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How Decisive Is the Decisive Voter?

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Abstract

This paper examines whether the voter with the median income is decisive in local spending decisions. Previous tests have relied on cross-sectional data while we make use of a pair of California referenda to estimate a first difference specification. The referenda proposed to lower the required vote share for passing local educational bonding initiatives from 67 to 50 percent and 67 to 55 percent, respectively. The jurisdiction median income appears to accurately capture the expected outcomes of majority votes on public service spending, and voters rationally consider such future public service decisions when deciding how to vote on voting rules

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1. Introduction

The median voter model has a long theoretical and empirical history within public economics. Since the pioneering work of Bergstrom and Goodman (1973), which established the conditions under which the median voter is also the voter with the median income, hundreds of studies have used the median voter framework to estimate demands for publicly provided goods and services. The enduring popularity of the median voter model stems both from its simplicity and its analytic tractability. As noted by Inman (1978), if governments act "as if" to maximize the preferences of the median income voter, the median voter hypothesis provides "a powerful starting point for predictive and normative analysis of government behavior."

Despite the wide spread popularity of the median voter model, a number of the key assumptions that make the model so tractable have been repeatedly challenged. In particular, empirical application of the model typically requires three strong assumptions. First, one must assume the political process leads to public services being delivered that satisfy the preferences of the median voter (the key implication of the model itself). That assumption is unlikely to hold when preferences are not single peaked (e.g. Stiglitz 1972; Mckelvey 1976), when elected officials seek to maximize their budgets (e.g. Niskanen 1975; Romer and Rosenthal 1979b, 1982) or when elected officials act strategically due to differential voter turnout (e.g. Hastings, Kain, Staiger, and Weinstein 2007). Second, one must assume that median income in a community provides a reasonable proxy for the identity of the median voter. That assumption may not hold when there are private alternatives to public goods (e.g. Epple and Romano 1996) or in the presence of Tiebout sorting (e.g. Goldstein and Pauly 1981; Ross and Yinger 1999). Finally, one must assume public spending is a reasonable proxy for the level of publicly provided services; an assumption that has been challenged by Behrman and Craig (1987) and Schwab and Zampelli (1987). If any of these three assumptions fail to hold, the key proposition that governments make spending decisions "as if" to maximize the preferences of the median income voter is unlikely to hold.[1]

Consequently, numerous studies have attempted to test whether the voter with the median income is empirically relevant for describing local public service provision including Pommerehne and Frey (1976), Pommerehne (1978), Inman (1978), Deno and Mehay (1987), Turnbull and Djoundourian (1994), Aronsson and Magnus (1996), Turnbull and Mitias (1998), and Turnbull and Chang (1998). While these studies employ different datasets and different methodologies, they nevertheless share a common feature: they all attempt to test the median voter hypothesis using the same framework that is typically used to estimate demand for publicly provided services. Specifically, all the aforementioned studies utilize

[1] See Holcombe (1989) for a review of the criticisms and concerns surrounding the median voter model, and see Wildasin (1986) for an extended discussion of the assumptions required for the median voter model to be applied empirically.

aggregate cross-sectional data to identify a relationship between public service expenditure levels and a community's median income. Because, these studies utilize public expenditures as a proxy for the level of publicly provided services, they are subject to the same criticisms that surround studies that utilize public expenditures to estimate demands for public goods; namely, that public expenditures may be a poor proxy for the actual services demanded by residents. Furthermore, these studies are likely biased because communities differ across a variety of dimensions including unobserved preferences for public services, the cost of providing public services, voter turnout, etc; and these differences are likely correlated with the distribution of income in each community.

In this paper, we propose an entirely new approach for testing the median voter hypothesis. We examine vote returns from a unique pair of California referenda that proposed changing the rules under which public spending decisions are determined. Specifically, the first referendum proposed to lower the required vote share for passing local educational bonding initiatives from 67 to 50 percent, and the second referendum, which was held only eight months later, proposed lowering the vote requirement from 67 to 55 percent. Thus, the first referenda would have changed the identity of the decisive voter from the voter in the 33rd percentile of the income distribution to the 50th percentile while the second referenda would have changed the identity of the decisive voter from the voter in the 33rd percentile to the voter in the 55th percentile. Using the results from these two referenda, we test whether people vote "as if" future spending decisions will be based upon the preferences of the newly proposed decisive voter by examining whether the change in the fraction of 'yes' votes cast in the two elections can be explained by the implied change in the newly proposed decisive voter's income (i.e. the difference between the 50th and 45th percentile income in a jurisdiction). We also test an additional implication of referenda voting models, namely that the influence of the decisive voter's preferences on vote outcomes declines with jurisdiction heterogeneity (Romer, Rosenthal and Munley 1992; Rothstein 1994).

Unlike previous tests of the median voter hypothesis, where public service spending is used to infer a relationship between the median voter's preferences and outcomes of the political process, our test infers that a median voter relationship holds because voters act as if the relationship holds when they cast their ballots to determine voting rules for choosing the level of public services provided. Consequently, our test avoids the fundamental problem of measuring the actual services demanded by voters within a jurisdiction which may be poorly proxied by the measures used in previous studies, such as expenditures per capita. Furthermore, by regressing changes in the fraction of 'yes' votes between the referendums on changes in the income associated with the decisive voter in each district, we are able to difference out school district unobservables that are likely correlated with the distribution of income within a district. We are also able to control for a variety of other factors that might have influenced changes in the fraction

of 'yes' votes between the two referenda including changes in the tax price of the decisive voter, changes in the size and composition of voter turnout, and district size.

Consistent with theory, we find a strong positive relationship between the change in the decisive voter's income and the change in the fraction of 'yes' votes between the two referenda.[2] Our estimates imply that the change in the decisive voter's income led to somewhere between a 1.6 and 2.6 percentage point increase in the percent voting yes for the school districts in our sample. This compares to an actual increase in the percent voting yes of 4.3 percentage points. Furthermore, based on constant elasticity of demand models, with and without controls for jurisdiction differences in preference heterogeneity, we obtain income elasticities of between 0.72 and 0.85, while traditional demand models based on California education capital spending have found income elasticites between 0.70 and 0.77 (Balsdon, Brunner, and Rueben, 2003).[3] We also find that the relationship between the decisive voter's demand and support for voting rules is weaker in more heterogeneous districts, a finding consistent with the theoretical and empirical results found in Romer, Rosenthal and Munley (1992), Rothstein (1994) and Gerber and Lewis (2004). Finally, we find that the relationship between the income of the jurisdiction's decisive voter and the likelihood of supporting a referendum does not hold for two counterfactuals estimated by replacing school districts with alternative definitions of jurisdiction based on census tracts and state assembly districts.

A key limitation of our analysis arises from the fact that the difference in the 50^{th} and 45^{th} percentile incomes is highly correlated with the difference in income between other percentiles, such as the difference between the 60^{th} and 40^{th} or the 80^{th} and 20^{th}. In our data, we cannot statistically distinguish between the influence of changes in the decisive voter's income and the influence of other measures of the income spread on voting patterns. This data limitation cuts two ways. Naturally, we cannot definitively show that households are responding to changes in the decisive voter's income, as opposed to other differences in the income distribution. On the other hand, this correlation also lowers the sophistication and information required for voters to respond rationally to changes in the vote share requirements. Specifically, voters only need to know something about the income spread in the community without knowing the exact change in income for the decisive voter. In addition, we examine

[2] A larger decline between the 50^{th} and 45^{th} percentile income implies a larger decline in the increase in education spending that voters can expect from moving the current vote share of two-thirds to the new requirement of 50 or 55. Consequently, more voters are willing to support the second referendum because it implies a smaller increase in spending.

[3] We also include a control for the tax price of the decisive voter, which is standard in empirical models of local public spending. We find no evidence of any relationship between the change in the tax price of the decisive voter and the change in the share voting yes. This finding is not surprising given that tax prices depend on the value of commercial property in a community and the productive efficiency of local government, both of which are difficult for voters to observe. Furthermore, unlike most other states, residential property assessments in California are based on past sales prices rather than market value, making it even more difficult for voters to discern the tax price facing the decisive voter.

traditional measures of income inequality, which are not so highly correlated with the change in the income of the decisive voter and find that after controlling for the change in the decisive voter's income, other measures of the distribution of income do not explain changes in voting between the two referenda. In other words, the change in the decisive voter's income is sufficient to explain changes in voting. Further, in models where we replace the income of the decisive voter with other income percentiles, the ability of the income inequality measures to explain voting typically increases, often becoming statistically significant as the percentiles move away for the decisive voter's percentile.

Regardless, our findings are consistent with a world where jurisdiction median income accurately captures the expected outcomes of majority votes on public service spending, and voters understand the impact of those changes on future spending decisions. In addition to providing evidence supporting applications of the median voter model to local public service provision, this paper provides unique evidence that voters rationally and accurately consider the specific impact of voting rules on local spending when voting. Finally, our finding that the influence of the decisive voter's demand on the determination of voting rules is weaker in more heterogeneous districts provides empirical support for the referenda voting models developed by Romer, Rosenthal and Munley (1992) and Rothstein (1994). Empirical support for referenda models of this type is especially important given that these models predict less electoral support for public spending initiatives in the presence of greater voter heterogeneity; a prediction that is consistent with empirical findings of Alesina, Baqir and Easterly (1999).

