THREE ESSAYS IN LOCAL PUBLIC FINANCE

A Dissertation
Presented to the Faculty of the Graduate School
of Cornell University
in Partial Fulfillment of the Requirements for the Degree of
Doctor of Philosophy

by
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January 2017

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Cornell University 2017

ABSTRACT

This dissertation studies local government taxation. I study whether changes in local tax levels impact alternative sources of revenue, how tax changes to fund school facilities affect property values, and how tax limits should be set to maximize the welfare of voters.

In the first essay, I study private donations to public school districts, which while primarily publicly funded government entities, most districts receive. I estimate how local school taxes crowd out private, voluntary contributions to public education. To do this, I exploit quasi-experimental variation in tax revenue stemming from local elections. I collect data from a large set of referenda in which local taxes face voter approval in four Midwestern states, combined with administrative records of the sources of school district revenues. Using a regression discontinuity design around voting thresholds that determine passage of local referenda, I show that private contributions to public school systems are not crowded out by local taxes.

The second essay uses variation in school facilities from local elections to approve capital investment to study whether improved school facilities change the property values of homes in Ohio. These elections allow me to use a regression discontinuity design around the voting threshold that allows school boards to issue bonds. I find no evidence that that is the case in Ohio, in contrast to other researcher's work in California.

The third essay, which is joint work with Stephen Coate, studies the optimal design of fiscal limits, a common feature in local public finance, in the context of a simple political economy model. The model features a single politician and a representative voter. The politician is responsible for choosing the level of taxation for the voter but is biased in favor of higher taxes. The voter sets a tax limit before his/her preferred level of taxation is fully known. The novel feature of the model is that the limit can be overridden, with the voter's approval. The paper solves for the optimal limit and explores how it depends upon the degree of politician bias and the nature of the uncertainty concerning the voter's preferred level of taxation.

Biographical Sketch

Ross Teichert Milton was raised in Massachusetts. He earned a Bachelor of Arts degree from Macalester College in 2008, a Masters of Art degree from Cornell University in 2014, and now a Doctor of Philosophy degree. He is now an Assistant Professor in the Department of Economics at Kansas State University.

Acknowledgments

I am grateful to my advisors Stephen Coate, Ronald Ehrenberg, and Maria Fitzpatrick for their guidance. Without each of their guidance and encouragement as well as their examples as successful, considerate economists I neither would have had the ability nor will to complete this dissertation. To them, I am indebted.

I appreciate and acknowledge the financial support of Cornell University and the Lincoln Institute of Land Policy. Many faculty members, classmates, and friends contributed useful comments and ideas throughout my graduate school career.

Lastly, I thank my parents, Diane Teichert and Don Milton and partner Corina Mommaerts. Without them this work would not have been possible.

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Chapter 1

Crowd-out of Private Contributions to Local Public Goods

Evidence from School Tax Referenda

1.1 Introduction

Public school districts coerce contributions though local and state taxes, but nonetheless receive contributions voluntarily from parents and community members in the form of cash gifts, fundraising, and volunteering. As local preferences and economic conditions change, local governments frequently decide whether and how to adjust the level of spending on schools. If these decisions influence the level of voluntary donations the education system will receive, governments must be aware that as they increase revenue from taxes they will lose revenue from another source. In this paper, I present empirical evidence on the extent to which taxes crowd out private contributions to public school districts.

Public schools are a major category of government spending, totaling $592 billion in

2012, 46% of which came from local sources. Although most goes to higher education institutions, education is the second most prevalent category of charitable contributions in the United States following religious organizations.

Classic economic models of voluntary contribution predict total crowd-out: each dollar of additional government spending will reduce private spending by a dollar (Bergstrom, Blume, and Varian 1986). However, across a variety of sectors, most empirical studies have shown evidence of incomplete crowd-out. This paper provides the first estimates of crowd-out in public education.

My crowd-out estimates rely on minimal empirical assumptions. I exploit quasi-experimental variation in tax revenues that arise from local elections to increase taxes in four Midwestern states: Michigan, Minnesota, Ohio, and Wisconsin. Following the work of Cellini, Ferreira, and Rothstein (2010) in California, I use a regression discontinuity design around the vote threshold that results in the higher taxes prevailing. Under the assumption that unobservable determinants of contributions are continuous at the vote threshold, this isolates variation in tax revenues that is unrelated to other factors by which school districts differ[1].

I use two sources of data from each state in my sample. First, I collect data on a large set of local elections to approve tax increases, or 'tax referenda'. These elections serve as my source of variation in local tax revenues. Second, I combine the election data with administrative reports that detail the sources of all revenues received by each school district, whether from taxes, private contributions, or other sources. Across the four states, my data include 5,376 referenda in 1,605 school districts.

Using this data, I estimate the effect of public spending on voluntary contributions to schools in two steps. In the first stage, I estimate the effect of the passage of an election on local tax revenue. I find that passing an election significantly increases revenues from

1. Isen (2014) also uses a similar design to estimate fiscal spillovers in Ohio.

local taxes by $350. In the second stage, I estimate the effect of tax revenue on private contributions using a 'fuzzy' regression discontinuity, or instrumental variable, approach. My results show that local tax revenues do not crowd out private contributions to local public school districts. Specifically, I can reject that a one-dollar increase in local taxes causes more than a 1.42-cent decrease in contributions.

This paper has four main contributions. First, I provide the most credible estimate of crowd-out of private contributions in the education sector. Most other estimates of crowd-out study non-profits that provide social services (Hungerman 2005; Gruber and Hungerman 2007; Andreoni and Payne 2011a; Boberg-Fazlic and Sharp 2015[2]), and the limited evidence from other sectors suggests that crowd-out may differ across varying types of charities. There is very little evidence for or against crowd out in the education sector[3] and this is the first paper to address the question in the realm of primary and secondary education[4].

Second, I study crowd-out of voluntary contributions directly to government agencies. The existing field estimates exclusively deal with donations to and the charitable activity of non-profit organizations.[5] In these situations, it is not always clear whether the good produced by the organization is a direct substitute for that produced through government funding. When donations go directly to the government, private and public funding are likely to be producing the same good[6].

Third, I study crowd-out by a local government. Compared to goods that benefit a

2. Andreoni and Payne (2013) and Andreoni (2006) review earlier papers in this literature.

3. In an exception to this, Payne (2001) finds that public and private research fundings for universities are positively correlated.

4. Connolly (1997) and Ehrenberg, Rees, and Brewer (1993) study not whether donations are crowded out but whether external grants crowd out internal funding at universities. Jones (2015) finds that the introduction of state lotteries that fund education crowd out donations to higher education.

5. "Voluntary taxation" has been studied in the lab. Li et al. (2011) and Li et al. (2015) implement lab experiments that compare giving to non-profits and governments.

6. This assumes that school finances are fully fungible, which at least in regards to relatively small contributions, I believe to be plausible.

larger region, it may be easier to sustain cooperation to provide local public goods via private contributions because they benefit fewer people. Additionally, changes in the level of school funding come directly from taxes paid by the potential donors. Residents pay these taxes through property taxes that are highly salient.[7] In other settings, increases in federal government spending in a particular state is funded by residents of the entire country, or perhaps only by subsequent generations. For public schools, there is a tight link between increased funding and higher taxes.

Fourth, this setting provides estimates with policy relevance. The most convincing empirical evidence of crowd-out in the literature come from unusual circumstances. Gruber and Hungerman (2007) shows that church charitable activity decreased in response to the New Deal. However, the New Deal is unlikely to happen again and likely produced a fundamentally different response than smaller changes. This paper estimates the crowd-out that results from decisions on the level of public funding that governments make annually.

In addition to the literature on crowd-out, I contribute to a smaller literature on private contributions to public schools. This literature shows that school contributions respond to the perceived quality of schools (Figlio and Kenny 2009) and the size of the school district (Brunner and Sonstelie 2003; Nelson and Gazley 2014). This paper contributes a new source of data on contributions to public schools, administrative records of school finances, and tests whether they respond to fiscal policy.

In the next section, I describe the setting of contributions to public school districts. In Section 1.3, I lay out the empirical regression discontinuity strategy. In Section 1.4, I summarize the data. In Section 1.5, I provide evidence of the validity of the instrument and the effect of passing a referendum on revenues. In Section 1.6, I present the main

7. This salience is often thought to be the source of tax “revolts” that restrict property taxes and Cabral and Hoxby (2012) show that a plurality say that local property taxes are the “worst” tax.

estimates of crowd-out, and in Section 1.7, I conclude.

1.2 Contributions, School Finance, and Crowd-Out

This paper studies how taxes crowd out contributions to public school districts. School district funding is a public good in the sense that it benefits, more or less equally, all households with children in the schools. When a household contemplates contributing to their school, they know that it will benefit all students, not just their own. Without any government funding, families would need to contribute if they wish to keep the school operating. However, schools receive large amounts of government funding, predominantly from state and local governments.

In order to rationalize private donations to public schools, there must be demand for higher funding that goes unmet by taxation. If all households in a district had identical preferences and local governments set funding levels to the voter's optimum then there would be no reason to donate additional funds. This is not likely to be the case. First, without perfect Tiebout sorting heterogeneity in the preferred level of public spending within school districts remains. Second, in most states school districts face constraints in their ability to set funding levels due to state laws intended to decrease funding inequities across districts.

If the public good funding level motivates donors, classic economic models suggest we should observe dollar for dollar crowd-out. From the perspective of a donor, a dollar taken through taxes is identical to a dollar donated so donors will decrease donations by the amount taxed (Bergstrom, Blume, and Varian 1986). In this setting, most people taxed are likely not donors. Nonetheless, standard models result in near total crowd out as long as the number of donors is large. Taxes paid by non-donors appear to donors as increases in spending power. They will desire to spend some, but not all, of this increase on the public good and hence will decrease donations by less than the amount of the non-donor's taxes. With a large enough number of donors decreasing their donations, the crowd-out will approach total crowd-out (Andreoni 1989; Andreoni 2006). While my data do not contain the number of donors to schools, it is likely that there are many. In 2012, 58 percent of parents reported being involved in school fundraising in some capacity (NCES 2015).

However, if donors give for reasons other than funding a public good, taxation may not be a perfect substitute for donations and crowd out will be less than total. Andreoni (1990) suggests that donations cause a "warm glow" that taxes do not. Donations may also have a social value in signaling a desirable quality (Glazer and Konrad 1996). In the case of giving to schools, both are possible. Parents may feel good about supporting their child's school and local business owners may seek to increase their stature among customers through donations.

The analysis presented in this paper uses data from four Midwestern states: Michigan, Minnesota, Ohio, and Wisconsin. In all four states, most school districts receive donations. However, as in all states, the vast majority of funding comes from a combination of state, local, and to a lesser extent federal taxes. The relative shares of these sources varies.

In Michigan, Minnesota, Ohio, and Wisconsin, the state government sets a base funding level per pupil. A combination of local taxes and transfers from the state fund this base level. Districts then have some abilities to raise revenues beyond this level. What these abilities allow them to do varies by state. In Michigan, local districts only have the authority to increase revenues for capital expenses. In Michigan, Minnesota, and Ohio, districts can increase revenues for both capital and operational expenses. In most cases, to increase taxes the school board must receive approval from the voters[8].

8. For operational expenses, the school board may alter taxes without voter approval if they remain below a state set level. To exceed this cap, they must ask the voters. In Michigan, Minnesota, and Ohio all capital expenses requiring issuing debt require voter approval. In Wisconsin, only indebtedness exceeding a debt cap requires voter approval.

If districts increase revenues, they face differing 'tax prices' between and within the four states. A district's tax price is additional revenue that the district will have to fund via local taxes in order to increase spending by a dollar. If the tax price is less than one, the state pays the difference. In most states, these formulas resulted from school finance reforms since the 1970's (Hoxby 2001)[9].

Table I summarizes major revenue and policy features of the four states and compares them to United States averages. The table only partially reflects the differences in their ability to increase revenues. Districts in Michigan and Minnesota get a smaller portion of their revenues from local sources than those in Ohio and Wisconsin. However, because local revenues fund some of the revenue base that the state sets, local taxes partly fund districts in Michigan, even though the districts have no ability to increase revenues.

Whether caused by heterogeneous preferences or limits on taxation, private contributions to public schools, as reported to the federal government, increased from $789 million in 2005-2006 to $1.03 billion in 2012-2013.[10] 64% of school districts reported receiving some private contributions.[11] Figure I displays county level averages of school contributions per student across the United States. Schools receive private contributions in all regions of the country and they are not limited only to urban areas. The four states in the analysis all have relatively high levels of contributions and variation within them.

In most cases in all four states, districts that want to increase taxes must send their residents to the polls. In all these cases, the school board proposes a new tax level. Then the voters approve or reject the proposal. If they reject it, taxes revert to a fallback level[12].

9. These tax prices differ for capital and operational spending. Michigan has a tax cost of one for capital spending while Ohio's is roughly .50 (Duncombe and Wang 2009). In Minnesota and Wisconsin tax prices differ by the size of the districts tax base and by their level of spending.

10. Author's calculation in 2014 dollars from Census/NCES Annual Survey of School System Finances. Includes only donations from those states reporting to the federal government which in 2012-13 was all but eight states.

11. The 64% of school districts receiving contributions enroll 78% of public school students.

12. For capital spending referenda, the fallback level is the prior level of taxes. For operational spending referenda, the fallback level varies across states. In Ohio, fallback levels are usually lower in real terms than

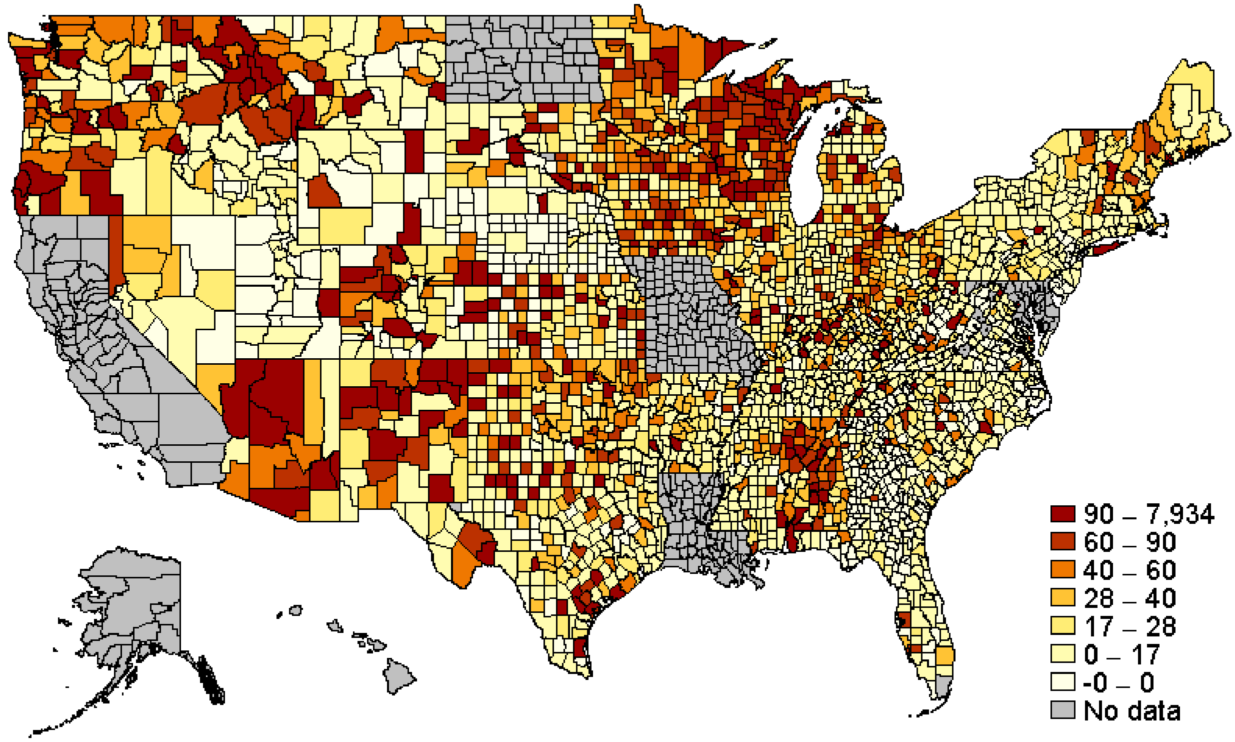


Figure I: Maps of average contributions to schools per student, by county

Note: For each county, the map shows the average level of contributions per student across all districts. Data from the 2013 Annual Survey of School System Finances, part of the U.S. Census Bureau's Annual Surveys of State and Local Government Finances.

Table I: Comparison of school finances in 4 Midwestern states

	Mich	Minn	Ohio	Wisc	US
# Districts	544	339	612	424	13,569
Avg # of students	2,539	2,367	2,637	2,037	3,560
Avg expenditures per student	$11,602	$13,650	$12,124	$13,345	$14,226
Local tax revenue per student	$4,532	$3,748	$6,221	$7,460	$6,739
% revenue from local tax	35.2	27.2	49.3	53.2	43.6
Private contributions per student	41.03	50.93	41.60	77.27	31.21
Able to increase operational budget?	No	Yes	Yes	Yes	
Able to increase capital budget?	Yes	Yes	Yes	Yes	
State median household income	48,273	60,702	48,081	51,467	52,250

Note: All school figures from the 2012-13 fiscal year. All dollar figures are in 2014 dollars. Median household income from 2013 Census American Community Survey estimates.

1.3 Regression Discontinuity Strategy

Estimating the extent of crowd out requires estimating the causal impact of tax revenues on private contributions to schools. A simple regression of contributions on tax revenues is unlikely to recover this parameter, although the direction of bias is unclear. The reasons that school districts have high tax revenues may also affect the level of contributions to schools. Districts whose residents prefer high levels of school spending are likely to have high tax revenues, but this might also lead them to make larger contributions to the schools. However, if districts with high levels of tax revenue tend to be more homogenous in their desired level of spending, they might be satisfied with the level of tax funding and not make additional private contributions to schools.

The requirement that voters approve tax changes through referenda creates an opportunity to estimate the causal effect of taxes on contributions. Because some elections fail while others pass, districts who proposed tax changes will vary in their tax revenues in subsequent years. While districts whose referenda pass may differ systematically from those whose fail, comparing referenda close to the election minimizes this bias. As long as there is some randomness in the portion of voters that vote in favor, elections close enough to the threshold of passing approximate a random experiment (Lee 2008). This creates quasi-random variation in the level of tax revenue between districts where the vote narrowly failed and those where it barely passed. I exploit this variation with a "fuzzy" regression discontinuity design to estimate the impact of tax revenues on private contributions to schools.

My analysis proceeds in two steps. First, I estimate the fiscal impacts of passing a referendum, a sharp regression discontinuity. Second, using this impact as the first stage in an instrumental variables (or fuzzy regression discontinuity) estimation, I estimate the

the prior year, as the tax system is defined in nominal terms. In Wisconsin and Minnesota, fallback levels depend on whether a tax approved in previous years is expiring.

impact of local tax revenues on private contributions. In the remainder of this section, I describe these steps in more detail.

1.3.1 The effect of referenda on taxes

I use a regression discontinuity design to estimate the effect of voters approving a referendum on the level of tax revenues from local sources. Since the purpose of each referendum is explicitly to approve higher taxes than what would occur if it failed, passage should cause higher tax revenues. Aside from any effects of the passage of the referendum, districts whose referendum came just short of passing should be similar to those whose referendum barely passed.

Following the regression discontinuity literature, I do not assume that referenda close enough to the passage threshold are directly comparable. Instead, I assume that any differences between them can be accounted for by controlling for the election's vote share. By orienting the estimation around each election, rather than around the district, I treat referenda as events and estimate their effect on tax revenues. Considering, for now, only the fiscal year that follows a referendum to authorize taxes, a regression discontinuity model can estimate the impact of the referendum passing on tax revenues. This results in the estimation equation:

$$R_i = f(V_i) + \gamma P_i + \epsilon_i \tag{1.1}$$

in which R_i is the level of local tax revenues one year after the vote in the district in which referendum i occurred. V_i is the vote share in favor. P_i is a binary variable indicating whether referendum i passed and γ represents the effect of passing a referendum on tax revenues one year later. While this yields a consistent estimate of γ under the assumptions of the regression discontinuity design, it is inefficient.

Much of the variation in school district revenues is across districts, but does not vary over time. In Equation 1.1, these district specific, but time invariant characteristics end up in ϵ. However, data from prior to the referendum provide information on the levels of these characteristics. Including these data in the model can reduce the residual variation and yield a more precise estimate.

To use this information on constant characteristics, I follow Cellini, Ferreira, and Rothstein (2010) and create a panel that contains observations both before and after each referendum. In districts with referenda in multiple years, data that correspond to one district in one year can end up as multiple observations, each associated with a different referendum.[13] This design treats each referenda as entirely separate events.

By organizing the dataset in this fashion, I can estimate a separate parameter for the effect of passing a referendum on revenues in each year after the vote. By including observations from before the referenda and constraining the referendum's effect to zero in those years, I can include referendum fixed effects, which control for the time invariant characteristics of the district. I estimate the equation:

$$R_{i\tau} = f_\tau(V_i) + \gamma_\tau P_i + \zeta_i + \alpha_{s\tau} + \kappa_{s,t(i)+\tau} + u_{i\tau} \tag{1.2}$$

where $R_{i\tau}$ is the level of local tax revenues τ years after the vote in the district in which referendum i occurred. γ_τ now represents the effect of passing a referendum on tax revenues τ years later and is constrained to be zero for all $\tau < 0$. $f_\tau()$ controls for the relationship between the vote share on the referendum's vote and the outcome, and is also set to be zero prior to the vote[14]. ζ represents referendum fixed effects. α represents fixed effects for each state by year relative to the election. Lastly, $t(i)$ represents the fiscal year in

13. For example if a district had referenda in both 1998 and 2000, the data corresponding to 1999 would end up in the new panel associated once with each referenda. In one case, it would represent the year after a referendum and in the other the year before.

