25)	0.12 (0.33)	24.95 [0.000]
Fraction with Full-Time Elected Officials	0.04 (0.18)	0.01 (0.09)	1.70 [0.090]
<u>(4) City Economic Characteristics:</u>			
City Population	31628 (35462)	30297 (31870)	0.34 [0.731]
Per Capita Income (1990 \$)	19914 (13932)	15178 (8225)	3.69 [0.000]
Fraction College Graduate	0.25 (0.18)	0.18 (0.13)	4.04 [0.000]
Employment-Population Ratio	0.46 (0.09)	0.44 (0.09)	1.97 [0.050]

Number of Cities	168	133
Number of Observations	2914	2203

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: OLS Estimates

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Treasurer Appointed	-0.49** (0.15)	-0.36** (0.15)	-0.32** (0.14)	-0.58** (0.26)
Year Fixed Effects	Yes	Yes	Yes	Yes
1990 Census Controls	No	Yes	Yes	No
Fiscal Policy Controls	No	No	Yes	No
City Fixed Effects	No	No	No	Yes
Number of Observations		5117		
Number of Cities		301		
Baseline Dependent Variable Mean [Standard Deviation]		1.85 [2.03]		

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 and positive interest expenses in each year. The variable *Treasurer Appointed* takes a value of one if the treasurer is appointed in city  $i$  in year  $t$ . The unit of observation is city-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. Models including city characteristics from the 1990 Census include: *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*. \* 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: The Effect of Appointive City Clerks on City Borrowing Costs: OLS Estimates

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Clerk Appointed	-0.39** (0.15)	-0.22 (0.15)	-0.19 (0.14)	-0.19 (0.31)
Year Fixed Effects	Yes	Yes	Yes	Yes
1990 Census Controls	No	Yes	Yes	No
Fiscal Policy Controls	No	No	Yes	No
City Fixed Effects	No	No	No	Yes
Number of Observations		5117		
Number of Cities		301		
Baseline Dependent Variable Mean [Standard Deviation]		1.85 [2.03]		

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 and positive interest expenses in each year. The variable *Treasurer Appointed* takes a value of one if the treasurer is appointed in city  $i$  in year  $t$ . The unit of observation is city-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. Models including city characteristics from the 1990 Census include: *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*. \* 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 Treasurers on City Borrowing Costs: RD Estimates

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	RD (1)	RD (2)
Treasurer Appointive Referendum Pass	-0.87** (0.33)	-0.67** (0.31)
Number of Observations		255
Number of Cities		31
Baseline Dependent Variable Mean [Standard Deviation]		2.96 [2.34]
Year Fixed Effects	Yes	Yes
1990 Census Controls	No	Yes

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 with a treasurer appointive referendum voted on. The variable *Future Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed 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. Models including city characteristics from the 1990 Census include: *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*. \* 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: RD Estimates, Smoothness Tests

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	RD (1)	RD (2)
<i>Panel A: Interest Rates Prior to Referendum</i>		
Future Treasurer Appointive Referendum Pass	-0.46 (0.76)	-0.12 (0.52)
Number of Observations		352
Number of Cities		31
Baseline Dependent Variable Mean [Standard Deviation]		2.96 [2.34]
<i>Panel B: Appointive Clerk Referendum</i>		
Clerk Appointive Referendum Pass	-0.33 (0.39)	-0.52 (0.43)
Number of Observations		205
Number of Cities		30
Baseline Dependent Variable Mean [Standard Deviation]		3.57 [4.72]
Year Fixed Effects	Yes	Yes
1990 Census Controls	No	Yes

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 trend in vote share. \* 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 Borrowing Costs: RD Estimates, Alternative Specifications and Samples