2. Review of the Literature on the Median Voter Model

Over the last several decades hundreds of studies have used the median voter model to estimate demands for publicly provided goods and services.[4] The vast majority of those studies use aggregate cross sectional data to identify a relationship between public service expenditure levels and a community's median income.[5] Consequently, these studies either implicitly or explicitly rely on the results of Bergstrom and Goodman (1973) that show that, "subject to certain strong assumptions, majority rule implies that one can treat an observation of expenditure levels in a given jurisdiction as a point on the demand curve of a citizen of that community with median income for the community" (Bergstrom Rubinfeld and Shapiro, 1982, p. 1184).

[4] A review of older studies that use the median voter framework to estimate demand can be found in Inman (1979). A few of the more recent studies include, Rothstein (1992), Silva and Sonstelie (1995), Stevens and Mason (1996), and de Bartolome (1997) for school spending, Schwab and Zampelli (1987) for police, Duncombe (1991) for fire, Balsdon, Brunner and Rueben (2003) for local general obligation bond issues, and Husted and Kenny (1997) for expansion of the voting franchise.

[5] A smaller set of studies, including Bergstrom, Rubinfeld and Shapiro (1982), Gramlich and Rubinfeld (1982) and Rubinfeld, Shapiro and Roberts (1987), use individual-level survey data to estimate demand for publicly provided goods and services. See Rubinfeld (1987) for a review of these types of studies.

Despite (or possibly because of) the wide spread popularity of the median voter model, the assumptions required for the model to hold have been repeatedly questioned. Preferences may not be single peaked when voters have preferences over multiple issues (McKelvey 1976) or when private alternatives exist (Stiglitz 1972; Epple and Romano 1996). Politicians and bureaucrats may use their ability to set the political agenda in order to maximize their budget (Niskanen 1975; Romer and Rosenthal 1979a, 1979b, 1982; Romer Rosenthal, and Munley 1992; Balsdon, Brunner, and Ruben 2003), or they may make decisions based on their party's or their own personal ideology (Levitt 1996; Reed 2006; Washington 2008). Similarly, politicians may have an incentive to act strategically (and in ways that deviate from the preferences of the median voter) because voter turnout may be influenced by differential voter reactions to their past actions (Hasting, Kain, Staiger, and Weinstein 2007) or by the media (Gentzkow 2006; DellaVigna and Kaplan 2007).

Further, the standard assumption that the median voter is also the voter with the median income has been repeatedly questioned. As demonstrated by Epple and Romano (1996), when there are private alternatives to public services (e.g. private schools), an equilibrium exists where the median income voter is not pivotal. Instead, the pivotal voter has an income that lies below the median. Similarly, most of the studies that estimate demand for local public goods have ignored the issue of Tiebout sorting, in which households choose communities based in part upon their demand for public services. Such sorting induces well-known biases on estimated public service demand parameters in cross sectional studies and also calls into question whether the median voter is also the voter with the median income.[6] As noted by Ross and Yinger (1999), with Tiebout sorting communities may contain both higher income households with weak preferences for public services and lower income households with strong preferences for public services. Consequently, the median preference voter may not be the voter with the median income.[7]

Finally, nearly all empirical studies that utilize the median voter framework suffer from the fundamental problem of measuring the actual services demanded by voters. The vast majority of studies use community-level expenditures to infer a relationship between the median voter's preferences for publicly provided services and outcomes of the political process. However, as noted by Behrman and Craig (1987), "… people pay taxes based on the city-wide amount of purchased inputs, but base their demand and voting behavior on the perceived level of neighborhood service output" (Behrman and Craig, 1987, p. 47). To the extent that the services produced differ substantially across jurisdictions given the same public inputs, public spending will provide a poor proxy for public service provision. Furthermore,

[6] See Goldstein and Pauly (1981) for a nice discussion of the implications of Tiebout sorting on estimated public service demand parameters.

[7] Epple and Sieg (1999) estimate a structural model that allows for preference heterogeneity and enables them to estimate income and price elasticities in a model that explicitly identifies the median preference voter.

unobserved community characteristics that influence the cost of providing public services are likely to be correlated with other community characteristics like median income.[8] As a result, studies that fail to properly control for the costs associated with providing public services are likely to be biased. That fact is highlighted by Schwab and Zampelli (1987) who find that studies of public service demand that fail to take into account the impact of community characteristics on the cost of public service provision can yield very misleading results.

Given all of the aforementioned caveats and concerns, it is not surprising that since the mid-1970's numerous studies have attempted to test whether the voter with the median income is truly pivotal. Pommerehne and Frey (1976), Pommerehne (1978), Inman (1978), Deno and Mehay (1987), Turnbull and Djoundourian (1994) and Turnbull and Mitias (1998) evaluate the performance of the median voter model by examining whether the use of median income in local public service demand regressions outperforms other specifications (such as replacing median income with mean income). The results of those studies generally support the hypothesis that the median income voter is decisive. Similarly, using a revealed preference approach, Turnbull and Chang (1998) find that local governments act "as if" to maximize the utility of the median income voter. On the other hand, Romer and Rosenthal (1979) argue that most studies that utilize the median voter framework, "fail to identify whether the median voter is pivotal or a voter at some other fractile is pivotal." That conclusion is supported by Aronsson and Magnus (1996) who test the predictive power of a model where the median income voter is assumed to be decisive against a more general statistical alternative. Their results lead them to reject the hypothesis that the voter with the median income is decisive. More recently, Gerber and Lewis (2004) utilize a unique dataset consisting of individual-level voting data to estimate the distribution of voter preferences within legislative districts located in Los Angeles County and then use those estimates to explain actual role call votes for federal and state legislators. They find strong support for the hypothesis that legislators from homogeneous districts represent the preferences of the median voter. In heterogeneous districts however, the authors find that the influence of the median voter is weaker and legislators tend to respond more to party pressure.[9]

While the majority of the studies listed above provide support for the median voter model, substantial skepticism remains about the model's empirical relevance primarily due to concerns about the validity of the aggregate analyses that support it. In particular, a common feature of nearly all prior studies that test the median voter hypothesis is that they rely on aggregate cross-sectional data to identify the relationship between public service outcomes and a community's median income. As previously

[8] See Ross and Yinger (1999) for a survey of studies that document cost heterogeneity across jurisdictions, as well as recent additional studies by Duncombe and Yinger (2005) and Reschovsky and Imazeki (2003).
[9] See Kalt and Zupan (1984), Goff and Grier (1993), and Bailey and Brady (1998) for earlier studies that document a link between jurisdiction heterogeneity and legislator's voting behavior.

noted, these studies are likely biased because communities differ across a variety of dimensions including unobserved preferences for public services, the cost of providing public services, voter turnout, etc; and these differences are likely correlated with the distribution of income in each community. Furthermore, prior studies have typically attempted to test the median voter hypothesis using the same framework that is used to estimate demands for publicly provided goods; namely, by examining the relationship between community expenditures and some measure of community income. Consequently, those studies suffer from the same fundamental problem of measuring actual service demands that plagues most studies that utilize the median voter model to estimate demands for publicly provided goods.

3. Conceptual Framework

Prior to 2000, local school bond measures in California required a two-thirds supermajority to pass. If voters approved a bond issue, the bonds were then repaid with local property tax increases that remained in effect until the bonds were fully repaid. In 2000 Californians voted on two statewide initiatives designed to ease this supermajority vote requirement. In March of 2000 Californians voted on Proposition 26, an initiative that would have reduced the vote requirement on school bond measures to a simple majority. The proposition garnered the support of only 47 percent of voters and thus failed. In November of 2000 Californians voted on Proposition 39, an initiative that was nearly identical to Proposition 26 except it called for reducing the vote requirement on local school bond measures to 55%. This time California voters approved the measure with 53 percent of voters voting in favor of Proposition 39.

To motivate our empirical test of the median voter model, we begin by examining the implications of the median voter model for the behavior of voters in a referendum on voting rules. Specifically, we develop a simple voting model based on Romer, Rosenthal and Munley (1992) and Rothstein (1994) in order to illustrate the relationship between support for a change in required vote share and the income of the decisive voter. Let S_{ij}^{*} denote the desired level of school spending of individual i located in school district j. The individual votes in favor of a decrease in the vote share required to pass spending referenda to *P* if and only if S^{P}, the spending level under the new vote share, is preferred to S^{0}, the spending level under the current, higher vote share requirement.