14. Since there is no regression discontinuity being estimated for years prior to the vote, it is not necessary to control for this relationship.

which vote i occurred so, κ represents state by fiscal year fixed effects.

This equation allows me to estimate efficiently the impact of passing a referendum on tax revenues in subsequent years. I will use the passage of these referendum, P_i, as an instrument for tax revenues in the second stage.

1.3.2 The effect of taxes on contributions

As long as passing a referendum has an effect on revenues, this can act as an instrument for tax revenues, enabling me to estimate the effect of tax revenues on contributions. This requires assuming that passing a referenda only effects contributions through its effect on tax revenues. Here, I am taking tax revenues to be a proxy for the funding of the public schools. Expenditures would be another possible proxy, but expenditures are lumpy due to the nature of capital investments and so are not a good measure of the level of the good produced.

Passing a referendum may affect contributions due to a few channels. First, potential contributors (as long as they live in the school district) must now pay higher taxes, leaving them less money to spend on contributions and other goods. Second, the schools are now receiving more money, perhaps lessening the need for the contributions. Third, there may be some impact on how people feel about giving to the public schools. The first two come through the level of tax revenues. The third may or may not depending on what the source of that feeling is, but I would argue that this is largely a semantic distinction.

Using the passage of a referendum as an instrument for revenues results in a fuzzy regression discontinuity design. First, I use this to estimate the impact of taxes on contemporaneous contributions to schools. To accomplish this, I use Equation 1.2 as the first stage, limited to those observations prior to the election ($\tau < 0$) and the first year following the election ($\tau = 1$).[15] The second stage estimation equation is given by:

15. Additional observations following the vote are used to estimate the dynamic impact of taxes in the next

$$C_{i\tau} = f_\tau(V_i) + \beta R_{i\tau} + \eta_j + \delta_{s\tau} + \lambda_{s,t(i)+\tau} + \varepsilon_{i\tau} \tag{1.3}$$

where $C_{i\tau}$ is the amount of private contributions per student τ years after the vote in the district where referendum i occurred and $R_{i\tau}$ represents tax revenues in that same district and year. δ, ζ, and η represent fixed effects as in the first stage Equation 1.2. Unlike in the first stage, P_i the passage of a referendum is excluded. Instead it is used to instrument for $R_{i\tau}$. β represents the impact of tax revenues on the same year's contributions. With only observations from one year following the referenda included in the estimation, there the model is just identified.

1.3.2.1 Dynamic effects of taxes

The models described in the previous section estimate only the effect of taxes on contemporaneous contributions. These use only data from one year after the referenda, when the first stage is likely to be strongest. If potential donors take more than one year to react to tax changes and adjust their contributions, these estimates would not show the full impact of taxes on contributions. In this section, I extend the analysis to the dynamic impact of taxes on contributions.

Rather than estimating only the effect of contemporaneous taxes on contributions, I allow the history of taxes to affect today's contributions. As when estimating only the contemporaneous effect, I can estimate the effect of current and lagged tax revenues on contributions in a fuzzy regression discontinuity design by instrumenting for revenues and past revenues with the passage of referenda. Since I am now estimating multiple parameters, I require multiple instruments. It is natural to allow the effect of passing a referendum to vary depending on the number of years since the referendum. This allows

section.

me to identify the dynamic impacts of taxes because passing a referendum does not have an equal impact on revenues in all subsequent years. This results in a new estimation equation:

$$C_{i\tau} = \sum_{k=0}^{T} \beta_k R_{i,\tau-k} + f_\tau(V_i) + \eta_i + \delta_{s\tau} + \lambda_{s,t(i)+\tau} + u_{i\tau} \tag{1.4}$$

where β_k captures the impact of tax revenues per student k years earlier on contributions per student.[16] While in theory, the potential donors could respond to the complete history of taxes, ($T = \infty$) in practice, an assumption must be made over this timespan, T. For the contemporaneous estimation, I included observations from only one year after a referendum passed; this estimation requires additional observations following the vote.

These structural parameters of the effect of current and lagged taxes on contributions to schools describe the dynamic effect of a tax change. Using them, I can describe the effect in following years of any changes in tax revenues. To illustrate crowd-out, I use these parameters to calculate the impact of a one-dollar increase in taxes on contributions τ years later, which is equal to $\sum_{k=0}^{\tau} \beta_k$.

1.3.3 Estimation

Estimating both the effect of referenda on revenues and the effect of revenues on contributions requires controlling for the relationship between the share of votes in favor and the outcome. In the models above, the $f()$ function represents this control. The regression discontinuity literature commonly uses either a "global polynomial" approach, where f is a flexible function of the vote share estimated over all observations, or a local linear approach, where f is a linear relationship and the estimator uses only observations within some bandwidth from the threshold. I do the latter. Imbens and Kalyanaraman (2011)

16. This assumes that contributions do not depend on future taxes.

shows how to calculate the bandwidth that minimizes the asymptotic mean squared error of the estimate. I calculate the optimal bandwidth to estimate the effect of referenda on revenues using their formula with revenues residualized by the fixed effects in the model as the outcome. To calculate the optimal bandwidth for the instrumental variables model, I use the Imbens and Kalyanaraman (2011) formula with contributions residualized by the fixed effects in the model as the outcome. Imbens and Kalyanaraman (2011) argue that while this is the optimal bandwidth for the reduced form model, in practice it will differ little from the optimal bandwidth for the full instrumental variables model. In addition, I use a range of alternative bandwidths to test the robustness of the results.

This provides an unbiased estimate of γ under the now standard regression discontinuity assumption that the potential outcome functions, $E[R|P = 1, V = v]$ and $E[R|P = 0, V = v]$ are continuous at the passage threshold, v (Imbens and Lemieux 2008). If agents selecting V have "Imperfect control," and cannot choose an exact value, it creates some randomness in V, which justifies this assumption (Lee and Lemieux 2010). In this setting, perfect control would be evidence of voter fraud.[17] Although I cannot test this assumption directly, I provide evidence that there is no discontinuity in district characteristics from prior to the election in Section 1.5.

1.4 Data

Estimating the models described in the previous section requires data on school district fiscal information, including private contributions, tax revenues, and records of local referenda to raise taxes for schools. I collect these data from four Midwestern states, Michigan, Minnesota, Ohio, and Wisconsin. In this section, I describe the datasets used in

17. In some other settings using elections for RD estimates, there is evidence that this assumption fails. For example, close unionization elections tend to swing against the union when Republicans control the National Labor Relations Board and for the union when Democrats do (Frandsen 2014). However, these issues are unlikely to apply here.

my analysis.

1.4.1 Fiscal Data

To estimate the impacts on private contributions to schools, I use data from administrative school finance records from four states. This is a previously unused dataset of information on contributions to schools.

Previous research on private contributions to schools primarily used non-profit tax filings to determine the quantity of donations received by local schools (Brunner and Sonstelie 2003; Brunner and Imazeki 2004; Nelson and Gazley 2014). As discussed by Figlio and Kenny (2009), these data are potentially inaccurate for two reasons. First, only non-profits with greater than $50,000 in revenues are required to file with the IRS.[18] In 2013, 65.3% of school districts that reported receiving some contributions received less than $50,000.[19] Second, it requires identifying non-profits that support a given school. These papers typically use keywords in the organization's name and categorization along with the address given in the tax filings to determine the district in which the organization is located. This is necessarily inexact[20].

Rather than relying on IRS filings or survey reports, I use new data from administrative reports of school district revenue from private contributions. States require that local school districts report detailed accounting information to state officials. These include detailed accounts of the sources of all revenues. In most states, one subcategory of local revenues is private contributions. The Wisconsin Uniform Financial Accounting Requirements manual

18. Prior to tax year 2010, this threshold was $25,000. While organizations with revenues under these amounts can voluntarily file 990 forms, they are not required to.

19. In fact, this understates the problem since non-profit organizations often support an individual school rather than the entire district. Less aggregation among the nonprofits makes it likely that fewer of them will reach the $50,000 threshold for filing.

20. A third reason, argued by Figlio and Kenny (2009), is that the same amount of eventual donations to the school may result in different organization revenues reported to the IRS, depending on whether the organization for instance, raises revenue by buying and reselling goods or sells donated items.

states that this includes “Gifts, fundraising, contributions, and development.” This does not include grants that the district may have received.

As part of the Annual Surveys of State and Local Government Finances, the U.S. Census Bureau conducts an Annual Survey of School System Finances. Although called a survey, it compiles accounting data already collected by states into a standardized national format. All tax revenue and expenditure data used herein comes from this source. Beginning in fiscal year 2006, these data include information on revenues from private contributions.

In order to increase the size of my sample, I collected additional administrative data of school district finances from further back in time. I collected these data as reported to their state government through public records requests to state departments of education.

I obtained these data for the fiscal years 1992 through 2013 in Wisconsin, in Minnesota for the fiscal years 2001 through 2013, and in Michigan for the fiscal years 2004 through 2013. Because this is in essence the same data source, for years in which both sources are available, they match well.[21] During the years covered by these data there have been some changes to the reporting of private contributions. In 2004, Wisconsin altered their accounting system which nearly doubled the average level of reported private contributions. Changes like this make it difficult to show changes over time in contributions accurately. However, they do not bias the models presented above, which all include state by year fixed effects that can control for these changes.

1.4.2 Referenda Data

I compiled a database of bond and tax referenda for the four states from a variety of sources. The Wisconsin Department of Public Instruction catalogs referenda from 1990 through 2014. This includes referenda to approve bond sales for capital projects, to approve “recurring”

21. The median absolute difference between the census and state data in contributions per student is $0.12 and the 95th percentile is $5.05.

taxes that permanently raise the tax revenue limit, and to approve "non-recurring" taxes that temporarily raise tax rates without permanently changing the limit.

Ohio referenda come from three sources. First, I use referenda data as collected from the Ohio School Board Association and the Ohio Secretary of State for years 2008 through 2013 by Kogan, Lavertu, and Peskowitz (2015). Second, I add bond election records for 1985-2012 from the Ohio Municipal Advisory Council. Third, I include updated records through 2014 from the Ohio School Boards Association. In total, these data include referenda to approve both general fund tax increases and to approve bond sales for capital investments.

Michigan referenda come from the state Department of Treasury for the years 1996 through 2014. As discussed in Section 1.2, Michigan school districts cannot increase taxes for operational expenses, so these data cover only referenda to approve bond sales for capital investments(Conlin and Thompson 2014). Specifically, these data cover those bonds accepted by the state under the Michigan School Bond Loan Program that allows districts to borrow at lower interest rates.

1.4.3 Descriptive Evidence

This research design depends upon districts proposing tax changes that require voter approval. My analysis is limited to districts where the school board brought tax proposals before the voters. This is not required; school boards have the option of staying below the tax levels that require voter approval. As a result, districts in which votes are required may differ from those that do not. If this were the case, the estimates that result would not necessarily be valid among a broader range of districts. To investigate this, Panel 1 of Table II compares summary statistics in 2012-13 for districts who never vote on a referendum, who vote on at least one referendum, and those who have a referendum that falls close to the threshold. Approximately half of districts in the four states hold a referendum in the sample period. Those that never hold a referendum have higher revenues and

Table II: Summary statistics for school districts with and without referenda.

		(1) District Characteristics			(2) All Votes		(3) Narrow Votes	
	All districts	Never has ref	1+ refs	1+ close refs	Failed	Passed	Failed	Passed
	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd	mean/sd
N	3,264	1,659	1,605	467	1,808	3,570	687	1,049
Avg # of refs	1.64	0.00	3.34	3.37	4.65	4.34	4.77	4.43
	(2.15)	(0.00)	(1.92)	(1.90)	(1.89)	(2.01)	(1.77)	(2.07)
Expenditures PP	13,375	14,458	12,455	12,389	11,965	12,083	11,957	12,088
	(5,342)	(6,901)	(3,241)	(3,127)	(2,659)	(3,041)	(2,657)	(2,836)
Total Revenue PP	13,484	14,630	12,511	12,482	11,931	12,172	11,936	12,129
	(4,987)	(6,665)	(2,493)	(2,241)	(2,182)	(2,579)	(2,217)	(2,437)
Local Tax Rev PP	6,306	7,266	5,491	5,660	4,852	5,254	4,995	5,347
	(4,317)	(5,265)	(3,079)	(2,928)	(2,364)	(2,821)	(2,506)	(2,622)
% Rev fr Local Tax	44.86	47.34	42.74	44.27	40.44	42.70	41.65	43.75
	(18.57)	(19.22)	(17.74)	(18.05)	(16.88)	(18.13)	(18.03)	(17.52)
Contributions PP	49.9	48.2	51.3	50.8	30.7	32.9	36.1	31.6
	(81.8)	(88.3)	(75.9)	(75.1)	(55.7)	(55.2)	(63.9)	(55.5)
Enrollment	2,298	2,098	2,505	2,640	2,793	2,781	3,158	3,045
	(4,928)	(5,726)	(3,927)	(3,591)	(4,006)	(4,485)	(4,757)	(3,977)

Note: Panel (1) shows mean district characteristics by whether they are observed proposing any tax increases and if a increase passes or fails by a margin of less than 10%. Panel (2) shows mean characteristics from the year prior to the vote, for each referenda observed, by whether the vote passed or failed. Dollar figures are per pupil in 2014 dollars. Enrollment and Fiscal data from Census/NCES Annual Survey of School System Finances.

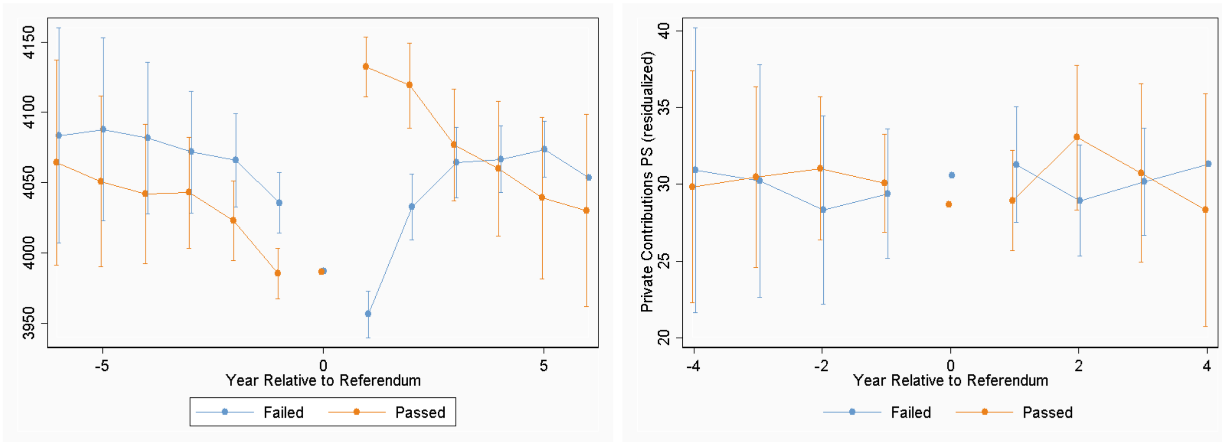
expenditures on average than those that do. In Ohio, districts require voter approval to exceed a tax rate not a tax level, so districts with high property values may not choose to hold referenda.

Because this is a regression discontinuity design, I am effectively estimating a local average treatment effect for those districts that have referenda near the threshold. The likelihood that this study's results would hold true in other districts depends in part, on whether districts with referenda near the passing threshold are unusual.

Districts that have at least one referenda are very similar on observable characteristics to those that have at least one close referenda. Panel 2 of Table II compares the characteristics of districts where a referendum failed the next year to those where a referendum passed the next year. On observable characteristics, they are remarkably similar. Referenda that passed tend to be in districts that received slightly more private contributions, though the difference is far from statistically significant.

Before turning to the regression discontinuity results, I examine briefly what the raw data show. Figure IIa plots local tax revenues per student before and after a referendum separately for those that fail and those that pass, after controlling for state by year effects. Prior to the vote, districts whose referenda later pass are trending similarly to those whose referenda fail. One and two years following the vote, the difference between districts where a referendum passed vs those where one failed is unmistakable. Three years after the vote, this difference has eroded. As in Cellini, Ferreira, and Rothstein (2010), this is because districts where a referendum failed are far more likely to pass one in subsequent years than those where one passed. School boards often decrease the tax level they are asking for and propose a new increase one or two years later.

Likewise, Figure IIb shows the trends in private contributions per student leading up to and following a referendum. Here, there appears to be no impact of passing a referendum. Districts whose referenda pass and those whose referenda fail look similar both before and



(a) Local tax revenues

(b) Private contributions

Figure II: Time trends before and after votes on referenda

Note: Graph shows the trends leading up to passage or failure among all referenda (not only those close to the passage threshold). Y axis shows the mean residual from a regression of the variable indicated on referenda and state by year fixed effects. Error bars come from a regression of the outcome on referenda, state by year fixed effects, and relative year by passage fixed effects with relative year zero omitted. Graph is balanced for relative years [-6,4], in that all points represent data from the same set of referenda; years outside that range include all referenda with data.

afterwards. Together, this is suggestive that the additional tax revenues that result from passing a referendum do not crowd out contributions.

Since these graphs compare taxes and contributions both before and after a referendum, time-invariant district characteristics would not result in a false conclusion. However, districts might pass tax referenda because of time varying characteristics. For instance, new residential construction might require new funding and change the composition of potential donors. Were this the case, the conclusion from this simple analysis could be unfounded.

This is the advantage of the regression discontinuity design. Rather than assuming that districts whose referenda failed are an adequate counter-factual for those whose referenda passed, I focus instead on those districts whose referenda passed or failed narrowly.

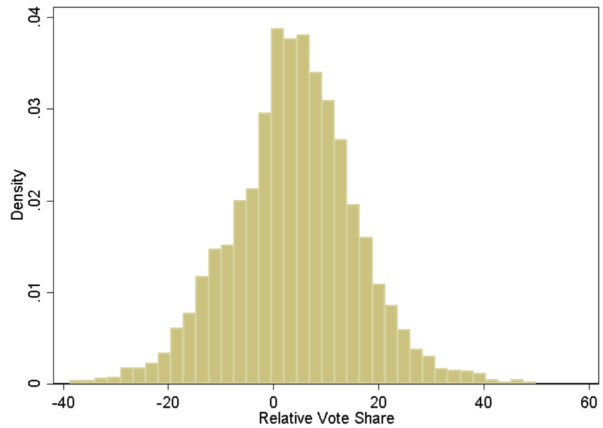


Figure III: Histogram of referenda vote shares

Note: Histogram of vote shares relative to the passage threshold.

1.5 Instrument Validity and the Effect of Referenda on Revenues

In this section, I first show standard tests for validity of the regression discontinuity design. I then show that the design produces a strong first stage result of the effect of referendum passage on revenues.

While there may be political campaigns operating both for and against a referendum, it should not be possible for any actor to exactly target an election vote share. In any election, even with well-run campaigns, there is uncertainty about who will go to the polls and how they will vote. If this were not the case, we would expect to see excess mass in the distribution of vote shares around the passage threshold. Figure III presents a histogram of vote shares relative to the threshold across all referenda. There is no evidence of clumping around the threshold.

If agents are unable to manipulate which side of the threshold a referendum ends up, then unobservable characteristics of districts should be continuous across the passage threshold. While this is not directly testable, I provide evidence in support by testing

Table III: Effect of passing a referendum on prior characteristics

	1 yr. before	2 yr. before	3 yr. before	4 yr. before
Exp. PS	135	-91	-483*	-341
	(182)	(223)	(220)	(196)
Bandwidth	11.30	8.59	8.05	10.85
Curr. Exp PS	-100.702	-77.212	-189.240*	-104.943
	(100.112)	(79.018)	(95.389)	(92.650)
Bandwidth	8.81	14.16	8.11	8.24
Cap. out. PS	252.300*	-3.059	-177.405	-118.218
	(113.985)	(146.772)	(174.990)	(159.189)
Bandwidth	13.11	9.01	6.02	8.75
Rev. PS	53	1	-265	-72
	(159)	(156)	(150)	(119)
Bandwidth	8.18	8.57	9.10	14.40
St. rev PS	173	121	66	108
	(158)	(151)	(170)	(166)
Bandwidth	7.23	7.18	5.56	5.89
Loc. tax rev. PS	-218	-242	-145	-232
	(168)	(168)	(165)	(151)
Bandwidth	5.98	5.94	6.18	6.94

Note: Regression discontinuity estimates of the effect of passing a referendum on outcomes fixed before the referenda. Each estimate comes from a separate regression that includes only referenda within the bandwidth shown of the passage threshold and include linear relationships between the vote share and the outcomes on either side of the threshold. Each model includes state by year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

whether district characteristics in the years prior to a referendum are discontinuous at the threshold. Table III presents estimates of these discontinuities. The estimates of these discontinuities differ significantly from zero at the 5% confidence level in only 3 of 24 of the tests. Furthermore, there is no discernible pattern to these results[22].

In order to use the regression discontinuity as an instrument for district revenues there

22. When the full estimation strategy with a panel data set is estimated as if the referendum occurred in a year prior to when it actually did, 6 of 20 estimates differ from zero at the 5% level. However, unlike the effect on post-referenda fiscal outcomes, these results disappear when the model includes referenda-specific time trends. Appendix A.1 shows these results.