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	RD (1)	RD (2)
<i>Panel A: Narrow Margin of Victory Sample</i>		
Treasurer Appointive Referendum Pass	-1.15** (0.56)	-1.22*** (0.22)
Number of Observations		161
Number of Cities		20
Baseline Dependent Variable Mean [Standard Deviation]		2.97 [2.34]
<i>Panel B: City Fixed Effects</i>		
Treasurer Appointive Referendum Pass	-0.75** (0.20)	--
Number of Observations	255	--
Number of Cities	31	--
Baseline Dependent Variable Mean [Standard Deviation]	2.96 [2.34]	--
<i>Panel C: Drop Cities With Multiple Referendums from Sample</i>		
Treasurer Appointive Referendum Pass	-0.76* (0.39)	-0.56 (0.39)
Number of Observations		197
Number of Cities		27
Baseline Dependent Variable Mean [Standard Deviation]		2.44 [2.27]
<i>Panel D: Drop Interest Rate Outliers from Sample</i>		
Treasurer Appointive Referendum Pass	-0.79** (0.32)	-0.59** (0.32)
Number of Observations		243
Number of Cities		31
Baseline Dependent Variable Mean [Standard Deviation]		2.96 [2.34]
Year Fixed Effects	Yes	Yes

1990 Census Controls

No

Yes

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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 with a treasurer appointive referendum voted on. The variable *Future Treasurer Appointive Referendum Pass* takes a value of one if a local treasurer appointive referendum passed 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. Models including city characteristics from the 1990 Census include: *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 trend in vote share. \* 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 City Fiscal Policy, OLS and RD Estimates

Model =	OLS (1)	RD (2)	RD (3)
<i>A. Dependent Variable = Real Total Expenditure per capita</i>			
Treasurer Appointed	-54.92 (158.84)		
Treasurer Appointive Referendum Pass		140.79 (227.64)	166.04 (270.29)
Number of Observations	5177	255	
Number of Cities	301	31	
Baseline Dependent Variable Mean [Standard Deviation]	1283 [1506]	934 [393]	
<i>B. Dependent Variable = Real Total Local Source Revenue per capita</i>			
Treasurer Appointed	16.47 (123.07)		
Treasurer Appointive Referendum Pass		108.57* (60.00)	86.19 (74.02)
Number of Observations	5177	255	
Number of Cities	301	31	
Baseline Dependent Variable Mean [Standard Deviation]	647 [1042]	403 [167]	
<i>C. Dependent Variable = Real Total Outstanding Debt per capita</i>			
Treasurer Appointed	-270.78 (166.73)		
Treasurer Appointive Referendum Pass		-139.17 (542.71)	-23.48 (624.41)
Number of Observations	5177	255	
Number of Cities	301	31	
Baseline Dependent Variable Mean [Standard Deviation]	1156 [1735]	831 [806]	
Year Fixed Effects	Yes	Yes	Yes
1990 Census Controls	Yes	No	Yes
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 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 trend in vote share. \* 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 Other Borrowing Costs: OLS and RD Estimates

Dependent Variable = Interest Rate on Total Outstanding Debt

Model =	OLS (1)	RD (2)	RD (3)
<i>A. Dependent Variable = Expenditure on Debt Consultants per capita</i>			
Treasurer Appointed	-9.93 (12.56)		
Treasurer Appointive Referendum Pass		-19.30 (47.48)	-17.37 (40.48)
Number of Observations	5177	255	
Number of Cities	301	31	
Baseline Dependent Variable Mean [Standard Deviation]	26.47 [173.27]	21.25 [62.71]	
<i>B. Dependent Variable = Expenditure on Management and Support Department per capita</i>			
Treasurer Appointed	12.57 (22.39)		
Treasurer Appointive Referendum Pass		22.10 (35.09)	65.51** (25.16)
Number of Observations	5117	255	
Number of Cities	301	31	
Baseline Dependent Variable Mean [Standard Deviation]	129.88 [219.19]	67.39 [43.78]	
Year Fixed Effects	Yes	Yes	254
1990 Census Controls	Yes	No	No
<b>Sample</b>	<b>All Cities</b>	<b>Referendum Cities</b>	

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 trend in vote share. \* 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 9: 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