Following Rothstein (1994), we parameterize individual preferences for school spending using the desired spending level S_{ij}^{*} so that an individual's indirect utility function $V(S_j \mid S_{ij}^{*})$ is maximized when a district's spending level is $S_j = S_{ij}^{*}$. Next, we assume that the distribution of preferences in district *j* is distributed around a district mean preference S_j^{*} or

$$S_{ij}^{*} = S_{j}^{*} + \mu_{ij}, \tag{1}$$

where μ_{ij} is a random disturbance. Assuming preferences over public service levels are single peaked, a unique α_j^P exists so that $V(S_j^0 \mid a_j^P) = V(S_j^P \mid \alpha_j^P)$ where V is a voter's indirect utility function conditional on their preferences for the public service.[10] A voter supports the referendum to lower the vote share requirement (presumably increasing public service levels) if and only if $S_{ij}^{*} > \alpha_j^P$. Parameterizing α_j^P as

$$a_j^P = \alpha(S_j^0, S_j^P) + \nu_j = (1-\delta)S_j^0 + \delta S_j^P + \nu_j, \tag{2}$$

where δ is between zero and one and substituting equations (1) and (2) into the inequality above, we find that a voter supports the reduction in the vote share requirement if and only if

$$S_j^{*} - [(1-\delta)S_j^0 + \delta S_j^P] > \nu_j - \mu_{ij}. \tag{3}$$

Let $pyes_j$ denote the fraction of voters in district j that prefer S^P to S^0. If ν_j and μ_{ij} follow independent type 1 extreme value distributions, then the difference has a logistic distribution with a cumulative distribution function based on equation (3) of

$$F(x) = 1 - \left[1 + \exp\left(\frac{x - \left(S_j^{*} - [(1-\delta)S_j^0 + \delta S_j^P]\right)}{\beta}\right)\right] \tag{4}$$

where the variance of the distribution is $\frac{1}{3}\beta^2\pi^2$ and equals the sum of the variances of ν_j and μ_{ij}, σ_ν^2 and σ_μ^2 respectively, assuming that the two disturbances are independent (Johnson, Kotz, and Balakrishnan, 1995). If we assume that β is constant across communities and without loss of generality initialized β to one, the log-odds ratio can by expressed as:

[10] Specifically, we assume that indifference curves are convex over public service levels and property tax rates so that given a well-behaved community budget constraint an individual has a unique preferred level of public service and utility declines as the public service level is increased above or decreased below that preferred level, see Epple and Romano (1996a, 1996b) as well as many other earlier papers that impose such assumptions. This implies that $V(S_j \mid S_{ij}^{*})$ is a concave function of S_j see Rothstein (1994) and Balsdon and Brunner (2005).

$$\ln\left(\frac{pyes_j}{1-pyes_j}\right) = c_0 + c_1 S_j^* - c_2 S_j^0 - c_3 S_j^P + \varepsilon_j , \quad (5)$$

where c_1, c_2, and c_3 are all non-negative as found by Rothstein (1994) and ε_j represents district specific factors that influence voting independent of public service demand.[11] Equation (5) suggests a simple differencing estimation strategy. Specifically, if the first initiative imposed a required vote share of 50% for spending referenda and the second initiative imposed a 55% vote share, the difference in the log-odds of the fraction of voters that support the two initiatives in district j is:

$$\ln\left(\frac{pyes_j^2}{1-pyes_j^2}\right) - \ln\left(\frac{pyes_j^1}{1-pyes_j^1}\right) = -c_3(S_j^{55} - S_j^{50}) + (\varepsilon_{j2} - \varepsilon_{j1}) , \quad (6)$$

where both the unique district mean preference for public service levels, the default level of public service provision, and any idiosyncratic, time invariant district attributes that influence voting drop out of the model.

The assumption that β is constant across communities, however, is quite strong given other assumptions in the model. Specifically, once μ_{ij} is restricted to follow an independent type 1 extreme value distribution, σ_μ^2, the variance of μ_{ij}, must be positively related to the difference between the 50th and 55th percentile demands since only an increase in the variance can create additional spread at the center of the distribution holding the form of the distribution fixed. Therefore, the assumption that β is constant requires that σ_ν^2 falls by the exact same amount as any increase in σ_μ^2. Further, there are reasons to believe that ν_j and μ_{ij} are correlated across communities and that σ_ν^2 might also depend upon factors that influence the heterogeneity of preferences. The disturbance ν_j captures community specific errors describing the indifference point between the proposed and initial spending levels, S^P and S^0, which obviously depends on S^*. Different values of S^P and S^0 fall in different regions of the indirect utility function, with different curvature, and so while S^0 will be differenced out of demand equations and S^P will enter directly into our model, they may still influence the distribution of ν_j.

[11] Equation (5) involves aggregate vote shares. The aggregation from Equation (3), which is based on individual preferences, is accomplished via the assumption of an extreme value distribution for the unobservable associated with the distribution of individual preferences within a jurisdiction and for the unobservable associated with parameterizing α_j^P, the preference level in a community that is indifferent between the referendum passing or failing. This assumption leads to jurisdiction vote shares that depend upon the standard logistic distribution.

Naturally, S^P and S^0 depend upon the same unobservables that influence the distribution of μ_{ij}. We therefore allow for heterogeneity in the distribution of preferences across districts by extending equation (6) as follows:

$$\ln\left(\frac{pyes_j^2}{1-pyes_j^2}\right)-\ln\left(\frac{pyes_j^1}{1-pyes_j^1}\right)=-\frac{c_3(S_j^{55}-S_j^{50})}{\beta_j}+(\varepsilon_{j2}-\varepsilon_{j1}) \qquad (7)$$

where β_j describes the unique standard deviation of preferences for community *j*.

As equation (7) reveals, preference heterogeneity, as measured by β_j, scales the influence that the decisive voter's demand has on referenda vote outcomes. Specifically, equation (7) shows that the influence of the decisive voter's demand on the determination of voting rules is smaller in more heterogeneous districts (i.e. districts with a larger β). This result mirrors the theoretical results of Romer, Rosenthal and Munley (1992) and Rothstein (1994), who demonstrated that, all else equal, greater preference heterogeneity within jurisdictions results in lower approval margins for local referenda, and is consistent with the empirical findings of Gerber and Lewis (2004). In the present context, equation (7) suggests that differences in the desired level of spending of the 50th and 55th percentile voters should have a smaller impact on the change in the fraction of 'yes' votes in districts with significant preference heterogeneity and a greater impact in districts with relatively little preference heterogeneity.

Further, given that the decisive voter in the second referenda has lower income and lower demand for public services, the demand term in equation (7) always implies an increase in voting yes between the first and second referenda, and increases in preference heterogeneity unambiguously reduce the increase in support for the proposal between the two referenda. This result is consistent with the conclusions of Romer, Rosenthal and Munley (1992) in their analysis of referenda that directly establish the level of spending on public goods. Specifically, they conclude that electoral support for spending increases is reduced in heterogeneous communities.[12] In our model, this result can be observed from equation (4) where the impact of support for a proposed level of public services S^P on the likelihood of voting yes is scaled down by preference heterogeneity as captured by β.

4. Empirical Methodology

In order to operationalize equation (6), initially maintaining the assumption that β is constant across communities, we assume that for referendum k the implied future level of public service is a

[12] See Proposition 2 in Romer, Rosenthal and Munley (1992).

function of the income and tax price percentile associated with the vote share required under the referendum. For a 50 percent vote share, the standard median voter assumption applies where the future level of public service is a function of the median income and median tax price in the school district. The tax price, p_j, is the median percentile assessed value divided by the property tax base per service recipient, or $p_j \equiv \left(E_j / G_j\right) \cdot h_j^{50}$, where the first term is the number of service recipients E_j, (in this case number of students in a school district) divided by the jurisdiction grand list of property assessments G_j, and the second term is the 50th percentile assessed value of owner-occupied homes.

For a 55 percent vote share, which is the share associated with the second referendum, the future level of public service is a function of the district's 45th percentile income and tax price. Specifically, a higher vote share for passage decreases the level of public service that will be acceptable to voters and thus shifts the decisive income and tax prices further down the distribution. Assuming a linear form for public service demand yields

$$S_j^{P_k} = S(y_j^{100-P_k}, p_j^{100-P_k}) = b + b_y y_j^{100-P_k} + b_p p_j^{100-P_k}, \quad (8)$$

where P_k is the required vote share for referendum k, $y_j^{100-P_k}$ or y_{jk} for short is the income at the decisive percentile, $p_j^{100-P_k}$ or p_{jk} for short is the tax price at the decisive percentile, and b_y and b_p are parameters describing the responsiveness of demand to income and tax price. Substituting equation (8) into equation (6) for the two referenda and rearranging yields

$$\ln\left(\frac{pyes_j^2}{1-pyes_j^2}\right) - \ln\left(\frac{pyes_j^1}{1-pyes_j^1}\right) = (d_1 - d_2) + d_y(y_{j1} - y_{j2}) + d_p(p_{j1} - p_{j2}) + (\varepsilon_{j1} - \varepsilon_{j2}), \quad (9)$$

where the d_y and d_p parameters are just the b parameters from equation (8) multiplied by the negative term $-c_3$ and the difference between d_2 and d_1 allows the mean of the district unobservable, ε_{jk}, to vary across referenda *k*. The median or decisive voter model predicts that d_y should be positive since public service demand increases with income. Specifically, each referendum is assumed to be supported by all voters who prefer the new higher level of spending to current spending based on the two-thirds vote requirement. A larger income difference between the 50th and 55th percentile voter (i.e. the 50th and 45th percentile income), implies a larger reduction in new education spending or a smaller increase in spending

over current levels, which is then supported by more voters. Similarly, d_p should be negative since public service demand falls with tax price.

By specifying public service demand to be a simple linear function of the decisive voter's income and tax price we are able to derive an estimation equation that is linear in the parameters, which is attractive from an estimation standpoint because unobservables in voting and also potentially in public service demand are differenced away. However, the common practice in the literature is to assume public service demand is characterized by constant income and price elasticities which implies

$$S_j^{P_i} = a\left(y_j^{100-P_i}\right)^{\theta}\left(p_j^{100-P_i}\right)^{\delta}, \tag{10}$$

where θ and δ are the income and price elasticities of public service demand, respectively. Substituting equation (10) into equation (6) and rearranging yields

$$\ln\left(\frac{pyes_j^2}{1-pyes_j^2}\right) - \ln\left(\frac{pyes_j^1}{1-pyes_j^1}\right) = c_3 a\left[y_{j1}^{\theta} p_{j1}^{\delta} - y_{j2}^{\theta} p_{j2}^{\delta}\right] + (\varepsilon_{j1} - \varepsilon_{j2}). \tag{11}$$

Note that equation (11) is nonlinear in the parameters. Consequently, in the empirical work that follows, we use non-linear least squares to estimate the parameters of this model. As above, the income elasticity θ is expected to be positive, and the price elasticity δ is expected to be negative.