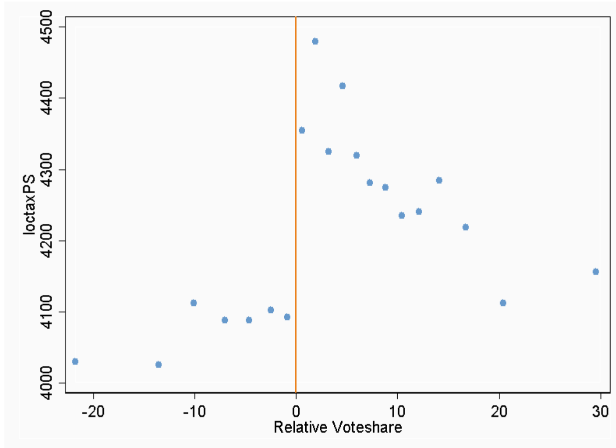


Figure IV: Graphical depiction of effect of referenda passage on local taxes one year later: Average local taxes per student by vote share.

Note: These figures graphically present the first stage impact of referenda passage on local tax revenues per student, γ_1 in Equation 1.2. To construct these figures I use the panel dataset described in Section 1.5 and regress local taxes per student on fixed effects for the referenda, state by year, and state by year relative to the referenda. I then split the observations into quantiles of vote shares, calculate the mean residual in each bin, and add back the mean level of local tax revenues per student.

must be a strong discontinuity in revenues at the passing threshold. I first demonstrate this graphically. Figure IV shows the mean level of tax revenues by bins of the share of votes in favor of passing the referenda, relative to the votes required. Due to the panel nature of the empirical strategy, it is worth taking a moment to describe the creation of this graph. I split observations in the year following a referendum into quantiles of the vote share. Rather than calculating the mean of the unconditional level of tax revenues in each quantile, I regress local taxes per student on fixed effects for the referenda, state by year, and state by year relative to the referenda. I then calculate the mean residual from this regression for each quantile, add back the mean level of local tax revenues per student and plot these values.

It is clear from Figure IV that local tax revenues are discontinuous at the passage threshold. Table IV displays a point estimate corresponding to this effect, as well as the effect on subsequent years. These estimates come from estimating Equation 1.2. One year following the vote, districts whose referenda passed have \$349 higher revenues than those whose failed. There is a strong discontinuity at the referenda passage threshold in total expenditures, current expenditure, capital outlays, and local tax revenues per student. These are apparent but weak the year of the referenda, very strong one year after and weaken thereafter.[23] None are statistically significant four years after the vote.

I use the passage of a referendum as an instrument for public funding for schools. To do this requires a measure of the level of public funding. The two obvious possibilities are expenditures and revenues, both of which passing a referendum affects. However, expenditures are not a good measure of the additional provision of school services the referenda has provided. Because many referenda authorize capital expenditures they have a much larger impact on expenditures than they do on revenues. However, while capital expenditures are spent in large sums, households feel their benefits over many years. The expenditure itself is not a good measure of the provision of the public good. As a result, it is preferable to focus on tax revenues. With this strong instrument for revenues in hand, I turn to the main results.

1.6 Results

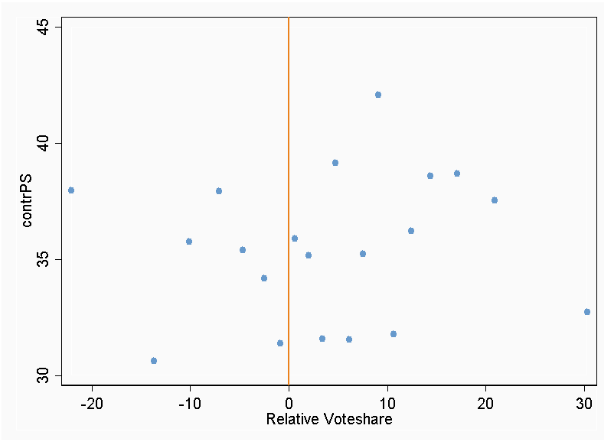
In the sections that follow, I describe the results of these methods. First, I report the main results of how tax revenues crowd out private contributions. Then, I discuss how to interpret this result given the level of contributions relative to taxes.

23. These first stage results are robust to a number of different specifications. In particular, this first stage is detectable without the panel data structure using the simple regression discontinuity design shown in Equation 1.1. In addition, adding referenda specific time trends to the panel data equation leaves the results qualitatively similar.

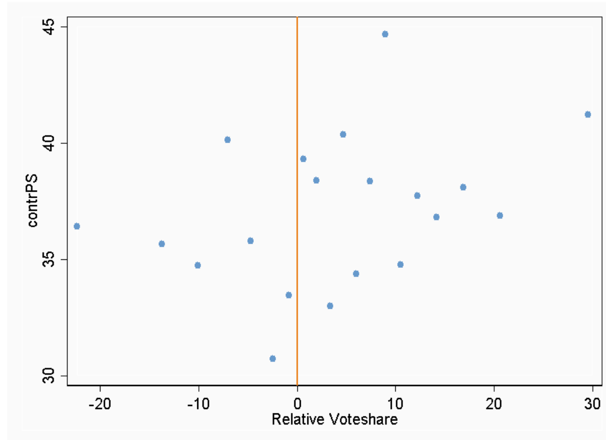
Table IV: Effect of passing a referendum on expenditures and revenues

	Yr. of ref	1 yr. later	2 yr. later	3 yr. later	4 yr. later
Exp. PS	522**	1,819**	1,401**	480	-123
	(181)	(243)	(312)	(326)	(282)
Bandwidth	6.93	7.35	7.69	8.03	7.78
Curr. Exp PS	108.656*	270.829**	192.257**	258.120**	54.547
	(42.711)	(67.784)	(48.993)	(71.546)	(88.615)
Bandwidth	11.12	6.76	16.84	9.56	8.76
Cap. out. PS	334.057*	1,279.985**	881.609**	-7.305	-197.654
	(146.183)	(181.359)	(246.738)	(249.617)	(218.156)
Bandwidth	6.87	8.79	6.47	8.11	8.41
Loc. tax rev. PS	130**	349**	190**	176**	127
	(44)	(54)	(70)	(68)	(77)
Bandwidth	8.90	8.87	7.63	12.00	11.44

Note: Regression discontinuity estimates of the effect of passing a referendum on fiscal outcomes following the referenda. Each estimate comes from a separate regression that includes only referenda within the bandwidth shown of the passage threshold and include linear relationships between the vote share and the outcomes on either side of the threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations prior to the referenda and from one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.



(a) 1 Year After



(b) 2 Years After

Figure V: Average contributions by vote share, one and two years after referenda

Note: These figures graphically present the reduced form impact of referenda passage on contributions, γ_1 and γ_2 in Equation 1.2, but with the outcome of contributions per student. To construct these figures I use the panel dataset described in Section 1.5 and regress contributions per student on fixed effects for the referenda, state by year, and state by year relative to the referenda. I then split the observations into quantiles of vote shares, calculate the mean residual in each bin, and add back the mean contributions per student.

1.6.1 Effect of taxes on contributions

To examine the results of taxes on contributions, I begin with a graphical analysis of the effect of passing a referendum on contributions to schools. Figure V shows this reduced form impact of the passage of a referendum on private contributions per student to the school district one (a) and two (b) years after the vote. The construction of this figure is akin to Figure IV and is discussed in Section 1.5. Unlike in Figure IV, there does not appear to be any shift in contributions at the passage threshold.

Table V shows point estimates of the effect of tax revenues on private contributions from the fuzzy regression discontinuity design. Column 1 shows results from Equations 1.2 and 1.3 as the first and second stages respectively. A one-dollar increase in local revenues increases slightly contributions per student by 0.62 cents. While this estimate is not significantly different from zero, there is no evidence of crowd-out. In fact, the estimate allows crowding out of more than 1.42 cents per dollar to be rejected along with crowd in

Table V: Regression discontinuity - instrumental variables estimates of the effect of local tax revenues on private contributions.

	(1)	(2)
Loc. tax rev. PS	0.0062	0.0092
	(0.0104)	(0.0131)
Bandwidth	10.58	12.00
Referenda specific trends	No	Yes

Note: Both columns are estimated by Equations 1.2 and 1.3 as the first and second stages respectively, but Column 2 also controls for referenda-specific time trends in both stages. Both include only referenda within the bandwidth shown of the passage threshold and include linear relationships between the vote share and the outcomes on either side of the threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations prior to the referenda and from one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

of more than 2.66 cents per dollar.

This result is robust to other specifications. Because there is more than one year of data prior to each referenda, referenda specific trends are identified along with the referenda fixed effects that are already present. This controls for the possibility of differing pre-trends across the passage threshold. Column 2 shows the results with this addition. The point estimate moves only slightly. The result is also robust to alternative bandwidths which is shown in Appendix A.3.

1.6.1.1 Dynamic effects

Panel A of Table VI shows the effect of contemporaneous and lagged tax revenues on contributions estimated using Equations 1.2 and 1.4 as the first and second stages respectively. β_k represents the effect of tax revenues per student k years earlier on contributions per student. These estimates are not significantly different from zero at any point, and they rule out large effects of contemporaneous or lagged revenues on contributions. However,

Table VI: Dynamic Effects of Taxes on Contributions

A. Structural parameters						
β_0	β_1	β_2	β_3	β_4	β_5	β_6
0.007	0.003	0.014	-0.009	-0.018	0.003	-0.017
(0.011)	(0.013)	(0.013)	(0.014)	(0.014)	(0.012)	(0.014)
B. Total effect of \$1 increase in taxes on contributions						
Year of change	1 yr later	2 yrs later	3 yrs later	4 yrs later	5 yrs later	6 yrs later
0.007	0.010	0.024	0.015	-0.002	0.001	-0.016
(0.011)	(0.014)	(0.014)	(0.018)	(0.022)	(0.024)	(0.032)

Note: Panel A shows the effect of contemporaneous and lagged revenues on contributions with parameters as specified in Equation 1.4, estimated with a fuzzy regression discontinuity design with revenues instrumented for with the passage of referenda in prior years. Panel B shows the effect of a one-dollar increase in taxes on contributions in subsequent years calculated using the estimates in Panel A.

they are difficult to interpret. When manipulating taxes, policy-makers are likely to change more than only one-year's taxes.

Using these parameters, Figure VI shows the effect of a permanent one-dollar increase in taxes on contributions in the subsequent years. In the year of the tax change, the change only influences contributions through its effect of contemporaneous taxes. One year after the tax change, it can influence contributions through both contemporaneous and the prior year's taxes. As in the estimates presented in Section 1.6.1, there is no evidence of crowd out. More than two years after the tax increase, the confidence interval expands as more parameters are involved in the calculation. Panel B of Table VI shows the point estimates associated with this graph. In the year of the tax increase and the following two years, the estimates allow rejecting crowd out larger than 1.46, 1.74, and 0.34 cents per dollar respectively.

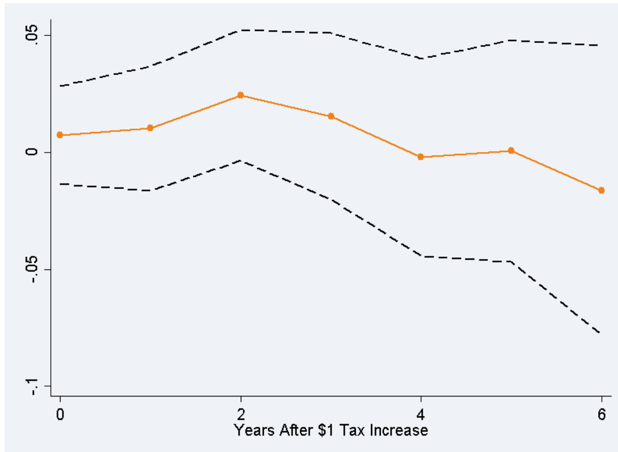


Figure VI: Dynamic Impact of $1 Increase in Taxes on Contributions

Note: Figure shows the effect of a one-dollar increase in taxes on contributions in subsequent years. Estimates are using the parameters given in Panel A of Table VI.

1.6.2 Interpretation

The estimate in Table V clearly indicates that government revenues have at most a small impact on contributions. However, this comes from an increase in government revenues per student, approximately $350, which dwarfs the average level of contributions per student, approximately $32. Since donors cannot reduce their contributions below zero, is there a limit to the level of crowd-out that I could have found?

A naive estimate suggests that the maximum amount of possible crowd-out would be 9.1 cents per dollar. The largest amount of crowd-out in the 95% confidence interval for my estimate, 1.42 cents, is small relative to a dollar of government revenue, but not small compared to a 9.1-cent maximum.

However, this does not accurately represent the maximum possible level of crowd-out. The first stage of the empirical strategy estimates the difference in tax revenues between districts that passed and those that failed referenda. The 9.1 cents calculation assumes that $350 difference in tax revenue comes entirely as an increase in revenue among those whose referenda passed and that districts whose referenda failed remain at the status quo.

Table VII: Regression discontinuity - instrumental variables estimates of the effect of local tax revenues on private contributions excluding zeros.

	No Zeros	Above Median Contr
Loc. tax rev. PS	0.0050	0.0176
	(0.0130)	(0.0200)
Bandwidth	9.71	5.84

Note: Column 1 excludes districts with no contributions in the year before the referenda and Column 2 also excludes those with below median contributions. Otherwise estimates are the same as in Table V.

In this case, then under total crowd-out, districts that passed referenda would reduce contributions to zero and those whose referenda failed would remain at their status quo, resulting in estimated crowd-out of 9.1 cents.

If instead, districts whose referenda passed remain at their status quo and the $350 difference came entirely as a decrease in revenue among those whose referenda failed, then it would be possible to observe full dollar for dollar. In fact, there would be no limit on the level of crowd-out that it would be possible to estimate.

It is clear in Figure IIa that this setting is in between these two extremes. In the three years prior to a referendum, revenues are trending downwards. Following a referendum, districts in which it failed continue to decrease, while those in which it passed increased their revenues.

I present two other results indicating that the lack of crowd-out is not due simply to low initial levels of contributions. First, if there truly was crowd-out but the relative dearth of contributions prevented detecting it, I would expect there to be greater crowd-out among districts with higher levels of contributions prior to their referendum. There is no evidence this is the case. Limiting estimation to districts with positive, or above the median contributions does not impact the result. Table VII presents fuzzy regression discontinuity results of the effects of local tax revenues on private contributions with these restrictions.

Table VIII: The effect of passing a referendum on having no contributions

	1 yr later	2 yrs later	3 yrs later	4 yrs later
Passed	0.0173	0.0211	0.0189	0.0262
	(0.0179)	(0.0220)	(0.0225)	(0.0220)
Bandwidth	11.78	7.81	7.29	10.59

Note: Regression discontinuity estimates of the effect of passing a referendum on binary outcome of having zero contributions. Each estimate comes from a separate regression that includes only referenda within the bandwith shown of the passage threshold. Each model has one observation per referenda by year relative to that referenda and include all available observations. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

Neither moves the point estimates towards crowd-out in an appreciable way.

Second, passing a referendum has no impact on a binary outcome of having zero contributions. Table VIII shows estimates of this effect one through four years following the referenda, estimated using Equation 1.2. In none of them is the effect significantly different from zero.

1.7 Conclusion

These results suggest that sharp increases in tax funding for schools led to at most very small changes in private contributions to them. I can reject crowd-out greater than 1.42 cents per dollar of increased taxes, and any crowd-in larger than 2.66 cents. In this setting, increased tax funding for schools comes directly from the pocketbooks of local residents. It is surprising then, that they do not cut back on their contributions to the same schools.

These results show that when school districts modify their budgets, private contributions will not change. This is the case with modest changes to school budgets. Dramatic changes to school finances, like those involved with large-scale school finance reforms may produce larger responses. However, in terms of the marginal changes that districts make annually, school districts need not worry that their funding decisions will have a perceptible impact on the donations they receive from parents and other community members.

This result appears to stand in contrast to prior research that finds non-negligible crowd-out. These papers find that a dollar of additional government funding crowds out private spending by 20 cents (Hungerman 2005), 70 cents (Andreoni and Payne 2011b), one dollar (Andreoni and Payne 2011a), or 3 cents (Gruber and Hungerman 2007).

Variations in the settings of these studies that offer potential explanations for the differing results. Andreoni & Payne (2011b; 2011a) show that estimates of crowd-out differ depending on the source of funds. Most, or in some cases all, of the estimated crowd-out comes from foundation and non-profit behavioral responses rather than directly from donors. In fact, while Hungerman (2005) finds that charitable activities were crowded out he finds no evidence that donations were. In this setting, there are fewer institutional players between donors and the public goods. School contributions typically come to schools through parent teacher organizations or local education foundations. These are small, single purpose entities.

Chapter 2

Valuing Public School Facilities with Bond Elections in Ohio

2.1 Introduction

In the 2009-2010 school year, there were 98,817 public schools in the United States. While some schools have new buildings costing hundreds of millions of dollars, others are in dire need of repair. The value of these repairs and of school buildings in general, to students and the community is unclear.

Estimating the value of school facilities or has two obvious difficulties. The first problem is that it is not clear what the appropriate outcome is. A large literature investigates the effect of schools on student outcomes, most commonly test scores. This literature has mixed results (Hanushek 1997, 2003). However, a recent study focusing on a district wide school construction project in which the timing of the construction of individual schools was plausibly exogenous finds that the construction of a new school raised test scores by up to 0.15 standard deviations, a large impact (Neilson and Zimmerman 2014).

However, schools may have value to students, parents, or the greater community beyond their effect on test scores. A high quality school or building could bring civic pride, provide community meeting spaces, improve student health, or simply be a visually attractive neighbor. All of these characteristics may be valued but not appear in measurable student outcomes. Since students do not pay tuition to attend public schools, there is no directly observe the price that they are willing to pay. Typically, in order to attend a public school, students must live in the school district. As a result, housing values represent the price to attend local schools, albeit along with many other characteristics of both houses and the location.

The second problem is that estimates of the effect of school quality suffer from endogeneity problems because local political systems determine public school expenditures. Therefore, school expenditures are correlated with unobserved preferences of the community, which are in turn, determined by the people who choose to live there (Kuminoff, Smith, and Timmins 2013).

In dealing with this endogeneity problem, one route is to find a source of variation in school quality unaccompanied by variation in unobserved characteristics. Black (1999) uses the discrete changes in school quality among neighboring houses caused by school attendance zones to estimate the effect of schools on housing prices. She finds that a 5 percentage increase in average student test scores results in a 2.5 percentage increase in house values. However Bayer, Ferreira, and McMillan (2007), show that various other characteristics change discretely at the attendance zone boundary suggesting that this effect is not solely due to school quality. In fact, controlling for these differences reduces the estimated parameter by approximately 30%. Nonetheless higher average student test scores result in increased property values, suggesting that home-buyers value test scores. However, this does not have direct policy implications. Improving the average test scores of schools is difficult to achieve. In contrast, school facility construction or renovation is something that policy makers can control.

Cellini, Ferreira, and Rothstein (2010) exploit differences in capital expenditures between districts in California that proposed new capital spending but whose voters narrowly rejected it in a public referenda and those districts whose voters narrowly passed it. Using these differences in a regression discontinuity design, they find that an additional dollar of school capital spending results in $1.50 higher house values. Since home-buyers were willing to pay more than the necessary taxes for the resulting school building, they argue that this result suggests that California school districts underinvest in facilities. This result has been widely influential. For instance, the National Clearinghouse for Education Facilities points school districts seeking ammunition in campaigns to improve school buildings towards the article.

It is important to know if this result is idiosyncratic to California or common to other states. California has an unusual political structure for local education finance. The California Supreme Court ruled in *Serrano v. Priest* that the current local property tax funding system for schools was unconstitutional in the 1970's. The court ruling forced property taxes that were previously used for local purposes only to be redistributed to communities around the state. However, in 1978, the voter initiative Proposition 13 amended the state Constitution of California drastically reduce property taxes. It set a maximum property tax of 1% of assessed value and prevented assessed values from rising more than 2% per year except when the property is sold. Proposition 13 was amended in 1984, again by voter initiative, to allow districts to propose bonds for capital expenditures to be voted on by the public. While some other states also implemented so called "tax revolt" reforms, few were as dramatic as California's.

In this paper, I replicate the analysis of CFR in Ohio, another large state that requires voter approval of capital expenditures and find that increases in school expenditures have no discernible effect on housing prices. While CFR's result suggests that districts underinvest in facilities in California, there is no evidence that that is the case in Ohio.

However, while the confidence intervals of most estimates in Ohio do not include the point estimates in CFR, some do, so I cannot conclusively state that Ohio and California differ.

To the extent that there is a discrepancy between Ohio and California, there are many possibilities for why that might be the case. The school funding landscapes differ broadly between the two states. While the level of current expenditures per student in California ($5104) and Ohio ($5147) are similar the money come from different sources. In California, 27% of this money comes from local sources while in Ohio 43% is from local sources[1]. In California the average property tax rate from 2005-09 was 0.55% while Ohio's was 1.3%. The national average of property taxes is 0.97% and tends to be correlated with per capita income so it is unusual for state like California to have such a low tax rate[2]. In California, districts cannot increase their tax levels for current expenditures, in Ohio, they can change both property and income taxes for current expenditures pending the approval of their voters.

In the following section, I describe the method used to estimate the effects of increased expenditures on housing. In Section 3, I describe the data, in Section 4, I present my results and in Section 5, I conclude.

2.2 Conceptual Framework

Since property values are a measure of home-buyers' overall willingness to pay to live in a community, an increase in property values means that the community's overall desirability has improved. Using a model based on Brueckner (1982) and Barrow and Rouse (2004), CFR argue that their result indicates that school districts underinvest in school facilities.

I describe a version of the Brueckner model here and how it relates the total value of property to the level of provision of a public good. In the model there are n identical

1. Source: U.S. Census Bureau, Public Education Finances: 2010
2. Source: National Tax Foundation

households that may differ in endowments, w_i, but are otherwise identical and have utility over private consumption, x_i, housing, h_i, and the public good z_j. I assume that $u(x_i, h_i, z_j)$ is continuously differentiable and its first derivatives are positive.