Debt Issues = Model =	All Debt Issues		Debt Issued Before Referendum		Debt Issued After Referendum	
	RD (1)	RD (2)	RD (3)	RD (4)	RD (5)	RD (6)
Treasurer Appointive Referendum Pass	-0.72 (0.57)	-0.89 (0.57)	-1.33* (0.76)	-1.56** (0.78)	0.58 (0.71)	0.05 (0.70)
Number of Observations	205	205	129	129	76	76
1990 Census Controls	No	No	No	No	No	No
Debt Issue Characteristic Controls	No	Yes	No	Yes	No	Yes
Baseline Dependent Variable:						
Mean	5.06		5.33		4.45	
[Standard Deviation]	[1.38]		[1.41]		[1.08]	
{Min, Max}	{0.92, 9.31}		{0.92, 9.31}		{1.75, 6.86}	
Sample	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 (1) and (2) also control for a third order polynomial in vote share. 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

<b>Date</b>	<b>City</b>	<b>Vote Yes</b>	<b>Vote No</b>	<b>Total Votes</b>	<b>Percent in Favor</b>	<b>Referendum Pass</b>	<b>Joint Referendum with Appointive Clerk</b>
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	Yes
2-Nov-04	Waterford	990	779	1769	0.56	Yes	
8-Nov-05	Pittsburg	4319	6016	10335	0.42	Yes	

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

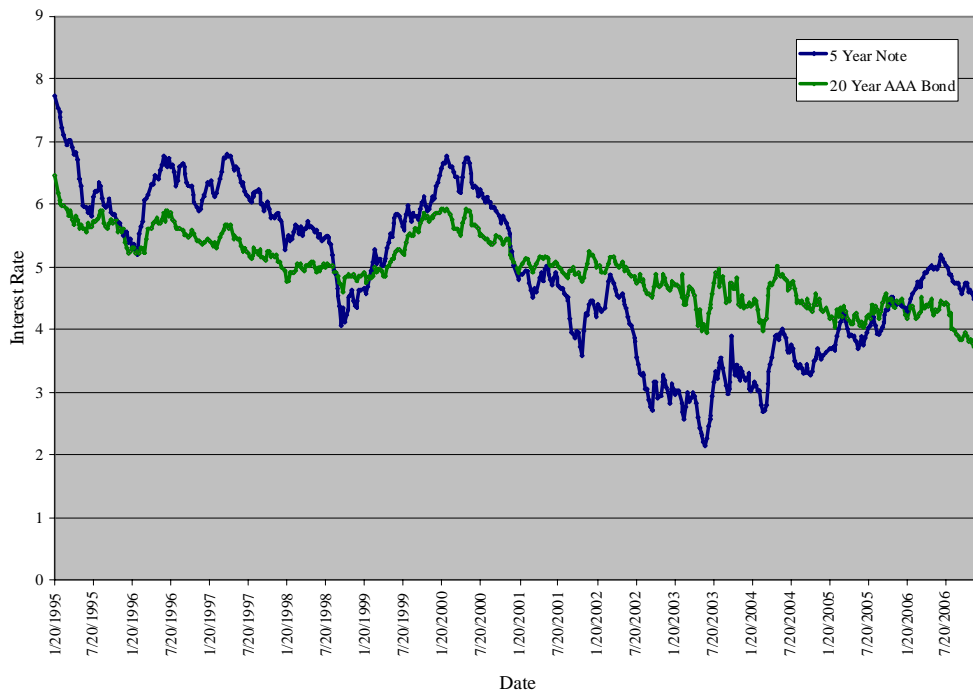
TABLE A2: Descriptive Statistics in 1992, by City Treasurer Appointive Referendum Status