Finally, relaxing the restriction that β is constant and allowing the distribution of preferences to differ across communities, equation (11) can be rewritten based on extended specification in equation (7):

$$\ln\left(\frac{pyes_j^2}{1-pyes_j^2}\right) - \ln\left(\frac{pyes_j^1}{1-pyes_j^1}\right) = c_3 \exp[\phi Z_j]\left[y_{j1}^{\theta} p_{j1}^{\delta} - y_{j2}^{\theta} p_{j2}^{\delta}\right] + (\varepsilon_{j1} - \varepsilon_{j2}), \tag{12}$$

where Z_j is a vector containing variables that capture heterogeneity in preferences within school district *j*.

The first difference specifications utilized in equations (9), (11), and (12) eliminate unobserved differences across districts in preferences for public services, political leaning, time invariant differences in turnout rates, as well as a host of other idiosyncratic differences that affect voting and additively enter the estimation equation. Consequently, they eliminate time invariant factors that might be correlated with the income distribution and thus bias a cross-sectional aggregate test of the median or decisive voter model. Nevertheless, our first difference specifications raise several concerns. The first concern is that

changes in the decisive voter's income between referenda may be correlated with unobservables that affect the change in vote share between referenda. In order to control for these factors, additional models of change in vote share are estimated including linear controls for district size, voter turnout, and other attributes intended to capture changes in the composition of voters between the two referenda.[13] For example, adding these terms to equation (11) yields

$$\ln\left(\frac{pyes_j^2}{1-pyes_j^2}\right)-\ln\left(\frac{pyes_j^1}{1-pyes_j^1}\right)=c_3 a\left[y_{j1}^{\theta}p_{j1}^{\delta}-y_{j2}^{\theta}p_{j2}^{\delta}\right]+\beta(X_{j2}-X_{j1})+(\varepsilon_{j2}-\varepsilon_{j1}) \qquad (13)$$

where X_{jk} contains time varying controls for jurisdiction *j* and referenda *k*.

The second concern is that the change in the decisive voter's income may simply be capturing a general correlation between the nature of the income distribution and changes in voting patterns between the two referenda. Specifically, as noted in the introduction, the difference between the 50th and 45th percentile incomes is highly correlated with the difference in income between the 60th and 40th, 70th and 30th, and the 80th and 20th percentile incomes. In fact, these alternative income differences all have a correlation of 0.95 or higher with the difference between the 50th and 45th percentile incomes. Consequently, our models simply cannot statistically distinguish between these variables. Naturally, this raises concerns that the distribution of income within school districts simply correlates with changes in voting between the referenda, and voting is not systematically related to changes in the income of the decisive voter.

We attempt to address this concern, at least partially, by examining alternative measures of the income distribution; namely various measures of income inequality based on the following generalized entropy index (GE):

$$GE(\gamma)=\frac{1}{\gamma^2-\gamma}\left[\frac{1}{N}\sum_{i=1}^{N}\left(\frac{y_i}{\bar{y}}\right)^{\gamma}-1\right], \qquad (14)$$

[13] In the paper, we focus on additive controls because our controls are primarily intended to capture heterogeneity in voting between the two referenda, such as changes in average political preferences of voters, rather than heterogeneity in public service demand. In the constant elasticity demand model, however, jurisdiction heterogeneity may remain that affects the scale of public service demand, e.g. $S = aY^{\theta}P^{\delta}\exp^{\beta x+\alpha}$ where *x* is a vector of referenda varying attributes and α is a jurisdiction fixed effect, and these unobserved jurisdiction attributes will not be differenced away. It is not feasible to estimate multiplicative jurisdiction fixed effects for the models specified in equations (11) and (12). However, we investigated models where jurisdiction size, turnout and the other controls enter in a multiplicative fashion, and all results are robust.

where γ captures the relative weight placed on different regions of the distribution. As γ increases from low levels, emphasis is shifted from the lower tail of the distribution to the middle of the distribution and eventually as γ continues to increase to the upper tail of the distribution. As a basis of comparison, values of γ between –0.5 and 0.0 closely resemble the Gini coefficient, while a γ of 1.0 yields the Theil index and a γ of 2.0 yields the coefficient of variation. The correlation between the generalized entropy index and the difference between the 50th and 45th percentile incomes ranges from 0.72 when γ is -2.5 to –0.21 when γ is 2.5 and it equals 0.04 when γ equals 0.

We use the generalized entropy index in equation (14) to test whether other measures of heterogeneity in the income distribution can explain any variation in vote share changes after controlling for the income difference between the 50th and 45th percentile. Specifically, the predicted residuals from equations (9), (11) and (12), $(\hat{\varepsilon}_{j1} - \hat{\varepsilon}_{j2})$, are regressed upon various generalized entropy measures created by allowing γ to range between –2.5 and 2.5. If these measures of income inequality within a school district can explain the residual in vote share, the influence of the income change between the 50th and 45th percentile on the change in the vote share might also be attributed a correlation between changes in the vote share and jurisdiction income heterogeneity or omitted variables that are correlated with jurisdiction income heterogeneity. On the other hand, failure of these income distribution variables to explain the residual lends support to the notion that the income of the decisive voter is sufficient for explaining the relationship between public service voting outcomes and jurisdiction income distribution. Further, this analysis can be repeated using alternative measures of the income spread, such as the difference between the 60th and 40th, 70th and 30th, and the 80th and 20th percentile incomes, in order to test whether those variables are sufficient to explain public service voting outcomes.

5. Data

We obtained data on vote outcomes for Propositions 26 and 39 from the Statewide Database, maintained by the Institute of Governmental Studies at the University of California, Berkeley. The database contains aggregate vote outcomes and voter registration information, for all statewide primary and general elections held in California since 1990. The primary unit of analysis in the statewide database is the Census block. We aggregated the block level vote tallies and voter registration information up to the school district level. Thus, our primary unit of observation is the school district.

In the empirical framework developed in Section 4, the difference in vote shares between Propositions 39 and 26 is a function of the difference between the 50th and 45th percentile income and the difference between the 50th and 45th percentile tax price in a school district. To construct estimates of the income percentiles, we used district-level data from the 2000 Census on the distribution of household

income. Specifically, the 2000 Census contains information on household income grouped into 17 income categories. We used this grouped income data and linear interpolation to estimate the 50th and 45th percentile level of income in each district.

We used a similar approach to construct measures of the 50th and 45th percentile tax prices. The 50th percentile tax price in district j is $\left(E_j / G_j\right) \cdot h_j^{50}$, where E_j denotes the total enrollment in district j, G_j denotes the total assessed value of property in district j and h_j^{50} denotes the 50th percentile assessed value of owner-occupied homes in district j. Similarly, the 45th percentile tax price is, $\left(E_j / G_j\right) \cdot h_j^{45}$ where h_j^{45} denotes the 45th percentile assessed value of owner-occupied homes in district j. District-level data on student enrollment in 2000 were obtained from the California Department of Education while data on the total assessed value of property in each school district in 2000 were obtained from the Coalition for Adequate School Housing, a California school advocacy organization. Unfortunately, district-level data on the assessed value of owner-occupied homes in California is unavailable. Consequently, we used data from the 2000 census on the distribution of house values (which are grouped into 25 categories) to estimate the 50th and 45th percentile level of home values and used these home value percentiles as proxies for h_j^{50} and h_j^{45} when constructing our tax price variables.

We also include a number of additional variables in several of our empirical specifications. The first variable is the difference in voter turnout between Propositions 26 and 39. Following Coate, Conlin and Moro (2008) and Coate and Conlin (2004) among others, we define voter turnout as the fraction of eligible voters (i.e., voting age population) in each district that voted on Proposition 39 and Proposition 26 respectively. We include the difference in voter turnout to account for the potential impact changes in voter turnout may have on vote outcomes between the two elections. The second variable is the difference in the fraction of registered Republicans between Propositions 26 and 39 and the third variable is the difference in the fraction of registered Democrats between the two propositions. We include these two variables to control for systematic changes in the ideological composition of voters between elections.

In addition, we develop controls for across district changes in the composition of turnout. While we do not have individual data on voting, we do observe turnout at levels of aggregation below the school district level, i.e. census block group. Accordingly, we calculate the within school district correlations between changes in turnout and two block group variables: mean income and the fraction of households with school aged children. These variables are included as controls in order to capture systematic shifts in the composition of voters at the school district level. The remaining variables are a set of district size fixed effects. Specifically, we sorted districts into four equally sized groups based on their total

population. We then created four indicator variables that take the value of unity if a district is in the first, second, third or fourth quartile of district size respectively. In our empirical work, we use districts in the first quartile as a reference group and include the second, third and fourth quartile indicator variables as controls. We include these district size fixed effects for two reasons. First, larger districts tend to be more heterogeneous and may have responded differently than smaller, more homogenous, districts to events that occurred between the votes on Propositions 26 and 39. Second, the difference between 50^{th} and 45^{th} percentile income is likely correlated with both district size and the general level of income heterogeneity in the district, implying that failure to control for district size could bias our key parameter estimates.