The cost to provide a level of public good, z, to n residents is $C(z, n)$, where C is continuously differentiable and increasing in z and n. The public good is provided by a government that must balance its budget and fund expenditures with a property tax at rate, τ.

If the total cost to rent a home in a community is given by R, the utility of a household in the community will be:

$$u(w_i - R, h_i, z_j) \tag{2.1}$$

This implicitly defines the bid-rent function for housing $R(h_i, z_j, u_i)$. The bid-rent function is related to the house value, V_i by:

$$V_i = R(h_i, z_j, u_i) - \tau V_i \tag{2.2}$$

and hence,

$$V_i = \frac{R(h_i, z_j, u_i)}{1 + \tau} \tag{2.3}$$

Individual property values in the community may vary due to differences in endowments. The total property value in the community is given by:

$$V = \sum_{i=1}^{n} V_i = \frac{R(h_i, z_j, u_i)}{1 + \tau} \tag{2.4}$$

Since the government taxes all property at a rate τ, the government's budget constraint is $\tau V = C(z, n)$. By plugging this budget constraint into Equation 2.4, we see the relationship between total property values and the level of public good provision:

$$V = \sum_{i=1}^{n} V_i = R(h_i, z_j, u_i) - C(z, n) \tag{2.5}$$

The parameter that CFR and I estimate is how the aggregate level of property values change when the level of public good changes, $\frac{\partial V}{\partial z}$. This parameter is directly related to the optimal provision of the public good. At the optimal level of provision, the Samuelson condition will be satisfied such that:

$$\sum_{i=1}^{n} \frac{\partial R_i(h_i, z_j, u_i)}{\partial z} = \frac{\partial C(z, n)}{\partial z} \tag{2.6}$$

From Equation 2.4 it is clear that this requires that $\frac{\partial V}{\partial z} = 0$. Under the assumptions over u and C, this implies that any positive change in property values resulting from a change in the public good level implies that the prior level was suboptimal. Any negative change would imply the converse.

2.3 Empirical Method

A simple regression of the stock of school facility capital on housing prices is unlikely to recover a causal parameter. The reasons that a district invests in facilities are likely to also direct affect house values. In Ohio, as in California and many other states, voters must approve the issuance of bonds covered by future taxes. Taking on debt is the standard method to fund new local investment. These referenda to authorize bond issuance for capital expenditures provide a natural regression discontinuity design. Bonds that receive just under 50% of the votes fail while those that receive just over pass. I use this boundary to estimate the effect of a bond passage on home prices.

However, this design is complicated by a feature of the school bond environment. School districts can repeatedly propose bonds. As a result, if their original bond proposal fails, a district is much more likely to soon make a new proposal than if their bond passes. Therefore, when comparing districts whose bond passed to those whose failed, many members of the control group may have passed a bond in a subsequent year. CFR devise a novel technique to estimate a dynamic treatment effect in this environment.

Following CFR I estimate the effect of a bond authorization on housing prices using a regression discontinuity method. This is a sharp regression discontinuity design and hence requires the two standard assumptions as in Imbens and Lemieux (2008). First, the conditional outcome functions must be continuous functions of the forcing variable, the vote share, at the discontinuity point. Second, the conditional distribution functions must be continuous at the cutoff. This requires that the distribution of the forcing variable is continuous at the cutoff point which is often described as having "imperfect control" over the forcing variable.

In the case of an election, imperfect control around the cutoff should be established by the randomness in any well-run election. Since the electorate is relatively large and it is not known precisely who will vote, it would be exceedingly difficult for school boards to precisely control whether they end up just below or just above the 50% passage cutoff.

Under these regularity conditions, even if the vote share affects the housing price outcome, by looking at elections close to the cutoff this effect, and those of unobserved variables (as long as they are not also discontinuous at the cutoff) can be made arbitrarily small.

In regression discontinuity designs, both global polynomial designs and local linear regression are commonly used to control for the relationship between the forcing variable and the outcome. CFR use a global polynomial approach and estimate a flexible polynomial in the vote share using all observed bond elections rather than using only those close to the cutoff. I follow that methodology[3].

3. This differs from Chapter 1 where a local approach provides better performance while here method-

$$y_{j,t+\tau} = \kappa + b_{j,t}\theta_{\tau}^{ITT} + P_g(\nu, \gamma_u) + u_j \tag{2.7}$$

Where y_{jt} is the average housing price in district j in period t, b_{jt} is equal to 1 if there was a bond authorized in district j in period t, and P_g is an order g polynomial in ν, the share of votes in favor, with parameters γ. θ_{τ}^{ITT} is then the effect of authorizing a bond τ periods ago on home prices.

Much of the variation in home prices is across districts and does not vay over time. In Equation 2.7, these district specific time invariant characteristics end up in u_j. However, data from prior the the referendum provide information on the levels of these characteristics. Including these data in the model can reduce the residual variation and yield a more precise estimate. Following CFR, I pool all relative years, τ, and constrain θ_{τ}^{ITT}, the referendum's effect, to zero in years preceding the vote. I then include referendum, year, and year relative to the referendum fixed effects. I estimate the equation:

$$y_{i,t+\tau} = b_{i,t}\theta_{\tau}^{ITT} + P_g(\nu, \gamma_u) + \alpha_{\tau} + \xi_t + \lambda_i + u_j \tag{2.8}$$

where $y_{i,t}$ is the average home price in the district where referendum i ocurred in year t, $b_{i,t}$ is an indicator for whether bond referendum i passed in year t. α_{τ}, ξ_t, and λ_i are vectors of relative year, calendar year, and referendum fixed effects respectively.

In Ohio, as in California, districts propose bonds more than once in our sample. If passing a bond in period $t - \tau$ changes the probability of passing an additional bond in any period $[t - \tau + 1, t]$ then θ_{τ}^{ITT} includes some of the effect of the intermediate bond. From the perspective of a district determining how passing a bond affected housing prices, this might be desirable. However for the eventual goal of estimating the willingness to pay for the public good, it is not. Since this is analogous to the effect of assigning treatment

ological consistency with CFR is paramount.

status, CFR refer to this as the intent to treat (ITT) effect and I denote it θ_τ^{ITT}.

Instead, I want to estimate a treatment effect that holds future policies constant. CFR refer to this as the effect of the treatment on the treated (TOT), which can be written:

$$y_{jt} = \sum_{\tau=0}^{\infty} b_{j,t-\tau}\theta_\tau^{TOT} + u_{jt} \tag{2.9}$$

Where θ_τ^{TOT} is the effect of a bond authorization τ periods in the past holding future policies constant. Then current housing prices are equal to the sum of these effects.

While the ITT effect is estimable as in equation 2.7 the TOT effect is more troublesome. However, CFR show the two are related by:

$$\theta_\tau^{ITT} = \frac{dy_{jt}}{db_{j,t-\tau}} = \frac{\partial y_{jt}}{\partial b_{j,t-\tau}} + \sum_{h=1}^{\tau}\left(\frac{\partial y_{jt}}{b_{j,t-\tau+h}}\frac{db_{j,t-\tau+h}}{db_{j,t-\tau}}\right) = \theta_\tau^{TOT} + \sum_{h=1}^{\tau}\theta_{\tau-h}^{TOT}\frac{db_{j,t-\tau+h}}{db_{j,t-\tau}} \tag{2.10}$$

Where the last term is essentially the intent to treat effect of bond passage on future bond passage which is estimable by the same process as the original ITT effect. I denote this term, $\frac{db_{j,t-\tau+h}}{db_{j,t-\tau}}$, π_τ. This allows for obtaining TOT estimates from ITT estimates.

2.4 Data

I obtained bond election results from the Ohio Municipal Advisory Council for the period of 1983-2010. For each bond proposed for school improvements they include the number of votes for and against, the size of the proposed bond, the additional millage rate that would be imposed, and the number of years that rate would be assessed. In some elections, voters vote once to approve two bonds, I treat these as one combined bond. In other districts, there are multiple bond elections within one year, either on one ballot or in separate elections. In these cases, I follow CFR and include only the bond election with the highest vote share in favor. This prevents districts that passed a bond being also included on the other side of the discontinuity. Table I provides summary statistics for bonds in my sample. In general the size of bonds per student is comparable but slightly smaller than that in CFR. I merged these with annual district financial data and enrollments from the National Center of Education Statistic's Common Core of Data.

The Ohio Department of Taxation provided me data on housing transactions what provide information on every "arms-length" real estate transaction for the years 1993 through 2011. For each transaction the data include sale price, use code for the property, and the land area. These data have two major shortcomings. First, they do not contain information on square footage which is strongly associated with house prices. Second, the data do not include information (e.g. address) that would allow direct mapping to the school district the house is in. Each transaction includes only the county and local political jurisdiction in which it took place[4].

Using boundary information on municipalities and school districts from the Census Bureau, I identify the school districts that correspond to each jurisdiction and calculate the land area in each. Since many jurisdictions overlap multiple school districts, I use only transactions in jurisdictions where 90% of their area lies within one school district.[5] This process yields a dataset with a subset of all the transactions occurring in many of the school districts. In total, I can assign 44.2% of non-vacant residential property transactions to a school district using this process[6]. This is far from ideal, however there are two features of the estimation strategy that make this less problematic. First, which part of a school district is included in the housing data is constant across time. Since I use years of data from before and after each referenda, these fixed differences are controlled with fixed effects. If the house price effect is uniform within the district, the results would be unchanged.

4. In Ohio local political jurisdictions include cities, villages, and townships

5. There are few jurisdictions whose largest school district accounts for between 60-90% of the land area so this was a natural cutoff although somewhat arbitrary.

6. Of the largest jurisdictions in Ohio, only one, the city of Columbus is excluded by this rule.

Table I: School Bond Election Summary Statistics

Year	N	Avg amount per stu	Fraction approved	Vote Share Mean	Vote Share SD
1987	40		0.65	52.3%	12.5
1988	33		0.61	52.9%	10.1
1989	32		0.44	44.9%	11.0
1990	44		0.55	52.3%	13.7
1991	54		0.46	50.0%	11.0
1992	44		0.41	47.0%	9.5
1993	42	4683	0.38	47.7%	9.4
1994	55	5370	0.56	51.8%	10.7
1995	40	5346	0.55	50.4%	12.9
1996	52	5787	0.35	47.7%	8.5
1997	72	5227	0.40	48.2%	14.5
1998	75	5560	0.49	49.6%	10.2
1999	79	4940	0.58	52.9%	13.6
2000	81	5581	0.70	53.9%	8.8
2001	49	6879	0.57	52.0%	10.8
2002	58	8874	0.43	49.2%	11.0
2003	55	8254	0.38	46.0%	10.7
2004	44	7599	0.45	49.0%	7.5
2005	23	11545	0.43	48.7%	10.9
2006	28	9045	0.36	46.7%	8.9
2007	24	7432	0.33	46.5%	9.5
2008	27	7873	0.56	49.7%	9.9
2009	18		0.39	46.0%	9.7
2010	19		0.42	47.1%	10.4
Total	1088	6458	0.49	49.8%	11.2

Notes: Dollar amounts in year 2000 dollars. Data obtained from Ohio Municipal Advisory Council, Enrollment data from Ohio ODE

Second, as long as whether school district boundaries line up with jurisdiction boundaries is not discontinuous at the bond passage discontinuity this will not bias the regression discontinuity design.

Nonetheless, this data problem represents the only major departure from the methods used in CFR. However, it is worth noting that the housing data used in that paper were also not idea. In fact, they had a similar problem in that their housing data was averaged at the census block level, which in places straddle multiple school districts. The geography in the data used herein is much coarser, but has the advantage of being at the individual transaction level.

I then construct district by year average sale prices for these districts which yields property value estimates for at least one post election year for 1169 bond elections in 405 unique districts. For 446 bond elections, I have housing data for a full two years prior to and 6 years following the election.

I use school district fiscal information from the National Center for Education Statistics' Common Core of Data Local Education Agency Finance Survey. This survey is available for 1990, 1992, and 1995-2009. These data contain information on the total fall enrollment of districts, as well as fiscal variables including total revenues and sources, capital outlays, and many categories of current expenditures.

Table II provides descriptive statistics on the school districts in Ohio. Columns 1-3 use all available observations for each district, with the number of observations given. Column 1 shows averages of all 613 districts in the state. Columns 2 and 3 show the averages for districts that never had a bond go to election and districts that had at least one bond go to election respectively. 89% of districts proposed at least one bond during the period. Nonetheless, those that did not propose a bond are similar to those that did in enrollment and total revenue. Columns 4 and 5 compare the data in period $t-1$ of districts that proposed and passed a bond in period t to those that failed a bond in period t.

Table II: School District Descriptive Statistics

	All	Never bond	Proposed 1+ bonds	Failed	Passed
Number of districts	614	35	579		
Number of observations	11591	590	11001	921	899
Log Enrollment	7.577	7.287	7.594	7.662	7.740
	(0.828)	(1.369)	(0.781)	(0.664)	(0.833)
Total Revenue PP	8337.5	8961.3	8302.2	7209.6	7189.1
	(3597.0)	(4151.0)	(3560.1)	(3375.8)	(2300.5)
Total Capital Outlay PP	986.2	687.2	1003.2	476.2	588.6
	(1981.0)	(1377.9)	(2008.4)	(860.1)	(1039.6)
Long Term Debt Issued PP	481.3	101.1	502.9	169.6	281.8
	(1880.2)	(612.1)	(1925.0)	(944.9)	(1440.2)

Notes: Columns 1-3 use all available observations. Columns 4-5 use observations from year $t-1$ to show pre bond differences in passers and failers. Enrollment and Fiscal data from NCES CCD

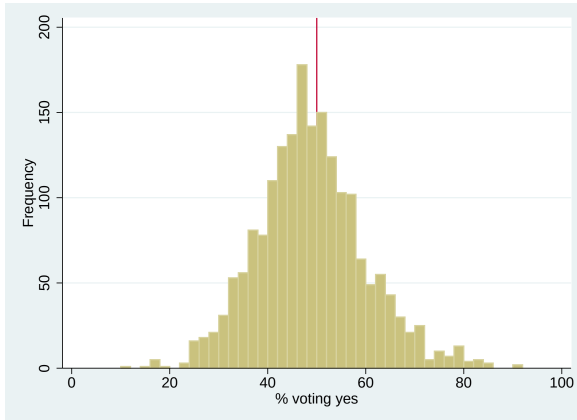
2.4.1 Design Validity

In this section, I present two pieces of evidence that the assumptions necessary to implement the regression discontinuity design with bond elections are likely to hold.

First, discontinuities in the density of the vote share around the cutoff value would suggest that agents are able to manipulate the voting outcomes to cause bond measures to either narrowly pass or fail. This would suggest that it is not random whether measures end up on one side of the threshold or the other. Figure I shows the frequency of vote shares in favor among the bond elections and provides no evidence of heaping on one side of the cutoff suggesting that this is not the case.

Second, there is no evidence that narrowly passing a bond measure is related to characteristics that were fixed prior to the vote. Table III shows the relationship between passing a referendum and fiscal or housing variables one and two years prior to the vote. Column 1 displays the coefficient on measure passage from regressions of the outcome on an indicator for whether the measure passed and year fixed effects. There

Figure I: Distribution of Bond Measure Vote Shares



Notes: Figure showing distribution of share of votes in favor of bond. Includes all bond measures in sample. Data from Ohio Municipal Advisory Council.

are large differences in both enrollment and revenues per pupil between districts where a measure passed and those where a measure failed. Column 2 also includes a cubic polynomial in the vote share. This addition removes all statistical significant differences and dramatically reduces the point estimates on both enrollment and revenues. This suggests that the regression discontinuity design adequately controls for the relationship between voting results and district characteristics at the passage threshold. Columns 3 and 4 show analogous results for one year prior to the referendum with very similar results[7].

2.5 Results

Intent to treat estimates describe the effect of passing a bond in one period on an outcome in that or a later period without accounting for the possible effect that passing a bond has on whether the district would pass a bond in a future period. The following results present that bond passage has no discernible effect on housing prices even though it has

7. Passing a referendum also has no discernible effect on housing prices prior to the vote. These results are displayed in Table I

Table III: Balance of Pre-Bond Measure Characteristics

	Two years before (t-2)		Year before election (t-1)	
	(1)	(2)	(3)	(4)
Enrollment	664**	301	907***	413
	(285)	(523)	(271)	(514)
Capital Outlays PP	68.2	57.8	97.5	76.4
	(53.1)	(98.16)	(57.0)	(107.3)
Total Revenue PP	352***	90.8	402***	-104
	(90.7)	(167)	(91.2)	(170)
Ln House Price	-.006	-.004	-0.013	0.014
	(0.035)	(0.065)	(0.032)	(0.060)
Year Effects	Y	Y	Y	Y
Cubic in vote share	N	Y	N	Y

Note: Each entry comes from a separate regression. Samples in columns 1-2 include observations from two years before the referendum while those in columns 3-4 include observations from the year before the referendum. All models include school year fixed effects. Columns 2 and 4 include a cubic polynomial in the vote share. Results reported are the coefficients on measure passage. **$p<0.01$ * $p<0.05$, + $p<0.10$

the expected effects of increasing the capital outlays of school districts.

2.5.1 Effect on School District Finances

If passing a bond had no impact on capital investment or taxes, there would be no reason for it to result in changes in house prices. Table IV presents estimates of the ITT effect of bond passage on capital outlays, total revenue, and debt issuance all in per student terms. These are estimated as in Equation 2.8. Districts that passed bonds spent more than those that did not in the years immediately following the bond passage. Column 1 shows that passing a bond resulted in districts who passed the bond spent 556 and 1524 dollars more per student on capital outlays than those that failed the bond. In years four through six, I find a negative effect of passing a bond somewhat smaller in magnitude than the positive effect in the earlier years. Since this is the ITT estimate this is likely caused by districts who just failed a bond measure subsequently passing a new bond.

Table IV: Effect of bond passage on district finance variables

	(1) Capital Outlay PP	(2) Total Revenue PP	(3) Debt Issued PP
θ_0^{ITT}	24	178	538+
	(173)	(150)	(310)
θ_1^{ITT}	556**	691*	4196**
	(191)	(271)	(581)
θ_2^{ITT}	1524**	657	-2820**
	(490)	(421)	(524)
θ_3^{ITT}	1258*	-104	-1072**
	(491)	(415)	(324)
θ_4^{ITT}	-788+	-237	-681*
	(461)	(316)	(286)
θ_5^{ITT}	-1079**	-110	-463
	(356)	(282)	(285)
θ_6^{ITT}	-778*	-115	-5
	(308)	(276)	(303)
Observations	7581	7581	7581

Note: θ_τ^{ITT} describes total effect on capital outlays, total revenue, and debt issuance per student τ years after bond passage. Estimated as pooled regression containing all observations from years [-2,6] relative to bond passage, where θ_τ^{ITT} is constrained to zero prior to election. All models contain a cubic polynomial in the vote share interacted with indicators for the year relative to the bond share, bond measure fixed effects, and indicators for the calendar year. Coefficients reported are on an interaction between the relative year indicators and bond passage. Standard errors robust to clustering at school district level.**p<0.01 * p<0.05, + p<0.10

Columns 2 and 3 of Table IV show that districts that pass bonds have higher total revenue and higher debt issuance in the first year following bond passage. Total revenue includes any debt issued by the local government and transferred to the school district which may explain why total revenue is higher following a bond passage. Debt issuance occurs primarily in the first year following bond passage when it increases by 4196 dollars per student. Similar to the district capital outlays, passing a bond has a negative effect on debt issuance in the second through fourth year following the vote. Again this is likely due to the possibility of later bonds.

2.5.2 Housing Price Effects

Column 1 of Table V presents results from intent to treat models where the dependent variable is log mean housing price in the district. These are estimated as in Equation 2.8 using all observations from two years prior to six years following the bond election[8]. All models contain a cubic polynomial in the vote share interacted with indicators for the year relative to the bond share, and bond measure, calendar year, and relative year fixed effects.

None of the estimates are significantly different from zero. In the first, second, fourth, and fifth years following the referendum the effect on housing prices is extremely small or negative. In the third year while the point estimate is somewhat larger it remains statistically indistinguishable from zero. While the treatment on the treated results are the main results that CFR rely on to draw their conclusions, I nonetheless present a comparison. Column 2 contains the analogous results in California as shown in CFR and column 3 presents the difference between the estimates for Ohio and those for California. While these estimates do not allow us to reject that there is no difference between them the results from Ohio are universally smaller than the CFR estimates.

8. Estimates with each year performed separately and including years t-1 and t-2 where there is no effect as I assume are shown in Table I.