	<u>Treasurer Elected in 1992:</u>		(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)	1.89 (2.17)	1.34 [0.183]
<u>(2) City Finances (2000 \$):</u>			
Total Debt Outstanding per capita	830 (805)	1031 (1680)	-0.91 [0.364]
Total Expenditure per capita	934 (394)	1022 (793)	-0.81 [0.422]
Total Local Source Revenue per capita	403 (167)	500 (551)	-1.56 [0.120]
<u>(3) City Government Characteristics:</u>			
Mayor-Council Form of Government	0.19 (0.40)	0.21 (0.41)	-0.15 [0.881]
Elected City Clerk	0.10 (0.30)	0.12 (0.32)	-0.33 [0.739]
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)	29749 (31342)	0.28 [0.784]
Per Capita Income (1990 \$)	13166 (4543)	15655 (8959)	-2.07 [0.040]
Fraction College Graduate	0.15 (0.09)	0.19 (0.14)	-1.87 [0.064]
Employment-Population Ratio	0.43	0.45	-0.88

	(0.07)	(0.09)	[0.381]
Number of Observations	31	102	

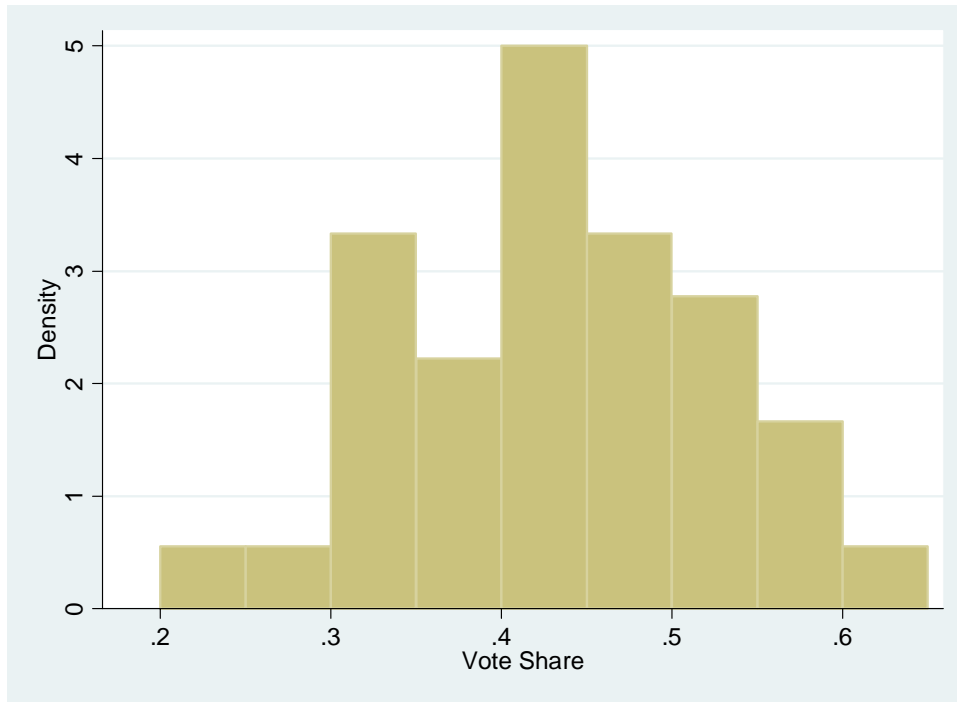
Notes: Source: Author's calculations using data from California CFTR, and CEDA data from 1992 to 2008, 1990 Decennial Census, and 1992 Census of Government data. The unit of observation is city. The main entries in column (1) present the mean of the selected variables for cities with elected treasurers 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.

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.