Finally, to implement the specification given by equation (12), which allows the distribution of preferences to differ across districts, we also develop two variables designed to measure preference heterogeneity within districts. As discussed in section 3, district preference heterogeneity, as captured by β, is likely related to the distribution of income. Consequently, we include a generalized entropy measure with γ equal to zero to control for income heterogeneity.[14] Preference heterogeneity is also likely to be related to the distribution of political ideology within a district. For example, Democrats may have stronger preferences for local public spending than Republicans. Thus, we also include a Herfindahl index to control for heterogeneity in political ideology. Specifically, the index we employ is: $H_j = 1 - (pdem_j^2 + prep_j^2)$, where $pdem_j$ is the fraction of registered democrats in district j and $prep_j$ is the fraction of registered republicans in district j. Greater values of this index are associated with greater heterogeneity in political ideology.

Our data have a number of limitations. The first limitation concerns school districts with overlapping boundaries. Specifically, California contains three types of school districts: unified districts, elementary districts and high school districts. The boundaries of the latter two types of districts overlap: one high school district typically contains two or more elementary districts. Thus, in non-unified districts there are really two decisive voters: the decisive voter for the elementary school district and the decisive voter for the high school district into which the elementary district feeds. That fact has ramifications for our empirical work since in non-unified districts it becomes unclear how one should measure the income and tax price of the proposed decisive voter. Consequently, in the empirical analysis that follows, we restrict our sample to unified school districts. The second limitation concerns missing data. Data on the

[14] The generalized entropy measures are too highly correlated across different values of γ for us to precisely estimate the separate influence of multiple measures of income inequality. However, models were estimated that simultaneously included the generalized entropy measures with γ equal to –2, 0, and 2, and our income elasticity estimates were robust.

fraction of voters supporting Proposition 26 is unavailable for 15 of the 323 unified school districts operating in California in 1999-2000.[15] We exclude these 15 districts from our analysis leaving a final sample of 308 unified school districts.

Table 1 provides means and standard deviations for the variables used in the analysis. These summary statistics are reported separately for Propositions 26 and 39 respectively. As expected the increase in the vote requirement from 50 to 55 percent is associated with a greater percentage of voters supporting the referendum, lower decisive voter income, and lower decisive voter tax share. The change from a March election (Prop 26) to a November election (Prop 39) also increases turnout from 28 to 43 percent of eligible voters.

{Insert Table 1 Here}

6. Results using Linear Demands

Regression results for the change in vote share using the linear demand specification in equation (9) are presented in Table 2. The first column presents the basic model that controls only for the change in the decisive voter's income and tax price. The second and third columns contain results from models that include controls for turnout and party affiliation among registered voters and those controls plus jurisdiction size fixed effects, while the fourth column contains results based on a model that includes the controls used in column three plus additional turnout controls based on block group-level correlations between turnout and either income or share families with children. As expected, all four regressions imply a strong positive relationship between the change in the decisive voter's income and the change in the fraction of 'yes' votes between Propositions 39 and 26.[16] The estimated coefficients on the change in the decisive voter's income range from 0.137 to 0.217 and are all statistically significant at the five percent level. These point estimates suggest that a large fraction of the change in vote shares between the two propositions can be explained by the change in the decisive voter's income. Specifically, based on equation (9) and applying the estimated income coefficient to the sample of unified school districts, our model predicts that the implied change in the decisive voter's income is consistent with a 1.9 percentage point increase in the percent voting yes in model 1, and a 2.6, 1.6, and 1.9 percentage point increase in the

[15] The counties of Monterey, Humboldt and San Luis Obispo did not report vote tallies to the Statewide Database for Proposition 26.

[16] We also divided the sample into three subsamples based on the size of the increase in turnout between the two referenda, and we could not reject the hypothesis that the effect of income was the same across these three subsamples. However, these tests are fairly weak because the subsamples are quite small.

percent voting yes in models 2, 3, and 4 respectively.[17] Given that the actual increase in percent voting yes was 4.3 percentage points, our model predicts that between 37% and 60% of the change in vote shares between the two propositions can be explained by the change in the decisive voter's income.[18]

In contrast to our findings concerning the decisive voter's income, we find no evidence that voters respond to changes in tax price between the 50th and 45th percentile. This may not be too surprising for several reasons. First, as noted previously, district-level data on the assessed value of owner occupied homes in California is unavailable. Consequently, our tax price variable uses self reported house values from the 2000 census as proxies for the 50th and 45th percentile assessed values. In California, however, home values are likely to vary dramatically from assessed values due to Proposition 13, which prohibits the reassessment of homes for property tax purposes except when the house is sold. Thus, two homes with the same market value may have substantially different assessed values depending on when they were last sold. Given this fact, our tax price variable most likely suffers from substantial measurement error. Second, even if we did have data on the assessed value of owner occupied homes, the reassessment limitations associated with Proposition 13 call into question whether voters could accurately identify the tax price associated with the decisive voter. Specifically, to identify the decisive voter's tax price, voters would need to know the actually distribution of assessed values in their district (which is unlikely) or have knowledge of the pattern of home sales in their district and be able to use that information to make an educated guess about assessed values. This aspect of the California property tax system complicates the already difficult problem of determining tax price, which requires information on commercial property values and the efficiency of local public service provision. Thus, voters are likely to know much less about the distribution of tax prices in their districts than they are about the distribution of income. As a result, it is not be too surprising that voters appear to respond more to changes in the decisive voter's income than to changes in the decisive voter's tax price.

{Insert Table 2 Here}

The results reported in Table 2 may be biased downwards if our decisive voter income variable suffers from measurement error. Concerns about measurement error seem warranted given that the 50th and 45th income percentiles for each jurisdiction must be imputed based on census tabulations that divide the population into 17 income categories. While these imputations may accurately represent across district differences in income, the difference between the imputed 50th and 45th percentile incomes may be

[17] The predicted change is estimated by calculating the change in log-odds for each school district and translating this change into a predicted change in share voting yes based on the actual share voting yes for proposition 26 in the school district.

[18] The actual increase in the percent voting yes of 4.3 percentage points is based on the sample of unified school districts and thus differs from the statewide increase in the percent voting yes which was 4.5 percentage points.

far less accurate in capturing across district differences in the *change* in the decisive voter's income. Measurement error should attenuate the coefficient estimate on the decisive voter income variable and thus the estimates reported in Table 2 should provide a lower bound on the relationship between the change in vote shares and the change in the decisive voter's income.

The standard approach for addressing measurement error is instrumental variables. The generalized entropy measures of income dispersion described in Section 4 provide reasonable candidates for instruments given the ability of these variables to explain changes in the decisive voter's income between the two referenda and the fact that these variables which describe the income distribution more broadly likely suffer from less measurement error arising from the census tabulations. Further, the standard overidentification test for instrumental variables estimation also implicitly tests our proposition, which will be examined more carefully in section 8, that the change in decisive voter income is sufficient to explain the entire empirical relationship between jurisdiction vote share and the jurisdiction's income distribution. Overidentification tests explicitly ask the following question: Do the instruments enter into the second stage equation in a way that is not captured by the linear combination of those variables that was estimated in the first stage? If our instruments enter into the second stage in a form that differs from how they enter the first stage, one or more of those variables could not have entered the second stage solely through the first stage variable, a violation of the exclusion restriction assumption. In our case, a rejection of the exclusion restrictions suggests that other measures of income heterogeneity belong in the vote share model, while a failure to reject the exclusion restrictions supports our initial hypothesis that information on decisive voter income is sufficient to explain changes in vote share.

Table 3 presents the results of these specification tests where each model includes three generalized entropy measures and the values of γ are chosen to reduce the correlation between the three measures being used in the same specification.[19] Specifically, the three panels in Table 3 present 2SLS parameter estimates that were obtained by using as instruments entropy measures based on the following values of γ: (-2.5, -0.5, 1.5), (-2.0, 0, 2.0), and (-1.5, 0.5, 2.5). Instrumenting for measurement error causes the parameter estimates on the decisive voter income variable to increase from between 0.137 and 0.217 to between 0.172 and 0.276. Thus, the 2SLS results are consistent with measurement error in the explanatory variable.[20] Furthermore, note that in every case the instruments pass the overidentification test based on Hansen's J Statistic.[21]

[19] The estimates of all first stage models are presented in an appendix that is available upon request along with F-tests that establish that the instruments have substantial explanatory power in the first stage, suggesting that our instrumental variable estimates are not biased by weak instruments.

[20] The reader should note that these instruments contain substantial variation for both explaining changes in the decisive voter's income and providing powerful overidentification tests. For example, the correlations between the change in the decisive voter's income and the entropy measure for a γ of -2.5, -0.5, and 1.5 are 0.72, 0.20, and –0.25,

{Insert Table 3 Here}

Finally, as we noted previously, measurement error may also be present in the tax price variable and the resulting attenuation bias may be substantially larger due to the reassessment limitations of Proposition 13. The change in tax price arising in Equation (9) is the difference between the 50th and 45th percentile house value divided by the property tax based per service recipient, or

$$\left(p_{j1} - p_{j2}\right) \equiv \left(E_j / G_j\right)\left(h_j^{50} - h_j^{45}\right).$$

The ratio of number of service recipients or students (E_j) to the jurisdiction grand list of assessments (G_j) provides an instrument that will be correlated with changes in decisive percentile tax price and yet uncorrelated with measurement error that arises because the census distribution of house values does not accurately represent the actual distribution of assessed values. Instrumenting for tax price using the ratio of the number of service recipients to the jurisdiction grand list has no meaningful impact on any previously estimated coefficients. The effect of decisive voter income is robust in terms of both significance and magnitude, and the estimated effect of tax price remains small and statistically insignificant.[22] These estimates suggest that our failure to find a relationship between tax price and referenda voting is more likely due to voter inability to observe changes in tax price rather than measurement error in our tax price variable.