Table V: Intent to treat estimates of effect of bond passage on housing prices

	(1) ITT Estimates	(2) CFR Estimates	(3) Difference
θ_0^{ITT}	0.010	0.021	-0.011
	(0.033)	(0.015)	(0.037)
θ_1^{ITT}	-0.024	0.027	-0.051
	(0.032)	(0.017)	(0.036)
θ_2^{ITT}	-0.002	0.036+	-0.038
	(0.035)	(0.020)	(0.041)
θ_3^{ITT}	0.038	0.058**	-0.020
	(0.032)	(0.022)	(0.039)
θ_4^{ITT}	0.001	0.038	-0.037
	(0.030)	(0.024)	(0.039)
θ_5^{ITT}	-0.001	0.038	-0.039
	(0.035)	(0.027)	(0.044)
θ_6^{ITT}	0.017	0.047	-0.030
	(0.037)	(0.035)	(0.051)
Observations	7477	7968	

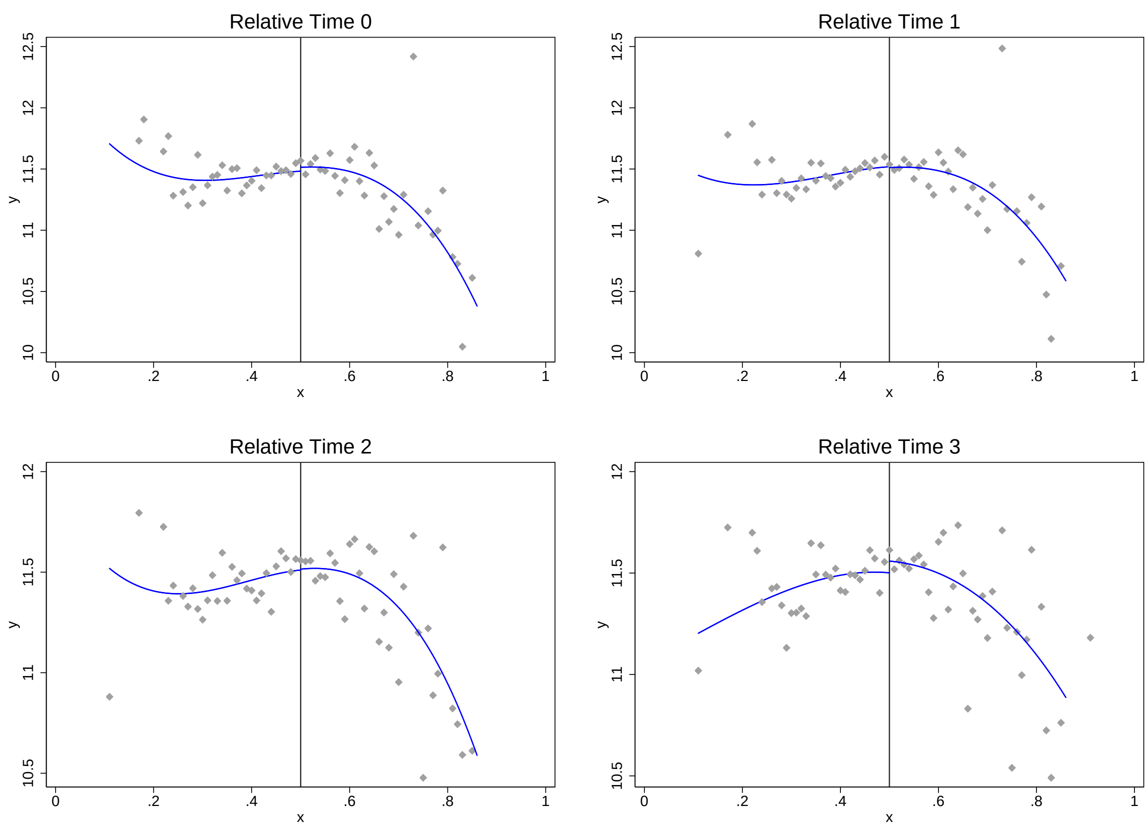
Note: θ_τ^{ITT} describes total effect on housing prices τ years after bond passage. Estimated as pooled regression containing all observations from years [-2,6] relative to bond passage, where θ_τ^{ITT} is constrained to zero prior to election. All models contain a cubic polynomial in the vote share interacted with indicators for the year relative to the bond share, bond measure fixed effects, and indicators for the calendar year. Coefficients reported are on an interaction between the relative year indicators and bond passage. Standard errors robust to clustering at school district level. Model is weighted by the number of housing transactions in sample.**p<0.01 * p<0.05, + p<0.10

Figure II presents the same ITT effects on housing prices visually for the year of the referendum through three years afterwards. Each figure displays the conditional expectation of housing prices in a given year relative to the referendum. The scatter plot presents mean house prices in blocks of 1 percentage point of the vote share with accompanied by a fitted cubic polynomial that allows a discontinuity at the passing threshold. In general, there is a negative relationship between the vote share in favor of a bond proposal and the level of house prices in the district. In the year of the referendum through year two there is clearly no discernible discontinuity at the threshold. Three years after the referendum, the fitted polynomial suggests there is a small discontinuity but it is difficult to detect in the scatter plot itself.

As discussed in Section 2.3, these ITT effects include not only the effect of approving a bond referenda but also the effect of being less likely to approve a bond in the subsequent years. We now present estimates of the effect of the treatment on the treated which represents the effect of approving a bond holding all future bond approvals constant. First, this requires estimating the ITT effect of passing a bond on passing a bond h years later which I denote π_h. As anticipated passing a bond in one period has a negative and striking effect on passing a bond in the following periods. These results are given in Table VI. Passing a bond in one year reduces the probability of passing a bond the next year by 42%. The effect is negative and significant two and three years afterwards as well. However, as these are intent to treat estimates they represent $\frac{db_t}{db_{t-\tau}}$ and do not hold constant bond passage in intermediate years. The TOT analog, $\frac{\partial b_t}{\partial b_{t-\tau}}$ is -.25 for $\tau = 2$ and -.35 for $\tau = 3$.

The π_h parameters could be estimated via Equation 2.7 with the dependent variable replaced with $b_{j,t+\tau}$. However, in order to also estimate the covariance of the π_h parameters with the θ_τ^{ITT} parameters I instead pool them into one estimation estimated as in Equation 2.8 with separate parameters for the p and b outcomes. I then use Equation 2.10 to calculate the TOT effects and the delta method to calculate standard errors. The results are given

Figure II: Conditional Expectation Function of House Prices by Vote Share



Note: Figures show the conditional expectation of house prices by vote share. Line fitted with cubic polynomial and discontinuity at cutoff. Scatter plot show average house prices in one percentage point bins of the share of votes in favor of the bond.

Table VI: Effect of Bond Passage on Future Bond Passage

Parameter	Estimate
π_1	-0.422***
	(0.0419)
π_2	-0.0760**
	(0.0370)
π_3	-0.110***
	(0.0365)
π_4	-0.00101
	(0.0312)
π_5	-0.00747
	(0.0340)
π_6	0.0235
	(0.0375)

Note: π_τ represents the total effect of bond passage in τ years prior on current period bond passage. All parameters are estimated as pooled model as in Equation 2.8 containing all available observations for each referenda where π_τ is constrained to zero prior to election. Each models contain a cubic polynomial in the vote share interacted with indicators for the year relative to the bond share, bond measure fixed effects, and indicators for the calendar year. Coefficients reported are on an interaction between the relative year indicators and bond passage. Standard errors robust to clustering at school district level.**p<0.01 * p<0.05, + p<0.10

Table VII: Treatment on the treated estimates of effect of bond passage on housing prices

	(1) TOT Estimates	(2) CFR Estimates	(3) Difference
θ_0^{TOT}	0.003	0.028+	-0.025
	(0.028)	(0.017)	(0.032)
θ_1^{TOT}	-0.022	0.041+	-0.063
	(0.036)	(0.021)	(0.042)
θ_2^{TOT}	-0.008	0.050*	-0.058
	(0.043)	(0.025)	(0.050)
θ_3^{TOT}	0.037	0.077*	-0.040
	(0.046)	(0.030)	(0.055)
θ_4^{TOT}	0.013	0.075*	-0.062
	(0.047)	(0.035)	(0.059)
θ_5^{TOT}	0.000	0.086*	-0.086
	(0.054)	(0.041)	(0.068)
θ_6^{TOT}	0.007	0.101*	-0.094
	(0.062)	(0.050)	(0.080)
Observations	41294		

Note: θ_τ^{TOT} describes bond passage's effect on housing prices τ years after bond passage holding constant future bond passage. Effect of bond passage on housing prices holding constant future bond passage. Estimated as described in Equation 2.9 using ITT estimates using all available observations for each bond measure. Otherwise ITT estimates use the same method as those shown in Table V. Standard errors calculated with the delta method. **p<0.01 * p<0.05, + p<0.10

in Column 1 of Table VII. These estimates of the TOT effect are not significantly different from 0 in any year. One and two years after the election, passing a bond actually reduces house values.

In comparison, CFR's estimates, shown in Column 2, were significantly greater than zero for the TOT effect in years one through six ranging in magnitude from 0.041-0.101. The estimates from Ohio lesser than those from California. However, the California estimates lie within the confidence intervals of these estimates and I cannot reject the hypothesis that they are equal. However, it is certain that the analysis in Ohio does not support the conclusion that investments in school facilities increase property values.

2.6 Conclusion

The aim of this paper is to document the empirical effect of school infrastructure bond passage on housing prices in Ohio. I have estimated this using the procedure of Cellini, Ferreira, and Rothstein (2010). Unlike their results in California, mine show that increases in school expenditures have small effects on housing prices that are statistically indistinguishable from zero.

While these estimates are uniformly smaller than those in CFR, I am unable to reject effects the size of CFR in the main treatment on the treated specifications that estimate the effect of passing a bond holding all else equal. This parameter can be interpreted by school districts as the effect on housing prices were they to approve a bond rather than not. So while this is not conclusive evidence of difference between the two states it is suggestive that the results from California may not generalize to other states.

California's system of education finance is unique in many ways. Local districts are constrained regarding their taxation and expenditures in ways that they are not in most other states including Ohio. It could be that these constraints are related to a positive effect on house prices of capital expenditures on schools.

Drawing stronger conclusions would require more precise results. Despite having a larger sample of bond elections, these standard error estimates remain larger than those in CFR. Refining these estimates with housing transaction data that include additional housing characteristics to use as controls and improved matching to school districts may help reduce standard errors by reducing the noise in house prices.

These results do suggest that there are differences in the education system or housing market that drive these bond referendums to have differential effects between the two states. Existing literature does not speak to what features of these systems might be important in determining this effect and provides a possible avenue for future research.

Chapter 3

Optimal Fiscal Limits with Overrides

3.1 Introduction

Despite being democratically elected, most state and local governments in the US cannot simply set fiscal policies as they see fit. Rather, tax and spending limits are a common feature of the state and local government fiscal landscape. At the state level, Rose (2010) and Waisanen (2010) report that thirty states operate under a tax or spending limitation.[1] In some states these limits are constitutional and in others they are statutory. They have been implemented both by state legislative bodies and directly by citizens through the initiative process. At the local level, Mullins (2010) reports that all but three states impose some form of constitutional or statutory statewide limitation on the fiscal behavior of their local governments.[2] Moreover, self-imposed limits are quite prevalent at the local level as

1. This is a conservative estimate, since Mullins (2010) reports that thirty five states have limitations. Eighteen states have revenue limits, twenty seven have spending limits, and nine have provisions limiting both revenue and spending.

2. The three exceptions are Connecticut, New Hampshire, and Vermont. The most common type of state-imposed limit on local governments is a property tax rate limitation (forty two states). Tax levy limits are also common (thirty states). Nine states limit spending growth in their local governments and two limit revenue increases. States also regulate what their local governments can tax. See Mullin (2010) for more details.

recently documented by Brooks, Halberstam, and Phillips (2016)[3].

Limits come in many forms and apply to a variety of different fiscal variables. With respect to taxes, there are limits on tax *rates*. In particular, limits on property tax rates are very common at the local level. There are also limits on the total amount of tax that can be raised, so called *tax levy* limits. These can apply to revenue raised from a specific tax or to total tax revenue. With respect to spending, there are limits on the total amount of spending that the government can do. Typically, limits have override provisions which specify when limits may be violated. Violation requires either direct approval of a majority of citizens in a referendum, or a super-majority vote of the governing legislative body.

In light of their practical significance, it is interesting to consider what principles might guide citizens in the setting of fiscal limits. This requires studying the optimal design of limits. To analyze the problem, it is first necessary to frame it. This necessitates taking a stand on why citizens might need limits and specifying the constraints they face in setting them. Regarding the former, it seems natural to assume that elected politicians tend to have a bias towards larger government[4]. Regarding the latter, it is clear that, at the time of setting a limit, citizens face uncertainty in what their preferred fiscal policies are going to be. This uncertainty is what motivates citizens limiting politicians' fiscal authority rather than simply specifying the levels of taxation and spending that they want.

With the problem framed in this way, it is natural to look to the literature on the *delegation problem* for guidance on optimal fiscal limits.[5] This literature considers the

3. These researchers find that one in eight of the municipalities in their large-scale survey have self-imposed limits (as distinct from state-imposed limits).

4. This assumption seems consistent with the history of local tax limitations. Most states adopted their current property tax caps in the late 1970's and early 1980's during an anti-tax movement following the passage of Proposition 13 in California (Mullins, 2010). The literature examining the causes of this 'revolt' commonly credits a perception among voters that taxes were too high, despite being set by democratically elected governments (Citrin, 1979; Ladd and Wilson, 1982). Why elected politicians might be biased towards larger government will be discussed further in Section 4.2.

5. The literature on the delegation problem is a sub-branch of the literature on contract theory. It includes Alonso and Matouschek (2008), Amador and Bagwell (2013), Amador, Werning, and Angeletos (2006), Holmstrom (1977), (1984), and Melumad and Shibano (1991). The theory has many interesting applications,

interaction between a principal and an agent. The agent has to choose a one-dimensional policy that impacts both the principal and agent's payoffs. The optimal policy for both principal and agent depends on the realization of a state of nature, which, prior to the policy choice, is only observed by the agent. The agent's payoff differs from that of the principal, being biased towards a higher or lower level of the policy. The key assumption of the literature is that policy-contingent transfers between the principal and agent are not possible. Rather, the nature of the interaction is assumed to be that the principal chooses a set of permissible policies for the agent and, given his information on the state of nature, the agent chooses his preferred policy from this set. Thus, the choice is delegated to the agent, but the principal places limits on the agent's discretion. The analytical question of interest is what is the optimal set of permissible policies from the principal's perspective? The literature finds reasonable conditions under which it is optimal for the set of permissible policies to be an interval and explores how this interval depends on features of the underlying environment such as the extent of the agent's bias and the degree of uncertainty in the state of nature.

To apply these results to the optimal design of fiscal limits, the principal is interpreted to be a representative citizen and the agent to be a politician.[6] The policy is the level of spending or taxation and the politician is assumed to be biased in favor of spending or taxes. The state of nature represents things that impact the citizen's and politician's preferred level of spending or taxation that are uncertain at the time at which the limit is set. Under the conditions which imply the optimal set of permissible policies is an interval, the upper bound of this interval will be the optimal fiscal limit. The determinants of the

including several in the field of political economy. One such application is to the delegation of policy-making from elected politicians to bureaucrats (see, for example, Epstein and O'Halloran 1994 and Huber and Shipan 2006). Another is to the delegation of policy-making from legislatures to standing committees (see, for example, Gilligan and Krehbiel 1987, 1989, and Krishna and Morgan 2001).

6. The application to "optimal fiscal constitutions" is noted explicitly by Amador, Werning, and Angeletos (2006).

optimal limit then follow from the determinants of the optimal interval.

There is, however, a discrepancy between this way of formalizing the problem and the way in which fiscal limits are structured in reality: namely, the presence of overrides. The formalization does not allow the politician to choose outside the permissible set of policies with the representative citizen's approval.[7] It is therefore natural to wonder how this impacts the applicability of the results of the delegation literature to the optimal design of fiscal limits. Of course, one possibility is that, while overrides are in principle available, in practice they are hardly ever used, and therefore it is reasonable to simply assume them away. Below we present evidence from three US states on local government property tax limits that this is far from the case. For example, in 2014, 18% of Wisconsin school districts, 4% of Massachusetts municipalities, and 11% of Ohio local governments overrode their tax limits. Given this, it seems worthwhile to introduce overrides into the analysis and explore how their presence impacts the optimal design of fiscal limits. This is precisely the purpose of this paper.

To introduce overrides into the analysis, the paper assumes that after the state of nature has been revealed, the politician can propose a taxation or spending level that violates the limit. If the representative citizen votes for the politician's proposal, it is implemented. If he votes against, the proposal is not implemented and the politician is required to select an alternative policy that respects the limit. At the time of voting, the citizen is assumed to know the state of nature, meaning that he knows his optimal policy.[8] However, since the

7. In the delegation problem setting, Mylovanov (2008) shows that the principal can implement an optimal outcome through *veto-based delegation* with an appropriately chosen default policy. In this implementation, the agent proposes a policy and then the principal approves it or not. If he does not approve it, the default policy is implemented. However, this differs from an override which requires that the politician obtains the citizen's approval only if he exceeds the limit. Moreover, in Mylovanov's scheme the principal is not fully informed when he is voting on the agent's proposal. Rather he makes inferences about what must be true from the agent's proposal.

8. This may appear similar to the set-up of Epstein and O'Halloran (1994). In their well-known model, a politician must decide how much discretion to provide to a bureaucrat. The bureaucrat makes a policy proposal within an interval of permissible proposals set by the politician. After the bureaucrat has made his proposal, the politician may veto it. If he does so, some default policy is implemented. As in our model, at

politician has agenda-setting power, this does not imply that the citizen gets to enjoy this policy.

The paper solves for the optimal limit under this assumption concerning overrides. It explores how the optimal limit depends upon the extent of politician bias and the nature of the uncertainty concerning the voter's preferred level of taxation. It also explains how the optimal limit with overrides compares to that without.

When the politician's bias exceeds a threshold, the optimal limit equals the *expected* preferred level of taxation. Below this threshold, the limit exceeds this level. For some distributions of the voter's preferred level of taxation, the optimal limit is more permissive the lower is the politician's bias and, as this bias goes to zero, converges to the maximum level of taxation the voter could desire. Surprisingly, however, for other distributions, the limit becomes more stringent as the politician's bias decreases. Examples suggest that greater uncertainty in the voter's preferred tax level results in a more permissive limit.

The optimal limit with overrides does differ from that without overrides, but only in special cases. The key observation is that if, under the optimal limit with overrides, the politician uses his agenda setting power when making override proposals to leave the citizen with the same utility as he would get with taxes at the limit, then the optimal limit without overrides is no different from that with. Under these circumstances, while equilibrium will involve overrides, the citizen's utility will be the same as if there were no overrides. As a consequence, the optimal limit must be the same with or without overrides. When making override proposals, the politician will leave the citizen indifferent between the proposal and the limit if his bias is large enough. For smaller levels of bias, however, the citizen may strictly prefer the politician's proposal to the limit. When this happens, the

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the time of the veto decision, the politician is fully informed. However, this model differs from ours in that i) the bureaucrat's proposal is always subject to the politician's veto, and ii) the bureaucrat cannot choose from outside the interval of permissible policies. In our model, the voter (who corresponds to the politician in their model) only votes if the politician (who corresponds to the bureaucrat) proposes something outside the permissible set.

optimal limit with overrides differs than that without. Indeed, it can be shown to be more stringent than that without. Coincidentally, it is precisely in these circumstances that the optimal limit with overrides becomes tighter as the politician's bias decreases.

The organization of the remainder of the paper is as follows. Section 2 discusses related literature. To motivate the analysis, Section 3 presents evidence of the empirical importance of the override provision from the US states. Section 4 outlines the model and characterizes the optimal limit. Section 5 compares the optimal limit with and without overrides. Section 6 presents three examples. Section 7 concludes with a brief summary of the findings and suggestions for further research on the optimal design of fiscal limits.

3.2 Related literature

There is a large literature on fiscal limits.[9] This literature can be divided into four branches. The first branch documents the types of limits faced by state and local governments in the US and describes when and how they were introduced (see, for example, Mullins 2010 and Waisanen 2010). This is a difficult and time consuming task because there is a great deal of variation across states and localities and a considerable amount of change over time. The second branch is devoted to understanding how limits impact the fiscal variables they seek to regulate and other related public policies.[10] This is challenging because of the problem of identifying the effect of limits. The third branch studies what citizens think about existing limits and why they were introduced (see, for example, Citrin 1979, Courant, Gramlich and Rubinfeld 1985, Cutler, Elmendorf, and Zeckhauser 1999, and Ladd and Wilson 1982). The fourth branch addresses the normative question of whether

9. Selective reviews are provided by Krol (2007), Mullins and Wallin (2004), and Rose (2010).

10. Papers in this branch of the literature include Bradbury, Mayer, and Case (2001), Brooks, Halberstam, and Phillips (2013), Dye and McGuire (1997), Figlio and Rueben (2001), Knight (2000), Poterba (1994), and Poterba and Rueben (1995, 2001). After surveying the literature, Rose (2010) concludes "tax and expenditure limits appear to be modestly effective in slowing the growth of government, particularly at the municipal level, although the evidence is somewhat mixed".

limits enhance citizens' welfare and, if so, what should be limited and how should limits be designed.

This paper fits in with this fourth, normative branch of the literature. Other papers in this branch are Brennan and Buchanan (1979), Besley and Smart (2007), and Brooks, Halberstam, and Phillips (2016). Brennan and Buchanan (1979) provide a wide ranging normative discussion of tax limits. They study the issue in the context of a model in which a Leviathan government wastes a fixed fraction of any revenues raised for public good provision. This Leviathan government would like to maximize revenues raised. Brennan and Buchanan discuss a number of different limits: tax rate limits, tax levy limits, and tax base limits[11]. They consider tax levy limits and argue that assessing the appropriate limit will be too complicated for average citizens. Our analysis seeks to provide guidance on exactly this type of question. They also question whether such limits can be effective in restraining government, arguing that the footprint of government in the economy does not equate to the tax revenue it raises. In particular, they point out that government can intervene with non-tax methods such as mandates and regulations. This very reasonable concern is abstracted from in this paper.

Besley and Smart (2007) study the operation of a tax revenue limit in the context of a two period political agency model. The politician holding office in each period chooses taxes and provides a public good, the cost of which is uncertain. Politicians can be good or bad. Good politicians maximize voters' welfare in an unstrategic way. Bad politicians are strategic and get utility from holding office and diverting tax revenues to their own consumption. The important point that Besley and Smart make is that a revenue limit in the first period not only limits the choices of the incumbent politician but also impacts how much voters learn about the incumbent. In particular, a revenue limit might induce

11. Base limits correspond to restrictions on what the government may tax. For example, local governments in many states are not allowed to tax income.

a pooling equilibrium between good and bad politicians in the first period, which leads to worse selection in the second period. This impact must be taken into account in a full welfare analysis of limits. This interesting point is abstracted from in our analysis which assumes that the politician's bias is independent of the fiscal limit.