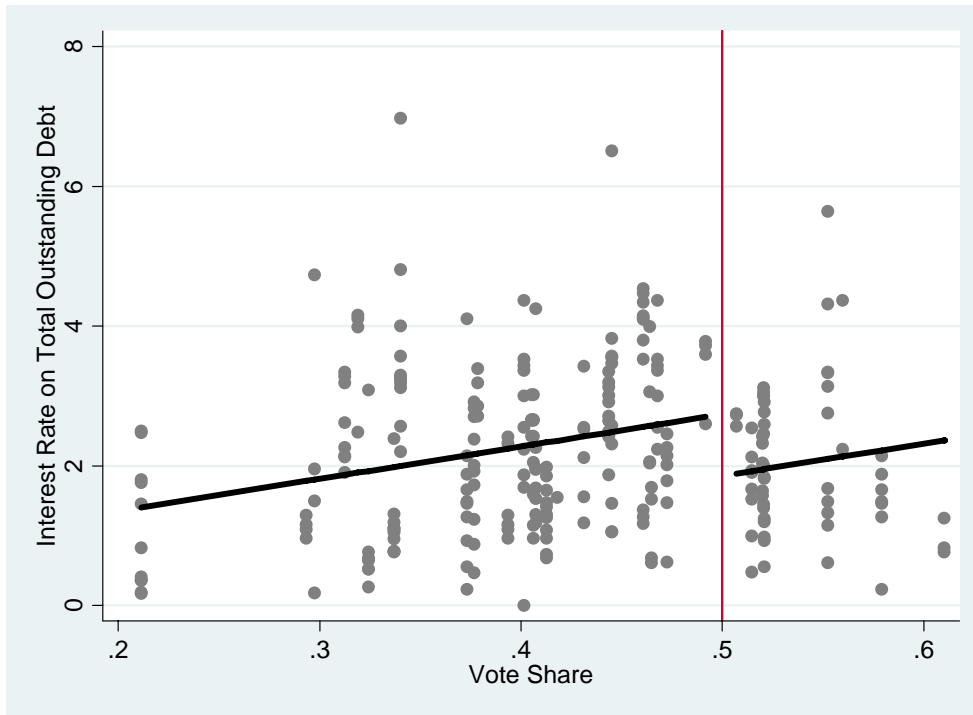
FIGURE 2: Appointive Treasurer Referendum Vote Distribution



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

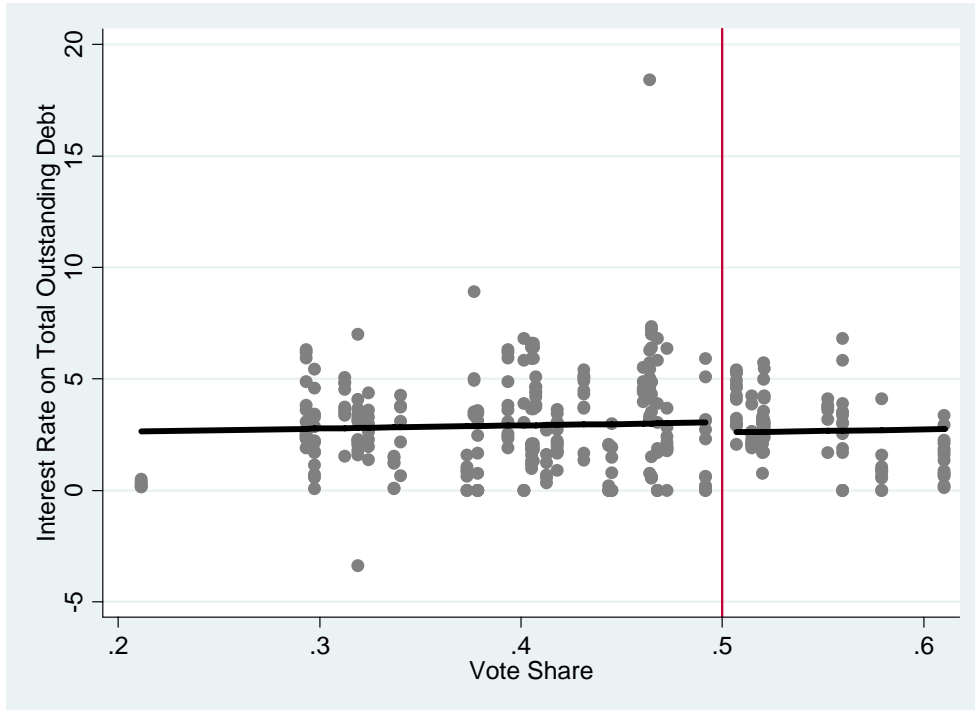


FIGURE 3: Treasurer Appointive Effect



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 trend 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 trend in referendum vote share.