7. Constant Elasticity of Demand Models

Nonlinear least squares results for the change in vote share using the constant elasticity of demand specification with homogeneous preferences in equation (11) are presented in Table 4. Similar to Table 2, the first column presents the basic model that controls only for the change in the demand of the decisive voter. The second, third, and fourth columns contain additively separable terms intended to control for differences in voting patterns between the two referenda including basic controls for turnout,

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respectively. Similarly, the correlations between the entropy measures themselves are 0.63 for γ set to –2.5 and –0.5, 0.10 for γ set to –2.5 and 1.5, and 0.67 for γ set to –0.5 and 1.5.

[21] Under the null hypothesis that all the instruments are exogenous, the J-Statistic is distributed Chi-Square and none of the chi-square values reported in Table 3 are statistically significant at the 10 percent level or better.

[22] All income and tax price instruments are included in the first stage models for both decisive voter income and tax price. As before, overidentification tests fail to reject the exclusion restrictions. Finally, F-tests confirm that the instruments have power to explain the decisive voter income and tax price variables both as a complete set of instruments, as well as when focusing on the instruments created for one decisive voter variable conditioning on the instruments created for the second variable. These estimates and the IV specification tests are available upon request.

jurisdiction size fixed effects, and additional turnout controls based on block group-level correlations. In Table 4, the estimated income elasticities lie between 0.715 and 0.855, and are comparable to income elasticity estimates based on actual education capital spending in California of between 0.70 and 0.77 (Balsdon, Brunner and Rueben, 2003). As in the linear demand model, price elasticity estimates are small, and in most cases statistically insignificant. Price elasticity estimates for California capital spending based on similar tax price variables range from -0.56 to -0.59 (Balsdon, Brunner and Rueben, 2003) again suggesting that results are driven by voter inability to observe tax price rather than measurement error.

{Insert Table 4 Here}

Nonlinear least squares results based on the constant elasticity of demand specification in equation (12), which allows the distribution of preferences to differ across districts, are presented in Table 5. The estimated income elasticities lie between 0.742 and 0.825 and are quite close to the income elasticity estimates presented in Table 4. As in the linear demand model, price elasticity estimates are small, and statistically insignificant once controls are added for jurisdiction size. The estimated coefficient on the generalized entropy measure we use to control for differences in preference heterogeneity is negative and statistically significant in all four specifications. Similarly, the estimated coefficient on our political heterogeneity index is also negative, although it is only significant in the first model. The negative sign on our two preference heterogeneity controls is consistent with greater heterogeneity in income and/or political ideology leading to larger heterogeneity in preferences or greater variance in the unobservables that shape voting behavior as captured by β in equation 7. Specifically, our results suggest that the influence of the decisive voter's demand on the determination of voting rules is smaller in more heterogeneous districts. That finding is consistent with the theoretical implications of Romer, Rosenthal and Munley (1992) and empirical results of Romer, Rosenthal and Munley (1992) and Gerber and Lewis (2004).

To put our results into context, we can use the results reported in Table 5 to examine how a one standard deviation increase in the preference heterogeneity term in equation (12), $\exp(\hat{\phi} Z)$, scales the influence of the decisive voter's demand on the change in the vote logit. Evaluated at the mean of $\exp(\hat{\phi} Z)$, a one standard deviation increase in the standard deviation of preferences leads to a 19.1% decrease in the contribution of the median voter's preferences to the log odds of voting yes in model 1, and a 13.7%, 13.0%, and 11.7% decrease in models 2, 3, and 4, respectively. In terms of the second referenda, a one standard deviation increase in preference heterogeneity in all unified school districts

would have decreased predicted support for the referenda by 5.6 percentage points in model 1, and by 5.1, 3.5, and 3.2 percentage points in models 2, 3, and 4, respectively. Given that the actual percent yes in the second referenda was 54.4%, these predicted declines in support suggest that the referenda would have been defeated in models and 1 and 2 and passed by a margin of only 1 percentage point in models 3 and 4.

{Insert Table 5 Here}

8. Is the Decisive Voter's Income Sufficient to Explain Voting Patterns?

As discussed earlier, one concern with the results reported in Tables 2, 4 and 5 is that the change in the decisive voter's income may simply be capturing a general correlation between the nature of the income distribution and changes in voting patterns between the two referenda. Table 6 presents the results of regressing the residuals from models two and four of Tables 2, 4 and 5 on various generalized entropy measures. Note that none of the models identify a statistically significant relationship between the residual and the generalized entropy measures of income dispersion. Thus, we do not find any relationship between changes in voting patterns and income inequality after controlling for information captured by the change in the decisive voter's income. In that sense, the change in the decisive voter's income appears to be sufficient for explaining the relationship between public service voting outcomes and the distribution of income within districts. While the F-tests are very close to zero for the model involving differences in preference heterogeneity, it is worth noting that this model already controls for the same or similar generalized entropy measures in the multiplicative term on voter preferences for public services, and so it is not entirely surprising that the entropy measures cannot explain the residual. Therefore, our conclusion that the change in decisive voter income is sufficient to explain voting changes is based primarily on the results for the linear models and the constant elasticity of demand models with homogeneous distribution of preferences.

{Insert Table 6 Here}

Despite the results reported in Table 6, one might still wonder whether other measures of the income spread, such as the difference between the 80^{th} and 20^{th} percentile incomes, might also be sufficient for explaining the relationship between changes in vote outcomes and the distribution of income within districts. To address that possibility, we replaced the difference between the 50^{th} and 45^{th} percentile incomes in equations (9), (11), and (12) with the difference between other percentiles of the income distribution (e.g. 70^{th} and 30^{th}) and then repeated our residual analysis. The results are reported in

Table 7 for models two and four. The first panel shows the results controlling for the change in the decisive voter's income, while the second, third and fourth panels show results that control for the difference between the 60th and 40th, 70th and 30th and 80th and 20th percentile incomes respectively. In the interest of brevity, Table 7 only shows results based on one generalized entropy index, namely the index for γ set to 0.0.[23]

The results reported in Table 7 provide mixed evidence on whether other measures of the income spread are sufficient for explaining changes in vote outcomes between the two referenda. For both the linear models and constant elasticity models with homogeneous distribution of preferences, the F-statistics increase as the income percentiles move away from the percentiles associated with the decisive voter. For model 2, the estimated coefficient on the inequality index is statistically significant in several specifications, suggesting that other measures of the income spread do not sufficiently explain changes in voting. On the other hand, in model 4, we cannot rule out that other measures of the income spread sufficiently explain changes in voting between the two referenda.[24] The results for models 1 and 3 are very similar to the results for models 2 and 4, respectively. As in Table 6, the F-statistics are very small for the model with heterogeneous distribution of preferences because the model already includes non-linear controls for the generalized entropy variables. Regardless, the results reported in Table 7 provide suggestive evidence that the income of the decisive voter is sufficient to capture most information about the income distribution that might explain changes in voting while other income percentiles are somewhat less effective in capturing this information.

{Insert Table 7 Here}

9. Counterfactuals

The results reported in sections 6, 7, and 8 consistently support the hypothesis that the income difference between the 50th and 55th percentile voter adequately and sufficiently explains changes in vote outcomes between Propositions 26 and 39. Thus, our results provide support for the hypothesis that the

[23] Replacing the GE index for γ set to 0.0 with a GE index for γ set to either -0.5 or 0.5 produces results similar to those reported in Table 6. Using an index based on any value smaller than -0.5 or larger than 0.5 produces coefficient estimates that are always statistically insignificant. It is interesting to note that the only inequality indices that have explanatory power in our residual analysis are those based on values of γ near zero. As noted previously, values of γ near zero yield inequality indices that are sensitive to income inequality around the middle of the income distribution. Thus, our results suggest that once we replace the change in the decisive voter's income with other measures of the income spread, those alternative measures are insufficient to capture information about income inequality in the middle of the income distribution that might explain changes in voting.

[24] These results arise primarily due to the strong explanatory power of our jurisdiction size controls. For example, if we remove the jurisdiction size controls in model 4, the estimated coefficients on the inequality index become statistically significant in panels three and four.

median voter is also the voter with the median income. However, before we accept that conclusion, it would be helpful to know whether we have truly identified a relationship between changes in the decisive voter's income and changes in vote shares or whether we are simply picking up a spurious relationship caused by some other unobserved factor that might persist at a variety of levels of spatial aggregation. To address this concern we conduct two counterfactuals. The logic behind our counterfactuals is simple: if the relationship we have identified is truly causal, then it should hold for school districts (which would have been directly affected by the outcomes of Propositions 26 and 39) but it should not hold for other political or geographic entities. For example, while we expect the income difference between the 50th and 55th percentile voter in a *school district* to explain differences in vote shares within school districts we would not expect the income difference between the 50th and 55th percentile voter in a *census tract* or a *state assembly district* to explain differences in vote shares within those geographic/political entities. That is, for political/geographic entities other than school districts, the income difference between the 50th and 55th percentile voter should be uncorrelated with changes in vote shares.

Our rationale for choosing census tracts and State Assembly Districts (SAD) is based on their size and their lack of relevance for the provision of any local public services. Census tracts tend to be much smaller than school districts while state assembly districts are much larger than census tracts and often contain many school districts. While some school districts such as Los Angeles Unified contain many SAD's, California contains a total of 80 SAD's relative to approximately 1,000 school districts. Thus, our counterfactuals cover geographic/political entities that are both smaller and larger than school districts on average. Further, since neither of these geographic regions represents a level of local government, the income and tax price variables could not be related to any unexpected fiscal implications of Propositions 26 and 39.