As a prelude to their empirical work, Brooks, Halberstam, and Phillips (2016) provide a theoretical analysis of optimal limits that is in the spirit of this paper. Their framework for understanding limits builds on a model of local government elections presented in Coate and Knight (2011). There are two groups of citizens with low and high preferences for public goods. The level of public good is chosen by an elected politician. Politicians are citizens and choose their preferred public good level if elected. However, citizens cannot observe candidates' preferences. A limit is implemented when the majority have low preferences and is intended to constrain high spending politicians. The cost of the public good is uncertain which makes the choice of limit non-trivial. The optimal limit is shown to be more permissive the higher the probability the elected politician is a low type. However, the analysis assumes there is no override. While it does not explicitly incorporate elections, our model is consistent with that of Brooks, Halberstam, and Phillips. Nonetheless, our analysis of optimal limits differs from theirs because we incorporate the reality that limits can be overridden. This changes the calculus of the optimal limit.

More generally, the paper contributes to a broader normative literature on fiscal constitutions. A fiscal constitution is a set of rules and procedures that govern the determination of fiscal policies (see, for example, Brennan and Buchanan 1980). It is distinct from a political constitution which sets up the architecture of government and the rules by which policy-makers are selected. The fiscal constitution literature seeks to understand the effectiveness of various rules and procedures in generating good fiscal policies for citizens. In addition to tax and spending limits, it studies balanced budget rules, budgetary procedures, debt limits, and rainy day funds. Rose (2010) provides a useful review of this literature. Recent theoretical contributions include Azzimonti, Battaglini, and Coate (2016) and Halac and Yared (2014) who study rules regarding government deficits in dynamic economies.

Finally, the model studied here is related to the well-known agenda setter model of Romer-Rosenthal (Romer and Rosenthal 1978 and 1979). The agenda setter model considers the interaction between a politician and a representative voter. The voter's utility depends on the level of public spending and the politician is responsible for choosing the level of this spending. The politician is not only biased in favor of spending in the sense that he always prefers a higher level than the voter, he is in fact a budget maximizer. The politician's proposed spending level must be approved by the voter and, if it is not, then an exogenous reversion level is implemented. In equilibrium, the politician proposes a spending level which leaves the voter indifferent between the proposal and the reversion level. The proposed spending level exceeds the reversion level whenever the latter falls below the voter's preferred spending level. In this paper, the choice of the limit can be thought of as endogenizing the reversion level. Moreover, the fact that the limit must be chosen before the voter's preferences are fully known makes the choice of limit interesting even in the case in which the politician is a budget maximizer and thus heavily biased.

3.3 The override provision in practice

The purpose of this section is to illustrate the significance of overrides in practice. We focus on local property tax limits, the most prevalent type of fiscal limit in the US. While differing in the details, forty two of the fifty US states have a limit on local property taxes (Lincoln Land Institute). Thirty five of these states allow local governments to override some or all of their limitations with the approval of a majority of their voters.[12] To provide

12. Sources: Lincoln Land Institute, Mullins (2010), and Mullins (2003). Of the seven states with limits that do not allow voter overrides, two (Indiana, South Carolina) allow overrides of the limit with supermajority

a feel for how these overrides are used in practice, we have assembled evidence from three states: Massachusetts, Ohio, and Wisconsin. We selected these states because they have data available detailing the tax limits in particular jurisdictions, taxes levied, and overrides of the limits.[13] As we will see, in all three states, overrides are common occurrences.

3.3.1 Massachusetts

In Massachusetts, Proposition $2\frac{1}{2}$ limits the total amount of property tax local municipalities can levy, which is the predominant source of local tax revenue.[14] Starting in 1983, each government's tax limit was set at their tax level in 1982, and increases by 2.5% plus an adjustment for increases in property values due to new construction, annually[15].

To exceed its limit, the governing body of the municipality must propose the amount of additional revenue it seeks and its purpose and allow the electorate to vote. If the override receives a majority of votes, the town or city can tax the additional amount. If it does not, the government must stay within the limit. Since the limit grows annually, a successful override increases the limit in all subsequent years.

Cities and towns have used the override measure to significantly increase their taxing authority. Between 1983 and 2016, 258 of the 351 municipalities (73.5%) passed at least one override to exceed their limit.[16] The overrides approved by fiscal year 2016 allowed additional taxes such that the total levy limit in 2016 is $943 million higher, representing

votes of the governing body or with an appeal to the state government and the remaining five (Alaska, Arkansas, Delaware, Utah, and Wyoming) provide no means for local governments to exceed the limit.

13. It is certainly possible that the availability of data is related to the widespread use of overrides in these states, making them unrepresentative. We doubt this is the case, but have no way to rule it out.

14. In Massachusetts, municipalities provide nearly all local services and collect over 95% of their tax revenue from the property tax (Tax Policy Center, 2016).

15. The same proposition also limited local governments tax revenues to 2.5% of the total assessed fair market value in the municipality. According to Bradbury & Ladd (1982), the Massachusetts Department of Revenue estimated that this limit would bind for approximately half of the state's municipalities in the first year. However, following the marked increase in property values in subsequent years, very few towns and cities faced it as a binding constraint.

16. Property taxes for capital projects and to repay debt also require voter approval but are not covered under the levy limit. These referenda are not included in these numbers although they are also widely used.

6.2% of the total 2016 levy limit.[17] Limited to only those cities and towns that passed at least one override, the taxes approved via override represent 12.5% of their total 2016 levy limit.

3.3.2 Ohio

Since 1911, Ohio has limited the ability of local governments to set property tax rates, and allowed voters to override those limits to approve higher taxes. At least in recent years, voters have approved dramatically higher taxes than what would be allowed within the limit.

In Ohio, multiple kinds of jurisdictions, including school districts, counties, municipalities, and special districts, have the authority to levy property taxes[18]. Collectively, all coincident taxing jurisdictions are limited to a property tax rate of 1% of the assessed property value.[19] In order to exceed it, the governing body must propose the amount of tax revenue they wish to collect and ask their voters to approve it in a public referendum. A majority of votes is necessary for approval. Override proposals must specify how long they would last and can be permanent or expire after a defined number of years.

Since overrides often last multiple years, the maximum that a government can levy in a given year without an additional override is the sum of their revenue from the 1% levy and that from any outstanding overrides. Their limit may decrease in years following the

17. The total 2016 value of overrides in a given jurisdiction is calculated by $\sum_{t=1983}^{2016} override_t 1.025^{2016-t}$, where $override_t$ is the value of the override passed in year t and zero if there was none. This somewhat understates the importance of overrides, since in addition to directly changing the levy limit, an override will also make future additions to the limit due to new construction larger. We lack sufficient data to calculate this for the full timespan, but for overrides passed between 1992 and 2016 this impact was approximately $117 million.

18. Unlike in the other states discussed here, local governments in Ohio can also raise tax revenue with sales and income taxes, and only 65% of local tax revenue comes from the property tax (Tax Policy Center, 2016). The state also imposes limits for these taxes, though they are not discussed here.

19. The assessed property value is mandated to be 35% of the fair market value. The allocation of these revenues are determined by the relative sizes of the jurisdictions' tax revenues in 1929-1933, the last 5 years in which a prior limit was in effect.

expiration of overrides[20].

Using data from the Ohio Department of Taxation on property tax rates and levies we can determine the prevalence and value of overrides of the 1% limit. In 2014, the 3417 local jurisdictions that raise property taxes charged a total of approximately 15.5 billion in property taxes. Of this, 13.3 billion (85.9%) came from taxes that required voter approval to exceed the levy limit[21].

While overrides vary in their duration, it is not the case that levy limits were driven high enough in previous decades that voters no longer approve overrides. In 2014, voters in 387 jurisdictions approved new or modified tax rates. 44.2% of jurisdictions have outstanding overrides in 2014 that were enacted between 2010 and 2014.

3.3.3 Wisconsin

Wisconsin restricts the ability of local governments to tax their residents, through two separate limits that apply to school districts and to counties and municipalities. The two limits have different structures and while voter overrides occur under both, they are more prevalent among school districts.

3.3.3.1 School districts

Since 1993, Wisconsin has limited the revenues of its school districts to their prior year level of revenue plus a per student increment that is occasionally adjusted by the legislature.[22] The limit covers the combined revenues from both the local property tax levy, which is

20. Prior to 1976, overrides approved a tax rate and the resulting revenue fluctuated with property values, but since then overrides effectively approve an amount of additional revenue.

21. Some taxing jurisdictions received no revenue from the 1% levy and could only levy property taxes with voter approval. Limited to the 2,948 jurisdictions that could levy taxes without an override, 2,435 (82.5%) of them also taxed beyond that amount with the approval of their electorate. These jurisdictions represent the overwhelming majority of property taxes. In total they levied 14.3 billion of which 12.2 billion required an override.

22. While in every other year the increment has been positive, in the 2011-12 school year, it instead reduced each district's revenue limit by 5.5%.

the source of 94.2% of local tax revenue, and state aid.[23] To exceed the limit, a district must propose an amount and whether it is a permanent increase or only for a defined number of years and its voters must approve[24]. Again, a majority of votes is necessary for approval. We use data on levy limits from the Wisconsin Department of Public Instruction to determine the frequency and effect of overrides.

The limit on property tax revenue that a district faces in a given year is the difference between their revenue limit and the amount of state educational aid they will receive. The limit binds frequently and each year most districts levy up to their limit. On average, 75% of districts are at their revenue limit and the median school district levied up to their limit in 17 of the 21 years from 1996 to 2016.

It is common for voters to approve overrides of the limit. Between 1996 and 2016, 59.9% of districts passed at least one referendum.[25] The combination of permanent and temporary overrides in effect in 2016 resulted in 231 million in higher property taxes, or 5.3% of the total levy limit. Limited to only those districts that passed at least one override, this represents 9.3% of their levy limits. Of this, 138 million was due to past and current permanent overrides and the remainder temporary.

3.3.3.2 Counties & municipalities

In contrast to their prevalent use among school districts, counties and municipalities in Wisconsin have made less frequent use of overrides. Beginning in 2006, state law has limited Wisconsin counties and municipalities in the amount they can increase their tax

23. There are also other exceptions that allow districts to exceed their limits without voter approval but for very limited purposes, for instance if they annex a neighboring district, transfer students in an open enrollment program, invest in energy efficiency measures, or lose federal aid.

24. While time-limited overrides approve a dollar amount of spending each year they are in effect, due to the way limits are calculated in subsequent years, permanent increases effectively approve a per student increase.

25. Districts also may have passed referenda between 1993 and 1995, but the Wisconsin Department of Public Instruction no longer has information on these.

levy from year to year. Governments are allowed to increase their levy by the greater of a minimum set by the legislature or the percent increase in the jurisdiction's property values due to new construction.[26] From 2006 through 2010 the minimum was between 2 and 3% but since 2011 it has been 0%. The statewide rate of new construction has also diminished from when the cap was first implemented, suggesting that for many governments the cap is tighter in recent years.

To exceed their cap, governments must ask the voters to pass a referendum. Between tax years 2009 and 2015, 297 of the 1852 municipalities (16.0%) exceeded their caps.[27] However, these were almost exclusively towns that have small populations and are allowed to vote to approve the override at the annual town meeting.[28] Only two of the 190 cities passed a referendum. Counties have similarly been infrequent users of referendums; in the same span only three of the 72 counties passed at least one referendum.

3.4 Optimal fiscal limits

3.4.1 The model

A politician is in charge of selecting a level of taxation for a community. A representative voter has to pay the taxes but benefits from the public spending they finance. The voter desires a certain level of taxation, but this preferred level is ex ante uncertain. The politician prefers a higher level of taxation than the voter. The voter is aware of the politician's bias

26. From 2009-2010 and 2013 to present governments levy limit was calculated as if they had levied at their limit the prior year.

27. Counties and municipalities may have also passed referenda between 2006 and 2008 however the Wisconsin Department of Revenue no longer has data covering these years. Most jurisdictions had more room within their caps during these years than subsequent years, making it unlikely that there were a larger number of referenda approved in years not covered by the data.

28. Towns with populations under 3,000 can approve exceeding the cap without holding a referendum in which votes are cast at polling places. Instead, the vote is held at an annual town meeting as part of a tradition of direct democracy. The differing forms of governance may relate to the relative frequency of exceeding the cap.

and, before he knows his preferred level of taxation, imposes a tax limit on the politician. The limit comes with an override provision that allows the politician to violate it with the voter's approval.

The level of taxation is denoted t. The voter's preferred level of taxation is τ. The voter has distance policy preferences $-|t-\tau|$ so that his utility declines linearly and symmetrically as the level of taxation diverges in either direction from his ideal.[29] The voter's preferred level of taxation τ (hereafter *preferred level*) is the realization of a random variable with range $[\underline{\tau}, \overline{\tau}]$ and cumulative distribution function $H(\tau)$. The associated density function, $h(\tau)$, is assumed to be symmetric around the mean $\tau_m = (\underline{\tau} + \overline{\tau})/2$.[30] In addition, the density is continuous and non-decreasing on $[\underline{\tau}, \tau_m]$. These assumptions imply that the cumulative distribution function is convex on the interval $[\underline{\tau}, \tau_m]$ and concave on the interval $[\tau_m, \overline{\tau}]$. The politician has preferences $-|t-(1+b)\tau|$ so that the parameter b measures the magnitude of the politician's bias.

The tax limit is denoted ℓ. The limit prevents the politician from implementing a level of taxes in excess of ℓ without the voter's approval. Without loss of generality, the limit ℓ is assumed to belong to the interval $[\underline{\tau}, \overline{\tau}]$.

The timing of the interaction between the voter and the politician is as follows. First, knowing H and b, the voter selects a limit ℓ from the interval $[\underline{\tau}, \overline{\tau}]$. Second, nature selects the voter's preferred level τ which is observed by both players. Third, the politician proposes a level of taxation τ. If the proposal does not exceed the tax limit ℓ, it is implemented. Fourth, if the proposed level of taxation violates the limit, an election is held and the voter votes in favor or against the proposal. If he votes in favor, the proposal is implemented. Fifth, if the voter votes against the proposal, the politician chooses another level of taxation

29. While not without their drawbacks (see, for example, Milyo 2000), distance preferences are very widely used in the political economy literature. They are both analytically tractable and simple to understand. The particular form used here assumes a linear loss of utility for the voter as spending diverges from his ideal. This is distinct from a quadratic loss which is also commonly assumed.

30. This means that for any τ below the mean τ_m, $h(\tau)$ is equal to $h(2\tau_m - \tau)$.

t' which respects the limit and this is implemented.

3.4.2 Discussion

The model raises a number of obvious questions which it is useful to briefly discuss. One question is why is the politician biased? In particular, why cannot voters elect a candidate who shares their tax preferences? Of course, to the extent that elections select in the right candidates, then limits would seem unnecessary. Thus, the prevalence of limits at the state and local government level suggests that candidate elections cannot be working perfectly in these environments.[31] To explain this imperfect functioning, it is common to point out that elections at this level of government are small scale affairs and that, as a consequence, voters are not well informed about candidates' policy preferences. Moreover, the rewards to holding office are not large enough to provide incentives for elected candidates to diverge from their preferences to increase their chances of re-election. But all this only means that, when elected, candidates will likely follow their policy preferences and that these preferences may not be congruent with those of the median voter. It does not explain a particular direction of bias. For this, there are (at least) two possible explanations. First, it may be that interest groups operate to put pressure on elected leaders to increase spending above the level preferred by voters. Many stakeholders stand to benefit from public spending. These include public employees, public contractors, and recipients of government grants. By the usual logic of concentrated benefits versus diffuse costs, these stakeholders may form groups to influence politicians. In such environments, politicians may act as if they prefer higher spending even if, as citizens, they share the general voter's preferences (see, for example, Grossman and Helpman 1994 and Besley and Coate 2001). The second explanation is selection. For certain local government offices, it is reasonable

31. This said, it is possible to generate an explanation for the limits states impose on their local governments while assuming that the median voter theorem applies at both the state and local level. See Calabrese and Epple (2011) and Vigdor (2004).

to believe that the people most likely to run are those who care intensely about the policies the office controls and thereby have higher preferences for spending on these policies. Good examples might be school board or town and city council.

A related question is how does the voter know the politician's exact bias? Would it not be more realistic to just assume the voter was uncertain of the degree of the politician's bias, allowing in effect a continuous distribution of bias? The answer to this question is obviously yes. A known level of bias is assumed purely for reasons of tractability.[32] The optimal limit design problem is quite complicated with a known level of politician bias and it makes sense to understand this problem prior to introducing a more general type of uncertainty.

Another question is why is the voter uncertain about his preferred level of taxation? If there were no such uncertainty, the voter should just impose a limit equal to his preferred tax level and that would be the end of the analysis. In particular, there would be no role for overrides. The ubiquity of override provisions suggests that in the real world there must be uncertainty. Moreover, such uncertainty seems intuitively plausible. The type of uncertainty will depend on the nature of the policies the politician is raising taxes to fund. If the policy is road maintenance (snow plowing, pothole repair, etc), then uncertainty would be created by weather, the prices of inputs like road salt, tarmac, etc. If the policy is police protection then uncertainty would be created by the underlying forces generating crime. If the policy is school spending then uncertainty would be created by the prices of school supplies, mandates from higher levels of government, and state and federal financial support.

A final question is why the voter cannot simply implement his preferred level of taxation once uncertainty is resolved? This reflects the assumption that the politician has agenda setting power; that is, the politician has the right to choose the tax proposal

32. In the literature on the delegation problem, it is standard to assume a known level of bias.

and citizens only have the right to veto it if it exceeds the limit. The model makes this assumption because it is an accurate description of reality but does not explain why this arrangement exists.[33] It might be interesting to consider alternative arrangements whereby voters could propose alternatives to the politician's proposal. Intuitively, the underlying reasons why we do not observe such arrangements likely include the problems that i) if citizens could also propose alternatives, it is not clear how to choose between all the alternatives that might be proposed, and ii) the forces that cause elected politicians to be biased might also be expected to result in biased citizen proposals.

3.4.3 The limit design problem

We are now ready to consider the problem of choosing a tax limit to maximize the voter's expected welfare. To pose the limit design problem formally, we must understand the policy implications of any given limit ℓ. Working backwards, consider what policy the politician would choose if he had to satisfy the limit. He will choose a tax level equal to the maximal permitted level ℓ if this is smaller than his optimal level $(1+b)\tau$. Otherwise, he will choose his optimal level. Thus his policy choice will be $\min\{\ell, (1+b)\tau\}$. The voter will recognize that if he votes down any alternative policy proposed by the politician, the tax level implemented will be $\min\{\ell, (1+b)\tau\}$. If the voter's preferred level τ is less than ℓ he will prefer this policy to any higher level and there is no point in the politician proposing to violate the limit. In this case, therefore, the implemented tax level will be $\min\{\ell, (1+b)\tau\}$. If τ exceeds ℓ, the voter will support taxation in excess of the limit. The

33. More generally, the analysis does not seek to explain why the institution defined by a fiscal limit and an override process is optimal. It simply focuses on the narrow, but practically relevant, question of what determines the optimal limit given the institution.

optimal policy proposal for the politician solves the standard agenda-setter's problem

$$
\begin{aligned}
&\max_t -|t-(1+b)\tau| \\
&s.t. -|t-\tau| \geq -|\ell-\tau| \,.
\end{aligned}
\tag{3.1}
$$

The constraint guarantees that the voter supports the proposal since the politician will choose tax level ℓ if his proposal is rejected. From the constraint, the maximum tax level the voter will support is $(2\tau-\ell)$. The politician will propose this if it is smaller than his preferred level $(1+b)\tau$. Otherwise, he will choose his preferred level. In summary, therefore, the policy implemented under limit ℓ will be $\min\{\ell,(1+b)\tau\}$ if the voter's preferred level τ is less than ℓ and $\min\{(2\tau-\ell),(1+b)\tau\}$ otherwise. Putting all this together, it follows that with limit ℓ, the voter's expected welfare will be given by

$$
-\left(\int_{\underline{\tau}}^{\ell}\left[\min\{\ell,(1+b)\tau\}-\tau\right]h(\tau)d\tau+\int_{\ell}^{\overline{\tau}}\left[\min\{2\tau-\ell,(1+b)\tau\}-\tau\right]h(\tau)d\tau\right). \tag{3.2}
$$

The limit design problem is to choose a tax limit from the interval $[\underline{\tau},\overline{\tau}]$ to maximize this function. Since the constraint set is compact and the objective function continuous, the problem has a solution.

Rearranging, the limit design problem can be restated as choosing a limit to maximize the objective function:

$$
V(\ell)=\int_{\max\{\underline{\tau},\frac{\ell}{1+b}\}}^{\ell}\left[(1+b)\tau-\ell\right]h(\tau)d\tau+\int_{\ell}^{\min_{+}\{\overline{\tau},\frac{\ell}{1-b}\}}\left[\ell-(1-b)\tau\right]h(\tau)d\tau-\int_{\underline{\tau}}^{\overline{\tau}}b\tau h(\tau)d\tau, \tag{3.3}
$$

where $\min_{+}\{\overline{\tau},\ell/(1-b)\}$ denotes the smallest positive number of the two.[34] The third term in this expression measures the voter's loss of welfare if there was no limit and the

34. If b exceeds 1, $\ell/(1-b)$ will be negative and hence $\min\{\overline{\tau},l/(1-b)\}$ equals $\ell/(1-b)$ while $\min_{+}\{\overline{\tau},\ell/(1-b)\}$ equals $\overline{\tau}$.

politician were to just choose his preferred level $(1+b)\tau$. The first two terms represent the surplus the voter can claw back through the limit. The limit design problem is then to find the limit ℓ that maximizes these first two terms.