To implement our counterfactuals we estimate models identical to those reported in the third column of Tables 2, 4 and 5 using data on vote shares, income percentiles (50th and 45th), tax price percentiles, etc, that are calculated for either census tracts or state assembly districts.[25] For example, our counterfactual involving census tracts utilizes information for 6,840 census tracts on vote shares in a census tract, income differences between the 50th and 55th percentile voter in a census tract, etc. Similarly, our counterfactual involving state assembly districts utilizes information for the 80 SAD's in California on vote shares within SAD's, income percentiles within SAD's, etc.

In addition, we also present estimates for census tract models that include controls for district specific effects. Controls for district effect are included to insulate the estimates against the systematic

[25] Column four of Tables 2, 4 and 5 includes controls for the within school district correlations between changes in voter turnout and three block group level variables. Since these controls can not be constructed for the counterfactual involving census tracts (not enough block groups per census tract) and also are statistically insignificant in all school district models, we only report counterfactuals for the third model in Tables 2, 4 and 5.

across district variation that drives the estimates in the school district sample. For the linear model, the specification in equation (9) is simply extended to include a set of district fixed effects. The constant elasticity models in equations (11) and (12) are estimated by non-linear least squares and school district fixed effects are not tractable. Rather, we follow a control function approach using the school district estimates to predict school district demand

$$E[S_2 - S_1 \mid y_{j1}, y_{j2}, p_{j1}, p_{j2}] = \hat{c}_3 \left[y_{j1}^{\hat{\theta}} p_{j1}^{\hat{\delta}} - y_{j2}^{\hat{\theta}} p_{j2}^{\hat{\delta}} \right], \tag{15}$$

and include the predicted district demand as a regressor in the census tract model. Obviously, relative to a model with district fixed effects this model has less flexibility to capture the across school district variation that drives our findings in Tables 5 and 6.[26] Naturally, the school district effects could also contaminate our SAD estimates (in fact some mid-sized school districts essentially are SAD's), but a natural analog to the census tract district specific effects model does not exist because some SAD's contain many school districts while others are entirely contained within school districts.

Results for the counterfactuals are reported in Table 8. In the interest of brevity, we report only the estimated coefficients on the income difference and tax price difference variables. The first panel of Table 8 replicates the school district results reported in column 3 of Tables 2, 4 and 5. The second, third and fourth panels present our counterfactuals based on state assembly districts, census tracts and a census tract model with district effects respectively. The results reported in Table 8 are quite striking. In all our counterfactuals the estimated coefficients on the difference between the 50th and 45th percentile income are significantly smaller than the estimates for school districts. For example, in the linear model the estimated coefficient on the income difference variable is 0.058 in the assembly district sample and 0.020 in the census tract sample with district fixed effects. The corresponding estimate for school districts is 0.156, which is approximately 2.7 times larger than the assembly district estimate and approximately 8 times larger than the census tract estimate. Further, none of the estimates for SAD's are statistically significant, and only one estimate for the census tract models that control for district effects (the model with no covariates) is statistically significant.

{Insert Table 8 Here}

[26] Standard errors for this model are also clustered at the school district level because heteroscedasticity can bias the estimation of standard errors in fixed effect models.

9. Conclusion

This paper provides a direct test of the political economy "as if" proposition that underlies nearly all empirical studies that utilize the median voter model. Specifically, we employ a unique dataset to examine whether the voter with the median income is decisive in local spending referenda and whether the influence of the median income resident's preferences on referenda vote outcomes declines with jurisdiction heterogeneity. Previous tests of the median voter model have typically relied on aggregate cross sectional data to examine whether the voter with the median income is pivotal. These studies are likely biased because communities differ across a variety of unobservable dimensions that are likely correlated with the distribution of income in a community. In contrast to previous studies, we make use of a unique natural experiment that allows us to estimate a first difference specification that controls for jurisdiction unobservables and avoids the fundamental problem of measuring the actual services demanded by voters. Consequently, we are able to avoid many of the problems that have hindered prior studies that have tested the median voter hypothesis.

Our empirical results suggest that jurisdiction median income accurately captures the expected outcomes of majority votes on public service spending and that voters understand the impact of changes in the identity of the decisive voter. These findings suggest that voters rationally and accurately consider the specific impact of voting rules on local spending in their own jurisdiction when voting on referenda that determine voting rules. Specifically, our coefficient estimates on the income of the decisive voter are consistent with between a 1.6 and 2.6 percentage point increase in the percent voting yes due to the change in the vote requirement from 50 to 55 percent, while the actual increase in percent voting yes was 4.3 percentage points. Instrumental variable estimation that corrects for measurement error increases the effect of the change in the decisive voter's income to between a 2.1 and 3.4 percentage point increase in the share voting yes. Further, constant elasticity of demand models provide estimated income elasticities of between 0.72 and 0.86, which are very stable across specifications and consistent with the existing literature. The estimated effect of median income on voting is not present in counterfactuals estimated at the census tract and state assembly district level. No evidence was found to suggest that voters can observe and then consider changes in the tax price of the decisive voter when voting in referenda.

Finally, in our model where the distribution of preferences is allowed to vary across school districts, we consistently find that school district income heterogeneity is associated with reduced influence of the decisive voter's preferences on support for the referenda, a result consistent with Gerber and Lewis (2004). In terms of magnitude, we find that a one standard deviation increase in heterogeneity among all school districts would have reduced support for the second referenda (which passed by 4.4 percentage points) by between 5.6 and 3.2 percentage points. Earlier work by Romer, Rosenthal and Munley (1992) and Rothstein (1994) conclude that the decisive voter's preferences should have less

influence on support for referenda in more heterogeneous jurisdictions, and our findings provide strong support for the implications of their theoretical models. Recent empirical work by Alesina, Baqir and Easterly (1999) and Alesina, Baqir and Hoxby (2004) find that heterogeneous communities spend less on productive public goods and that jurisdiction consolidation is reduced when the surrounding region is heterogeneous, respectively. Our model along with the earlier work of Romer, Rosenthal and Munley (1992) and Rothstein (1994) identifies another important mechanism by which heterogeneity influences public choice concerning the provision of local public goods. Referenda models of this sort clearly imply that heterogeneity in preferences within a jurisdiction will reduce electoral support for referendums that authorize spending on public services as well as referendums intended to liberalize the rules under which spending is authorized.

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Table 1
Summary Statistics

	Proposition 26		Proposition 39	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Fraction Yes	0.477	0.085	0.505	0.084
Income	47,297	17,299	42,724	15,815
Tax Price	0.530	0.292	0.503	0.278
Turnout	0.279	0.098	0.431	0.123
Fraction Republican	0.373	0.114	0.374	0.114
Fraction Democrat	0.441	0.108	0.436	0.106

Notes: Table contains means and standard deviations in the sample of unified school districts in California for the two referenda where the income and tax price variables represent the 50^{th} and 45^{th} percentile values for Proposition 26 and 39, respectively.

Table 2
Coefficient Estimates: Linear Demand Model

	(1)	(2)	(3)	(4)
Income ($10,000's)	0.156**	0.217**	0.137**	0.156**
	(0.053)	(0.064)	(0.059)	(0.063)
Tax Price	0.287	0.125	0.142	0.104
	(0.410)	(0.391)	(0.339)	(0.368)
Turnout		0.285*	0.253	0.274*
		(0.166)	(0.162)	(0.162)
Fraction Democrat		-0.758*	-0.371	-0.349
		(0.452)	(0.370)	(0.371)
Fraction Republican		0.101	0.014	-0.015
		(0.492)	(0.454)	(0.459)
Second Quantile of Size			0.007	0.006
			(0.024)	(0.025)
Third Quantile of Size			0.074**	0.072**
			(0.023)	(0.025)
Fourth Quantile of Size			0.094**	0.089**
			(0.022)	(0.025)
Turnout/Income Correlation				0.017
				(0.024)
Turnout/Families with Children Correlation				0.034
				(0.025)
Constant	0.036	0.041	0.035	0.033
	(0.029)	(0.035)	(0.033)	(0.033)
R-Square	0.04	0.07	0.14	0.15
Observations	308	308	308	308

Notes: Columns 1-4 contain OLS parameter estimates for the change in log odds of share voting yes between the two referenda. The rows denoted by income, tax price, turnout, fraction democrat, and fraction republican contain estimates on the change in those variables between the two referenda while the next six rows contain estimates on the school district variable levels. Robust standard errors are shown in parentheses, and statistical significance at the 10% and 5% level are denoted by * and **, respectively.