3.4.4 The optimal limit with large politician bias

When the politician's bias is large, the limit design problem is straightforward to solve. In particular, suppose that it were the case that b exceeded $(\overline{\tau} - \underline{\tau})/\underline{\tau}$. Then $\ell/(1+b)$ would be less than $\underline{\tau}$ for any limit ℓ in the range $[\underline{\tau}, \overline{\tau}]$. Moreover, if it were positive, $\ell/(1-b)$ would exceed $\overline{\tau}$ for any limit ℓ in the range $[\underline{\tau}, \overline{\tau}]$. Accordingly, from (3.3), the objective function $V(\ell)$ would be

$$V(\ell) = \int_{\underline{\tau}}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau + \int_{\ell}^{\overline{\tau}} [\ell - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau. \tag{3.4}$$

Differentiating this expression, we obtain

$$V'(\ell) = 1 - 2H(\ell). \tag{3.5}$$

Recall that H is assumed to have a density that is symmetric around the mean and hence $H(\tau_m)$ is equal to $1/2$. Thus, the voter's welfare is increasing in the limit as long as it is less than τ_m and decreasing thereafter. The optimal limit is therefore equal to τ_m - the *expected* level of taxation the voter would like.

The intuition here is equally transparent. When the politician's bias exceeds $(\overline{\tau} - \underline{\tau})/\underline{\tau}$, then, even if the voter's preferred level was at its lowest possible level ($\tau = \underline{\tau}$), the politician's preferred level will still exceed $\overline{\tau}$. As a result, whatever the limit, he will always choose a tax level equal to the maximum allowable level under the limit (ℓ) when the voter's preferred level is less than the limit ($\tau < \ell$). Moreover, when the voter's preferred level exceeds the limit ($\tau > \ell$), the politician will choose a tax level that provides the voter with exactly the same payoff as he would get from the maximum allowable level under the limit (ℓ). As a result, the voter's payoff is exactly that which would arise if the policy were just set equal to the maximum level permitted by the limit (ℓ). The optimal limit is therefore the level which, if committed to ex ante, would yield the voter the highest expected payoff. This is the expected preferred tax, τ_m.

In fact, we can weaken the requirement on bias and still keep the conclusion that the limit should equal the expected preferred tax level. As the following proposition shows, it is sufficient that the degree of bias b exceeds $(\overline{\tau} - \tau_m) / \underline{\tau}$.

Proposition 1 *If the politician's bias b exceeds $(\overline{\tau} - \tau_m) / \underline{\tau}$, the optimal limit is τ_m.*

With a level of bias between $(\overline{\tau} - \tau_m) / \underline{\tau}$ and $(\overline{\tau} - \underline{\tau}) / \underline{\tau}$, it remains the case that the voter's payoff from limit τ_m with a biased politician is exactly that which would arise if the tax level were just set equal to τ_m ex ante. However, for a limit ℓ in excess of τ_m, it could be the case that the politician would choose his preferred policy $(1 + b)\tau$ rather than ℓ for sufficiently small τ.[35] If so, the payoff from such a limit would strictly exceed that associated with just choosing tax level ℓ ex ante. Similarly, for a limit ℓ less than τ_m, it could be the case that the politician would choose $(1 + b)\tau$ rather than $(2\tau - \ell)$ for sufficiently large τ. It therefore becomes less obvious that the optimal limit is τ_m because the payoff from alternative limits may have improved. However, the proof of the Proposition shows that, under the conditions on the density function h, τ_m remains optimal.

A graphical interpretation of the result is provided in Figure I. In each panel, the range of values for the voter's preferred tax τ is measured on the horizontal axis. The three upward sloping lines are $(1 + b)\tau$, τ, and $(1 - b)\tau$ respectively. The parameters are chosen so that b is exactly equal to $(\overline{\tau} - \tau_m) / \underline{\tau}$. In Panel A, the shaded area represents the surplus that the voter would lose if the politician were to choose his preferred level $(1 + b)\tau$. This

35. This requires that $(1 + b)\underline{\tau}$ is less than ℓ.

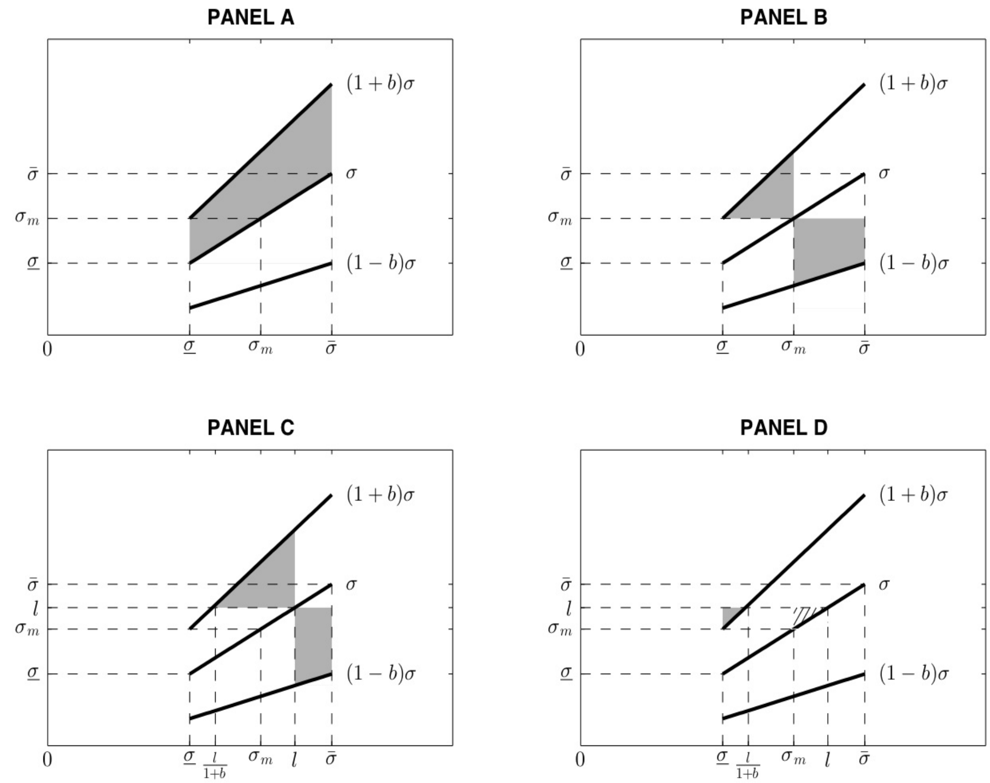


Figure I: Graphical Interpretation of Proposition 1

area is the third term in (3.3). In Panel *B*, the shaded area represents the surplus generated for the voter by the limit τ_m, which corresponds to the first two terms of (3.3). Panel *C* illustrates the surplus generated for the voter by a limit ℓ larger than τ_m. Notice that with this limit, the politician chooses his preferred level $(1+b)\tau$ when τ is sufficiently low. The difference in surplus from limit τ_m as opposed to limit ℓ is illustrated in Panel *D*. In the proof of Proposition 1, this difference is shown to equal the difference between twice the striped area ($\int_{\tau_m}^{\ell} [\ell - \tau]\, h(\tau) d\tau$) less the shaded area ($\int_{\underline{\tau}}^{\frac{\ell}{1+b}} [\ell - (1+b)\tau]\, h(\tau) d\tau$). The assumptions on the distribution function *H* and bias parameter *b* imply that this difference is positive.

3.4.5 The optimal limit with small politician bias

We now turn to the more challenging case in which b is smaller than $(\overline{\tau} - \tau_m)/\underline{\tau}$. We first prove that, under our assumptions on the distribution function H, the optimal limit is never smaller than τ_m.

Lemma 1 *The optimal limit is always at least as big as τ_m.*

We now characterize the solution when the politician's bias is smaller than $(\overline{\tau} - \tau_m)/\underline{\tau}$ but larger than $(\overline{\tau} - \tau_m)/\overline{\tau}$. In this range of bias levels, with a limit equal to τ_m, the politician will choose his preferred tax level $(1+b)\tau$ when τ is sufficiently low but will always choose the tax level $(2\tau - \tau_m)$ when τ exceeds τ_m.

Proposition 2 *If the politician's bias b lies between $(\overline{\tau} - \tau_m)/\overline{\tau}$ and $(\overline{\tau} - \tau_m)/\underline{\tau}$, the optimal limit solves the equation*

$$1 + H\left(\frac{\ell}{1+b}\right) = 2H(\ell). \tag{3.6}$$

It is straightforward to show that there must exist a solution to equation (3.6) on the interval $(\tau_m, \overline{\tau}]$.[36] While there is no guarantee that this solution is unique, it is difficult to come up with examples satisfying our assumptions in which there are multiple solutions. Figure II illustrates a situation in which there exists a unique solution. The Figure depicts the curves $2H(\ell)$ and $1 + H(l/(1+b))$ on the interval $[\tau_m, \overline{\tau}]$. The curve $2H(\ell)$ must be concave since, under our assumptions, H is concave on $[\tau_m, \overline{\tau}]$. The curve $1 + H(l/(1+b))$ is convex on the interval $[\tau_m, (1+b)\tau_m]$ and concave thereafter. This follows from the fact that H is convex on $[\underline{\tau}, \tau_m]$. As illustrated, the end points and shapes of the two curves guarantee they intersect, with $2H(\ell)$ intersecting $1 + H(l/(1+b))$ from below.[37]

In the case covered by Proposition 2, the optimal limit becomes more stringent as the

36. This is shown in the proof of Proposition 2.

37. If there is more than one solution to equation (3.6), the end points and shapes of the two curves imply that there must be three. The optimal limit is either the smallest or the largest intersection point, where $2H(\ell)$ intersects $1 + H(\ell/(1+b))$ from below.

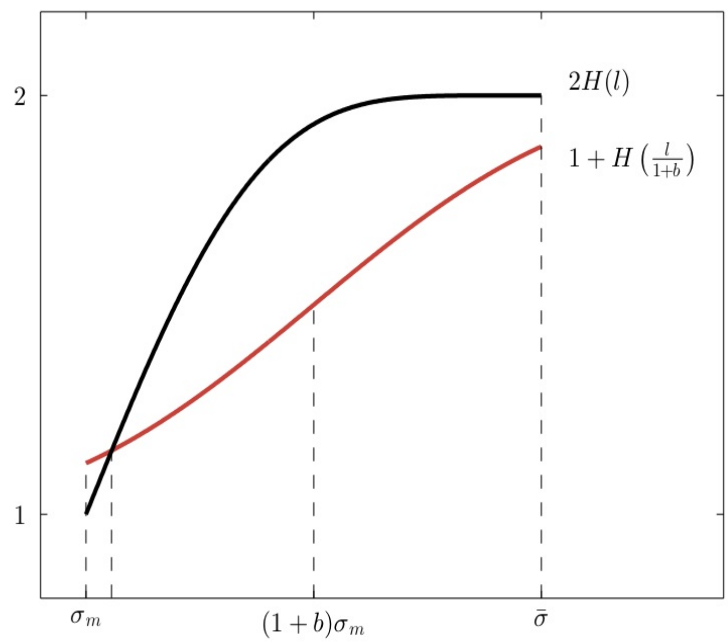


Figure II: Illustration of Proposition 2

politician's bias increases. An increase in b shifts down the curve $1 + H\left(\ell/(1+b)\right)$. If there is a single intersection point, it must shift to the left, implying a lower optimal limit[38].

Finally, we tackle the case in which the politician's bias is smaller than $\left(\overline{\tau} - \tau_m\right)/\overline{\tau}$. In this range of bias levels, with a limit equal to τ_m, the politician will not only choose his preferred tax level $(1+b)\tau$ when τ is sufficiently low but will also do so when τ is sufficiently high.

Proposition 3 *If the politician's bias b is smaller than $\left(\overline{\tau} - \tau_m\right)/\overline{\tau}$, the optimal limit is* ***either*** *bigger than $(1-b)\overline{\tau}$ and solves equation (3.6)* ***or*** *is smaller than $(1-b)\overline{\tau}$ and solves the equation*

$$H\left(\frac{\ell}{1-b}\right) + H\left(\frac{\ell}{1+b}\right) = 2H(\ell). \tag{3.7}$$

38. If there are multiple intersection points, both the smallest and largest shift down. The only way that an increase in b could increase the optimal limit, therefore, is if it caused a shift from the smallest intersection point to the largest. But it is easy to see that an increase in b makes a move from the smallest intersection point to the largest less attractive.

As noted above, there must exist a solution to equation (3.6) on the interval $(\tau_m, \overline{\tau}]$. We show in the proof of Proposition 3, that if this solution is smaller than $(1-b)\overline{\tau}$, then there must be a solution to equation (3.7) which is also smaller than $(1-b)\overline{\tau}$. Again, there is no guarantee that there exists a unique solution, but multiple solutions do not arise in examples. Figure III graphs the three curves $2H(\ell)$, $1 + H(\ell/(1+b))$ and $H(\ell/(1-b)) + H(\ell/(1+b))$. The curve $H(\ell/(1-b)) + H(\ell/(1+b))$ coincides with $1 + H(\ell/(1+b))$ at limits higher than $(1-b)\overline{\tau}$ and lies below it for lower limits. Over this range, it has a steeper slope. The Figure illustrates a situation in which the solution to equation (3.6) is smaller than $(1-b)\overline{\tau}$. The optimal limit is therefore found where the curves $2H(\ell)$ and $H(\ell/(1-b)) + H(\ell/(1+b))$ intersect.

It is important to note that in the case in which the optimal limit is found where the curves $2H(\ell)$ and $H(\ell/(1-b)) + H(\ell/(1+b))$ intersect, it is not necessarily the case that increasing the politician's bias will make the optimal limit more stringent. This is because an increase in b has ambiguous effects on the curve $H(\ell/(1-b)) + H(\ell/(1+b))$. While an increase in b reduces $H(\ell/(1+b))$ for all ℓ, it increases $H(l/(1-b))$. The net effect is ambiguous.[39] Following the discussion of the override provision, section **6** provides examples which illustrate that the stringency of the optimal limit can be both decreasing and increasing in bias.

39. The derivative of the function implicitly defined by the equation $H(\frac{\ell}{1-b}) + H\left(\frac{\ell}{1+b}\right) = 2H(\ell)$ is given by

$$\frac{d\ell}{db} = \frac{h(\frac{\ell}{1-b})\frac{\ell}{(1-b)^2} - h\left(\frac{\ell}{1+b}\right)\frac{\ell}{(1+b)^2}}{2h(\ell) - \frac{h(\frac{\ell}{1-b})}{1-b} - \frac{h\left(\frac{\ell}{1+b}\right)}{1+b}}.$$

Assuming that $2H(\ell)$ intersects $H(l/(1-b)) + H(\ell/(1+b))$ from below, the denominator in this expression will be positive. However, the sign of the numerator is ambiguous.

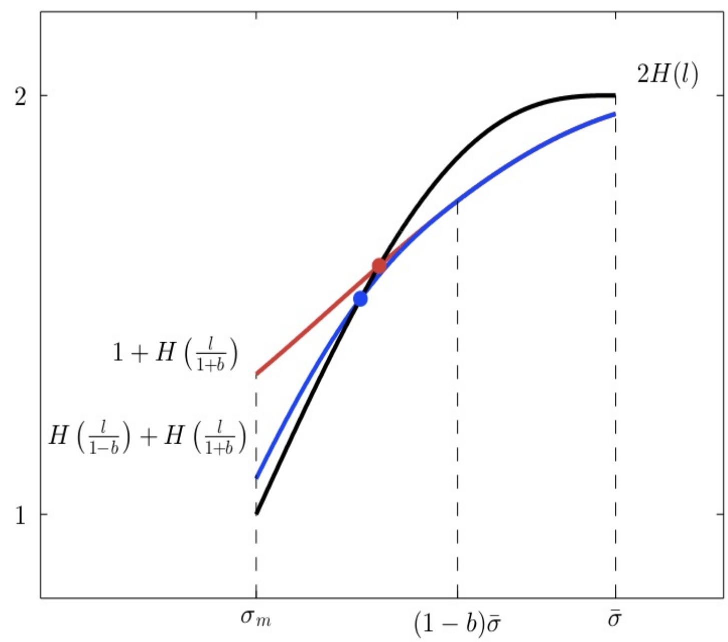


Figure III: Illustration of Proposition 3

3.5 The impact of the override provision

The results of the previous section describe the optimal limit when overrides are possible. To understand how the possibility of overrides impacts the optimal limit, this section characterizes the optimal limit without overrides and compare the results.

With no overrides and limit ℓ, the politician will choose a tax level equal to ℓ if this is smaller than his preferred level $(1+b)\tau$. Otherwise, he will choose his preferred level. Thus his policy choice will be $\min\{\ell, (1+b)\tau\}$. As a result, the voter's expected welfare will be given by

$$-\left(\int_{\underline{\tau}}^{\overline{\tau}} |\min\{\ell, (1+b)\tau\} - \tau| h(\tau) d\tau\right). \tag{3.8}$$

The limit design problem without overrides is then to choose a tax limit from the interval

$[\underline{\tau}, \overline{\tau}]$ to maximize this function.

To make this comparable with our earlier analysis, we can rewrite the problem as choosing a limit to maximize the objective function

$$V_N(\ell) = \int_{\max\{\underline{\tau}, \frac{\ell}{1+b}\}}^{\ell} [(1+b)\tau - \ell] h(\tau) d\tau + \int_{\ell}^{\overline{\tau}} [\ell - (1-b)\tau] h(\tau) d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau) d\tau. \quad (3.9)$$

As in the override case (see, in particular, (3.3), the last term in (3.9) represents the welfare loss if the politician was allowed to choose their preferred policy all the time and the first two terms represent the surplus the voter can claw back through the limit. The limit design problem is then to find the limit that maximizes the first two terms. Let ℓ_N^o denote the optimal limit with no overrides and ℓ^o the optimal limit with overrides.

To understand the relationship between the problems with and without overrides, it is helpful to observe that

$$V_N(\ell) = V(\ell) - \int_{\min_+\{\overline{\tau}, \frac{\ell}{1-b}\}}^{\overline{\tau}} [(1-b)\tau - \ell] h(\tau) d\tau, \quad (3.10)$$

where $V(\ell)$ is the objective function for the case with overrides defined in (3.3). This expression reflects the fact that the only benefit from overrides for the voter comes when the voter's preferred level exceeds the limit and the politician's preferred level $(1+b)\tau$ is smaller than the maximum level the voter will support $2\tau - \ell$. In this scenario, $(1-b)\tau$ is larger than ℓ and hence the second term on the right hand side of (3.10) will be positive. This scenario occurs with positive probability only if $(1+b)\overline{\tau}$ is smaller than $2\overline{\tau} - \ell$. If this condition is not satisfied, while overrides are used in equilibrium, they do not benefit the voter, because the politician uses his agenda setting power to leave the voter with the same utility as he would get from the limit (henceforth, his "limit utility").

Given that the voter's expected welfare with no overrides can be no larger than that with overrides, if $V_N(\ell^o)$ is equal to $V(\ell^o)$, then it must be the case that the optimal limit with no overrides is equal to that with overrides. From (3.10), $V_N(\ell^o)$ will equal $V(\ell^o)$ if $(1+b)\overline{\tau}$ is greater than or equal to $2\overline{\tau} - \ell^o$ or equivalently if ℓ^o is greater than or equal to $(1-b)\overline{\tau}$. In this case, at the optimal limit with overrides, whenever the state τ exceeds ℓ^o and overrides are used, the politician exploits his agenda setting power to leave the voter with his limit utility. Accordingly, the voter obtains the exact same utility with an identical limit and no overrides. We record this important observation as:

Proposition 4 *If the optimal limit with overrides is greater than or equal to $(1-b)\overline{\tau}$, then the optimal limit with no overrides is equal to that with overrides.*

Recall that Lemma 1 tells us that ℓ^o is always at least as big as τ_m. It therefore follows from Proposition 4, that there is no difference between the optimal limit with and without overrides if the politician's bias is greater than or equal to $(\overline{\tau} - \tau_m) / \overline{\tau}$. The optimal limit without overrides is then described by Propositions 1 and 2. The interesting case is when the politician's bias is smaller than $(\overline{\tau} - \tau_m) / \overline{\tau}$. Proposition 3 suggests that the optimal limit with overrides can be smaller than $(1-b)\overline{\tau}$, in which case the optimal limit with no overrides could in principle differ from that with overrides. Our next Proposition shows that in this case the optimal limit with no override must satisfy equation (3.6).

Proposition 5 *With no overrides, if the politician's bias b is greater than $(\overline{\tau} - \tau_m) / \underline{\tau}$, the optimal limit is τ_m. If the politician's bias is less than $(\overline{\tau} - \tau_m) / \underline{\tau}$, the optimal limit solves equation (3.6).*

Thus, the optimal limit with overrides does differ from that with no overrides, but only in the case in which that optimal limit with overrides is smaller than $(1-b)\overline{\tau}$ and solves equation (3.7). In this scenario, the optimal limit with overrides is *more stringent* than that with overrides.

We can draw on the literature on the delegation problem to help us understand all these findings. Consider first Proposition 5. The literature tells us that the key consideration in setting the optimal limit when there is no overrides is what the voter can infer about the state of nature from the fact that the politician is setting taxes equal to the limit . In the large bias case, the politician setting taxes at the limit provides no information about the state, since the politician would do this regardless of the state. Accordingly, the limit is set to maximize the voter's expected welfare with no additional information on the state. In the small bias case, the politician setting taxes at the limit provides the information that $\tau(1+b)$ is greater than ℓ. The optimal limit is set to maximize the voter's expected welfare conditional on this information. Given the assumptions on preferences, this is the median of the posterior distribution. If $\hat{\tau}$ is the median level in this range, it is defined by $2H(\hat{\tau}) = 1 + H(\ell/(1+b))$. Equation (3.6) defines the solution at which $\hat{\tau} = \ell$.