Table 3
Instrumental Variables Estimation

	(1)		(2)		(3)		(4)	
	O.L.S.	2SLS	O.L.S.	2SLS	O.L.S.	2SLS	O.L.S.	2SLS
Income	0.156**	0.172**	0.217**	0.234**	0.137**	0.172**	0.156**	0.202**
	(0.053)	(0.056)	(0.064)	(0.075)	(0.059)	(0.070)	(0.063)	(0.078)
Tax Price	0.287	0.260	0.125	0.106	0.142	0.107	0.104	0.054
	(0.410)	(0.399)	(0.391)	(0.382)	(0.339)	(0.330)	(0.368)	(0.358)
Hansen J Statistic		1.56		2.00		2.17		1.98
Income	0.156**	0.194**	0.217**	0.261**	0.137**	0.185**	0.156**	0.216**
	(0.053)	(0.058)	(0.064)	(0.078)	(0.059)	(0.073)	(0.063)	(0.082)
Tax Price	0.287	0.224	0.125	0.076	0.142	0.093	0.104	0.040
	(0.410)	(0.394)	(0.391)	(0.382)	(0.339)	(0.332)	(0.368)	(0.361)
Hansen J Statistic		2.34		2.58		2.76		2.80
Income	0.156**	0.207**	0.217**	0.276**	0.137**	0.177**	0.156**	0.212**
	(0.053)	(0.068)	(0.064)	(0.090)	(0.059)	(0.087)	(0.063)	(0.100)
Tax Price	0.287	0.201	0.125	0.060	0.142	0.101	0.104	0.044
	(0.410)	(0.393)	(0.391)	(0.384)	(0.339)	(0.333)	(0.368)	(0.365)
Hansen J Statistic		1.90		2.01		2.20		2.68

Notes: Table presents OLS and two stage least squares estimates of the four log odd vote share models presented in Table 2 using generalized entropy measures as instruments. Specifically, panel 1 instruments using GE(-2.5), GE(-0.5), GE(1.5), panel 2 instruments using GE(-2.0), GE(0), GE(2.0), and panel 3 instruments using GE(-1.5), GE(0.5), GE(2.5). A Hansen J statistic for valid overidentification is provided for each 2SLS model estimated.

Table 4
Coefficient Estimates: Constant Elasticity Demand Model

	(1)	(2)	(3)	(4)
Income	0.715**	0.780**	0.809**	0.855**
	(0.208)	(0.181)	(0.236)	(0.229)
Tax Price	0.293**	0.200	0.188	0.165
	(0.147)	(0.125)	(0.169)	(0.183)
Turnout		0.250	0.210	0.231
		(0.157)	(0.153)	(0.154)
Fraction Democrat		-0.703	-0.350	-0.307
		(0.464)	(0.381)	(0.387)
Fraction Republican		-0.024	-0.134	-0.151
		(0.470)	(0.438)	(0.445)
Second Quantile of Size			0.008	0.010
			(0.024)	(0.024)
Third Quantile of Size			0.073**	0.073**
			(0.023)	(0.024)
Fourth Quantile of Size			0.092**	0.091**
			(0.023)	(0.025)
Turnout/Income Correlation				0.017
				(0.024)
Turnout/Families with Children Correlation				0.032
				(0.025)
Constant	0.001	0.0004	0.0002	0.0001
	(0.002)	(0.001)	(0.0006)	(0.0003)

Notes: Table presents the estimates from the constant elasticity of demand model shown in equation (11). The estimates presented in the first two rows under income and tax price represent elasticities while the other estimates are coefficients on the variables in a standard linear specification. Robust standard errors are reported in parentheses.

Table 5
Coefficient Estimates: Constant Elasticity of Demand with Preference Heterogeneity

	(1)	(2)	(3)	(4)
Income	0.742**	0.786**	0.788**	0.825**
	(0.205)	(0.170)	(0.219)	(0.213)
Tax Price	0.324**	0.207*	0.209	0.192
	(0.146)	(0.119)	(0.151)	(0.163)
Turnout		0.281*	0.233	0.252
		(0.154)	(0.152)	(0.154)
Fraction Democrat		-0.688	-0.366	-0.325
		(0.426)	(0.367)	(0.376)
Fraction Republican		0.090	-0.039	-0.053
		(0.463)	(0.440)	(0.447)
Second Quantile of Size			0.0006	0.003
			(0.024)	(0.024)
Third Quantile of Size			0.064**	0.066**
			(0.023)	(0.024)
Fourth Quantile of Size			0.083**	0.084**
			(0.023)	(0.026)
Turnout/Income Correlation				0.018
				(0.024)
Turnout/Families with Children Correlation				0.028
				(0.026)
Constant	0.0006	0.002	0.001	0.0006
	(0.016)	(0.004)	(0.003)	(0.002)
Preference Heterogeneity Parameters				
GE(0)	-2.089**	-1.642**	-1.743*	-1.605*
	(0.933)	(0.739)	(0.956)	(0.933)
Party Affiliation Index	-2.663**	-1.744	-1.320	-1.063
	(1.245)	(1.101)	(1.525)	(1.511)

Notes: Table presents the estimates from the constant elasticity of demand model with preference heterogeneity shown in equation (12). The estimates presented in the first two rows under income and tax price represent elasticities while the other estimates are coefficients on the variables in a standard linear specification. Robust standard errors are reported in parentheses.

Table 6
Coefficient Estimates from Regressions of Residual on Alternative Income Inequality Measures

	(1)	(2)	(3)	(4)	(5)	(6)
	Linear		Constant Elasticity		Constant Elasticity with Preference Heterogeneity	
	Model (2)	Model (4)	Model (2)	Model (4)	Model (2)	Model (4)
GE(-2.5)	-0.001	0.000	-0.002	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
F-statistic	0.19	0.01	1.16	0.49	0.10	0.13
GE(-1.5)	-0.011	-0.008	-0.021	-0.017	0.014	0.008
	(0.019)	(0.018)	(0.188)	(0.018)	(0.019)	(0.018)
F-statistic	0.35	0.18	1.23	0.85	0.59	0.22
GE(-0.5)	-0.103	-0.075	-0.118	-0.083	0.054	0.037
	(0.079)	(0.076)	(0.080)	(0.077)	(0.079)	(0.077)
F-statistic	1.72	0.96	2.19	1.16	0.46	0.23
GE(0.5)	-0.171	-0.114	-0.162	-0.095	0.061	0.059
	(0.132)	(0.130)	(0.134)	(0.132)	(0.131)	(0.129)
F-statistic	1.69	0.77	1.45	0.52	0.22	0.21
GE(1.5)	-0.068	-0.033	-0.054	-0.014	0.033	0.044
	(0.079)	(0.078)	(0.081)	(0.078)	(0.077)	(0.076)
F-statistic	0.75	0.18	0.28	0.03	0.18	0.34
GE(2.5)	-0.006	-0.000	-0.004	0.002	0.003	0.007
	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)
F-statistic	0.6	0.01	0.27	0.08	0.11	0.63

Notes: Each row contains the parameter estimate, robust standard errors in parentheses, and F-statistic associated with the regression of the residual from the differenced log odds vote share regressed upon a Generalized Entropy measure where the value of γ for Generalized Entropy measure is shown in parentheses. The columns correspond to the log odd vote share models presented in Tables 2, 4 and 5.

Table 7
Residual Analysis Based on Alternative Income Spread Measures

	Linear		Constant Elasticity		Constant Elasticity with Preference Heterogeneity	
	Model (2)	Model (4)	Model (2)	Model (4)	Model (2)	Model (4)
	50^{th} and 45^{th} Percentile Incomes					
GE(0)	-0.161	-0.112	-0.164	-0.107	0.061	0.051
	(0.113)	(0.111)	(0.115)	(0.113)	(0.114)	(0.112)
F-statistic	2.03	1.03	2.04	0.90	0.29	0.21
	60^{th} and 40^{th} Percentile Incomes					
GE(0)	-0.183	-0.130	-0.177	-0.124	0.071	0.054
	(0.113)	(0.111)	(0.117)	(0.113)	(0.115)	(0.112)
F-statistic	2.62	1.36	2.28	1.19	0.38	0.23
	70^{th} and 30^{th} Percentile Incomes					
GE(0)	-0.195*	-0.141	-0.194*	-0.145	0.071	0.051
	(0.113)	(0.111)	(0.116)	(0.133)	(0.115)	(0.113)
F-statistic	2.97*	1.61	2.76*	1.63	0.38	0.21
	80^{th} and 20^{th} Percentile Incomes					
GE(0)	-0.219**	-0.158	-0.212*	-0.162	0.058	0.039
	(0.111)	(0.110)	(0.115)	(0.112)	(0.113)	(0.111)
F-statistic	3.85**	2.09	3.37*	2.09	0.26	0.13

Notes: The first row presents the results from regressing the residual from the differenced log odds vote share upon a Generalized Entropy measure with γ set to zero using the models from Tables 2, 4, and 5. The next three rows present the results using residuals arising from a log odds vote share equation that controls for the income difference between the 60th and 40th percentile, the 70th and 30th percentile, and the 80th and 20th percentile, respectively, rather than the difference between the 50th and 45th percentiles.

Table 8
Counterfactuals:

		Linear	Constant Elasticity	Constant Elasticity with Preference Heterogeneity
	Income	0.156**	0.855**	0.825**
(1)		(0.063)	(0.229)	(0.213)
School Districts	Tax Price	0.104	0.165	0.192
		(0.368)	(0.183)	(0.163)
	Observations	308	308	308
	Income	0.058	0.444	0.549
(2)		(0.088)	(0.350)	(0.418)
State Assembly	Tax Price	0.529	-0.007	0.122
Districts		(0.737)	(0.236)	(0.238)
	Observations	80	80	80
	Income	0.037**	0.349**	0.352**
(3)		(0.016)	(0.120)	(0.099)
Census Tracts	Tax Price	0.163**	0.059*	0.047*
		(0.068)	(0.032)	(0.027)
	Observations	6840	6840	6840
	Income	0.020*	0.217	0.235
(4)		(0.011)	(0.258)	(0.184)
Census Tracts	Tax Price	0.010	0.015	0.029
with District Effects		(0.086)	(0.027)	(0.032)
	Observations	6840	6840	6840

Notes: The first panel presents the estimates for the third model specification in Tables 2, 4, and 5 for the school district sample. The next three panels present the estimates for the same model specification using data on state assembly districts, census tracts, and census tracts in a model that also controls for school district effects.