Turning to Propositions 1 and 2, the logic underlying Proposition 5 continues to apply despite the fact that there are overrides because the agenda setting power of the politician leaves the voter with his limit utility. However, the case covered by Proposition 3, is more interesting. In that case, when the optimal limit is smaller than $(1-b)\overline{\tau}$, the politician may not set taxes such that the voter receives his limit utility when an override occurs. Thus, the fact that the politician sets taxes such that the voter receives his limit utility provides more information about the state. Specifically, $\tau(1+b)$ is greater than ℓ but less than $2\tau - \ell$. Again, the optimal limit is at the tax level that, conditional on this information, the voter would prefer, which is the median of the posterior distribution. Equation (3.7) defines the solution where the median is the limit. The same intuition that explains the optimal limit in the case without overrides, therefore explains the optimal limit with overrides, even when they differ.

3.6 Examples

This section presents three examples involving different distributions of the state of nature. These examples are used to illustrate how the optimal limit with overrides differs from that without and how those limits depend on the politician's bias and the extent of uncertainty in the voter's preferred tax level.

3.6.1 Uniform distribution

Suppose that the distribution of the voter's preferred tax level is uniform; i.e., $H(\tau)$ equals $(\tau - \underline{\tau}) / (\overline{\tau} - \underline{\tau})$. Then, when the politician's bias b is greater than 0 but less than $(\tau_m - \underline{\tau}) / \underline{\tau}$, the optimal limit is

$$\ell^o(b) = \ell(b) = \overline{\tau} \left(\frac{1+b}{1+2b} \right). \tag{3.11}$$

To see this, note first that equation (3.7) has no solution in the uniform case unless $b = 0$.[40] It therefore follows from Proposition 2 and 3 that the optimal limit with override must satisfy equation (3.6) and be equal to the optimal override without an override provision. Solving this equation yields (3.11).

Equation (3.11) implies that as b approaches 0, the limit converges to $\overline{\tau}$ and so the politician is completely unconstrained. Without the override, this is natural, because the politician is becoming a perfect agent for the voter and there is little gain from constraining him. With the possibility of overrides, the limit is irrelevant when the politician is a perfect agent of the voter and therefore it is not clear to what point the limit will converge. However, with the voter's preferred tax level uniformly distributed, as long as the politician and the voter are not perfectly aligned, any override that occurs does not improve the

40. For equation (**??**) to be satisfied $H(\ell) - H(\ell/(1+b))$ must equal $H(\ell/(1-b)) - H(\ell)$. In the uniform case, this is not possible since $\ell - \ell/(1+b)$ cannot be equal to $\ell/(1-b) - \ell$, unless $b = 0$.

voter's welfare. As a result, the logic of the case without overrides also applies to that with overrides.

Note also from (3.11) that the limit gets progressively tighter as we increase b, until the point at which b equals $(\tau_m - \underline{\tau}) / \underline{\tau}$ and the limit equals the expected preferred level, τ_m. Further increases in bias have no impact on the limit beyond this point. Figure IV illustrates the optimal limit as a function of b for the case in which $(\underline{\tau}, \overline{\tau})$ equals $(0.1, 0.3)$. The solution is the curve that takes on the value of 0.3 when b is equal to 0 and equals 0.2 for b greater than 0.1.

To understand the impact of changing the distribution of preferred tax levels, it is instructive to consider a parameterization in which $(\underline{\tau}, \overline{\tau})$ equals $(\eta - z, \eta + z)$. As we increase z, we hold constant the voter's expected preferred level but increase the dispersion. We therefore implement a mean preserving spread. Proposition 1 and (3.11) tell us that the optimal limit is

$$\ell(b) = \begin{cases} \frac{(\eta+z)(1+b)}{1+2b} & \text{if } b \in [0, \frac{2z}{2(\eta-z)}] \\ \eta & \text{if } b > \frac{2z}{2(\eta-z)} \end{cases} . \tag{3.12}$$

These limits are illustrated in Figure IV for η equal to 0.2 and various values of z. The main point to take away is that with a mean preserving spread, the limit becomes more permissive when the bias of the politician is not too large.

3.6.2 Tent distribution

Suppose that the distribution of the voter's preferred tax is a tent distribution; i.e.,

$$H(\tau) = \begin{cases} \frac{(\tau-\underline{\tau})^2}{2(\tau_m-\underline{\tau})^2} & \text{if } \tau \in [\underline{\tau}, \tau_m] \\ \frac{1}{2} + \frac{(\tau-\tau_m)[2\overline{\tau}-(\tau+\tau_m)]}{2(\tau_m-\underline{\tau})^2} & \text{if } \tau \in [\tau_m, \overline{\tau}] \end{cases} . \tag{3.13}$$

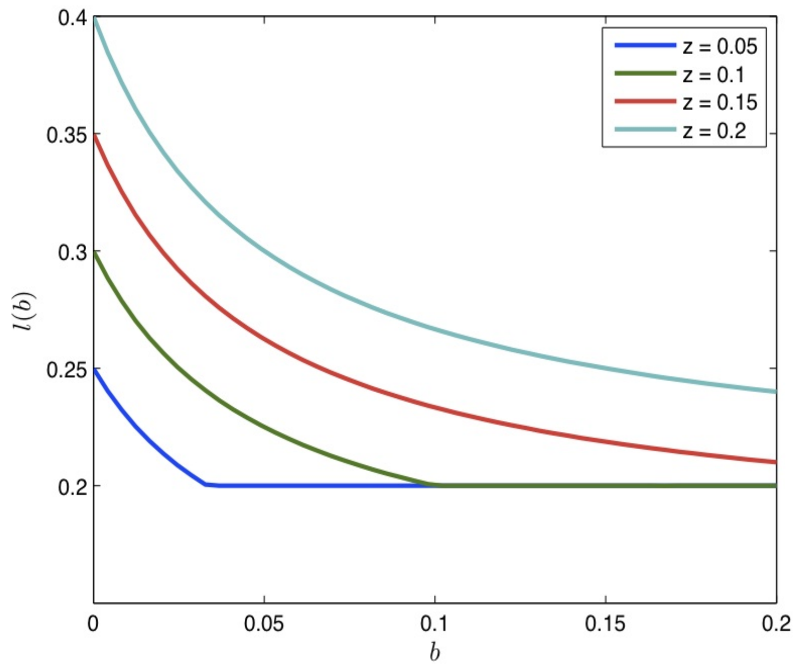


Figure IV: Optimal Limits for the Uniform Distribution

The density associated with this distribution rises linearly from 0 to $1/\left(\tau_m - \underline{\tau}\right)$ over the interval $[\underline{\tau}, \tau_m]$ and comes back down the other side. Despite its simplicity, this case turns out to be very complicated. Thus, to simplify and permit comparison with the uniform case, we set $(\underline{\tau}, \overline{\tau})$ equal to $(0.1, 0.3)$.

In this case, for levels of bias less than 0.3 the optimal limit occurs where the curves $H\left(\ell/(1-b)\right) + H\left(\ell/(1+b)\right)$ and $2H(\ell)$ intersect. For higher levels of bias, the optimal limit occurs where the curves $1 + H\left(\ell/(1+b)\right)$ and $2H(\ell)$ intersect. Solving the appropriate quadratic equations reveals that the optimal limit is

$$\ell(b) = \begin{cases} \dfrac{12+\frac{2}{1+b}-\frac{6}{1-b}-\sqrt{\left(12+\frac{2}{1+b}-\frac{6}{1-b}\right)^2-32\left(2+\frac{1}{(1+b)^2}-\frac{1}{(1-b)^2}\right)}}{20\left(2+\frac{1}{(1+b)^2}-\frac{1}{(1-b)^2}\right)} & \text{if } b \in (0, 0.3) \\ \dfrac{\frac{2}{1+b}+12-\sqrt{8+\frac{48}{1+b}-\frac{64}{(1+b)^2}}}{40+\frac{20}{(1+b)^2}} & \text{if } b \in (0.3, 1) \end{cases} \tag{3.14}$$

Without the override provision, the optimal limit is always determined by the intersection of the $1 + H(\ell/(1+b))$ and $2H(\ell)$ curves. When the level of bias is larger than 0.3 the optimal limit is the same with or without the override provision. However, when the bias is less than 0.3 they diverge. The optimal limit without the override is

$$\ell^o(b) = \begin{cases} \dfrac{\frac{30}{1+b} - 60 + \frac{30\sqrt{2}b}{1+b}}{100\left(\frac{1}{(1+b)^2} - 2\right)} & \text{if } b \in (0, \frac{1}{4}(2-\sqrt{2})) \\ \dfrac{\frac{2}{1+b} + 12 - \sqrt{8 + \frac{48}{1+b} - \frac{64}{(1+b)^2}}}{\frac{20}{(1+b)^2} + 40} & \text{if } b \in (\frac{1}{4}(2-\sqrt{2}), 1) \end{cases} \tag{3.15}$$

These optimal limits are graphed in Figure V. The optimal limit with the override provision is the solid green curve, the higher of the two curves that take on the value 0.2 when b is equal to 0. The optimal limit without overrides is the dotted green curve that diverges from the solid curve when b is equal to 0.3. Without overrides, the limit is qualitatively similar to that under the uniform distribution, the limit converges to $\overline{\tau}$ as b approaches zero and is τ_m when b exceeds $\frac{1}{3}$. However, the possibility of an override dramatically changes the optimal limit at low levels of bias.

With overrides, the limit does not converge to $\overline{\tau}$ as b approaches zero. In fact, the optimal limit does not necessarily become more permissive as the politician becomes less biased. To the contrary, it becomes more stringent over some part of the range! Analytically, this reflects the fact that when equation (3.7) determines the optimal limit, whether it increases or decreases with the bias depends upon the distribution of the preferred tax level.

This dependence is illustrated by observing the results of a mean preserving spread in the voter's preferred level. Figure V illustrates the optimal limits, with and without the override provision, for the cases in which $(\underline{\tau}, \overline{\tau})$ equals $(0.2 - z, 0.2 + z)$ for various values of z. Note first that the lesson from the uniform case remains: a mean preserving spread results in the optimal limit becoming more permissive when the bias of the politician is not too large. Second, note that as we spread out the distribution, the non-monotonicity of the optimal limit with overrides exhibited in the case where $(\underline{\tau}, \overline{\tau})$ equals $(0.1, 0.3)$ disappears. This makes sense intuitively because as we flatten out the tent distribution, it approaches a uniform distribution. Nonetheless, even with a mean preserving spread, the optimal limit with overrides does not converge to $\overline{\tau}$ as b approaches zero.

While the optimal limits with and without the override provision continue to diverge as b decreases, the improvement in the voter's expected welfare afforded by the possibility of overrides does not. Figure VI displays graphs the voter's expected welfare with and without the override provision for the distributions displayed in Figure V. For all distributions, the voter's welfare is decreasing in the politician's bias. At some levels of bias, the override provision affords the voter higher levels of welfare. Figure VII graphs the difference in the voter's expected welfare with and without the override provision. While very low levels of bias is when the optimal limits with and without overrides differ the most, the welfare gain of the override provision is nonetheless small. The welfare gain is largest at moderate levels of bias.

3.6.3 Symmetric Beta distribution

Suppose that $[\underline{\tau}, \overline{\tau}]$ equals $[0, 0.2]$ and that the distribution of the preferred level is a symmetric Beta distribution

$$H(\tau; v) = \frac{\int_0^{\tau} x^{v-1}(0.2 - x)^{v-1} dx}{\int_0^{0.2} x^{v-1}(0.2 - x)^{v-1} dx}, \tag{3.16}$$

for v greater than or equal to 1. Recall that when v equals 1 this distribution is just the uniform distribution and when v equals 2 it is the parabolic distribution. As we continue to increase v, probability mass becomes more and more concentrated around the mean 0.1.

Figure VIII graphs the optimal limit with and without the override provision as a

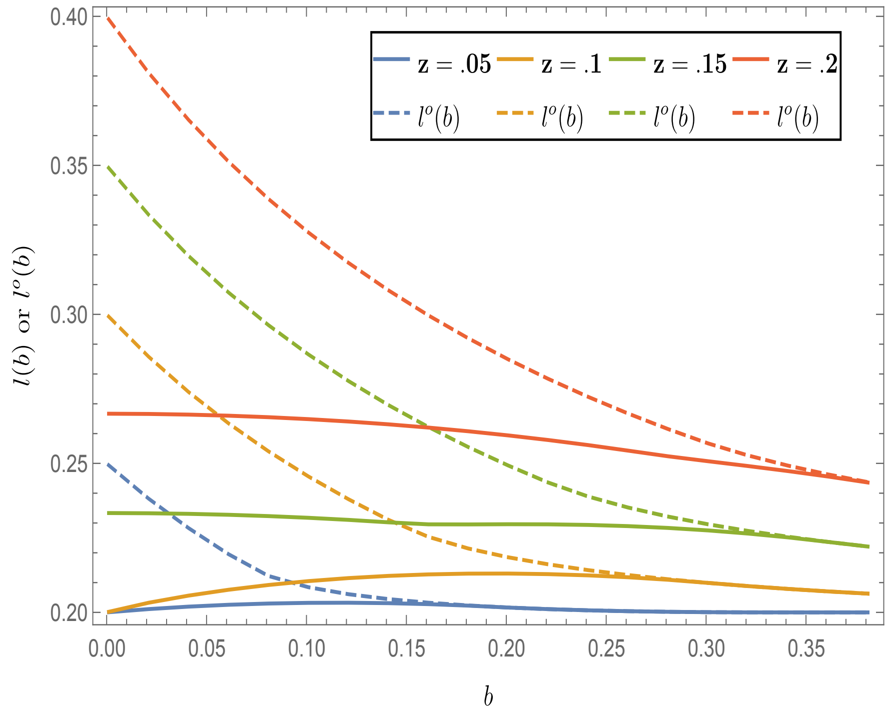


Figure V: Optimal Limits for the Tent Distribution

function of b for various values of v.[41] There are two points to note. First, for given v, the optimal limit is decreasing and approaches the expected preferred tax level τ_m which equals 0.1. It does not quite reach the mean because $\underline{\tau}$ equals 0 and so Proposition 1 never applies. Second, for given b, as v becomes higher, the optimal limit becomes smaller. Thus, less discretion is provided to the politician as uncertainty is reduced. This is consistent with the findings from the two previous examples, because a move from a symmetric Beta distribution with a higher to a lower v amounts to implementing a mean preserving spread.

Figure IX graphs the voter's expected welfare with and without the override provision for values of v, and Figure X graphs the the difference in the voter's expected welfare with and without the override provision. More concentrated (higher v) distributions produce

41. A closed form solution is not available for this case, so the optimal limit is obtained numerically.

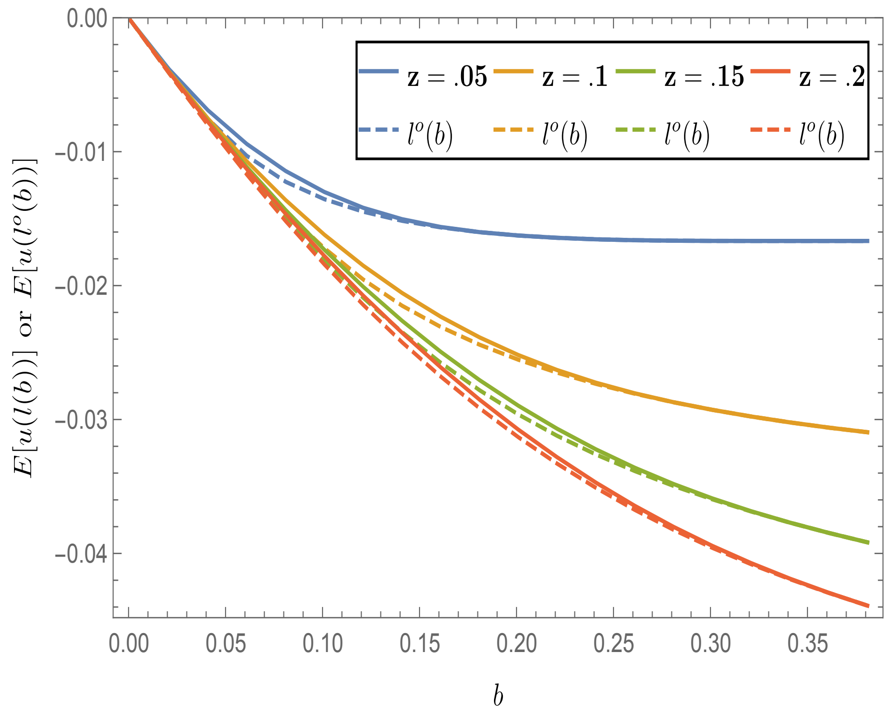


Figure VI: Voter's Expected Welfare for the Tent Distribution

higher expected welfare for the voters, they also produce larger expected welfare gains through the override provision. This is somewhat intuitive since at one extreme is the uniform distribution, under which the override provision offers no benefit to the voter. However, at the other extreme is a degenerate distribution under which the override provision also does not benefit the voter[42].

3.6.4 Discussion

These examples provide a few instructive features. First, as we increase the uncertainty in the voter's preferred level, the optimal limit both with and without the override provision becomes more permissive at least for bias levels that are not too large. Related comparative static findings have been shown in a number of papers in the literature on the delegation

42. Further numerical simulations suggest that the benefit to the voter continues to increase as v increases.

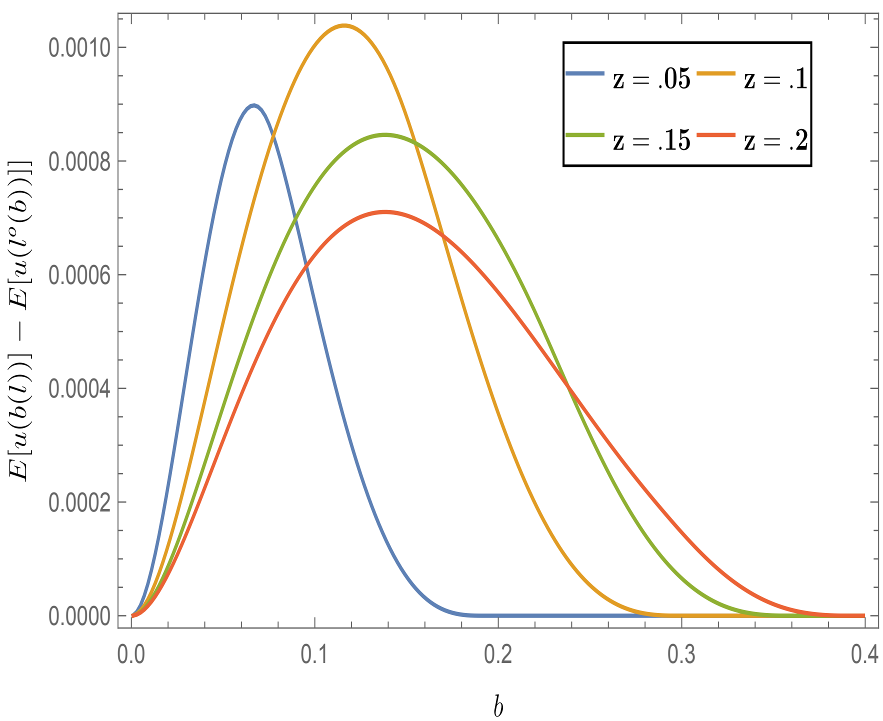


Figure VII: Expected Welfare Benefit to the Voter of the Override Provision for the Tent Distribution

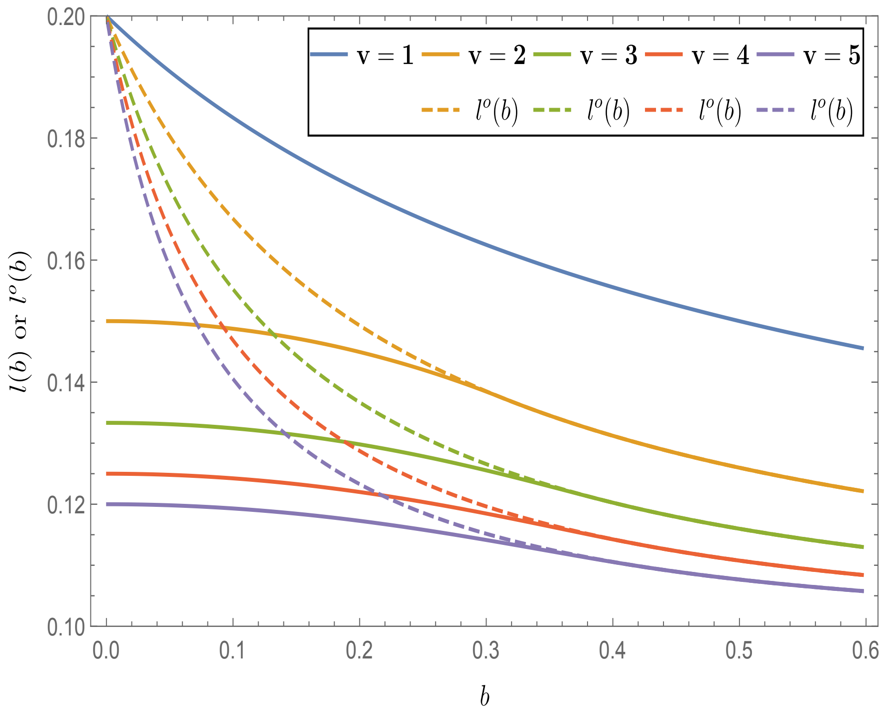


Figure VIII: Optimal Limits for the Beta Distribution

problem. Indeed, Huber and Shipan (2006) refer to the idea that the optimal permissible set of actions for the agent is increased when the principal faces more uncertainty as the *Uncertainty Principle*. It is interesting that this principle continues to hold even though the agent can opt out of the permissible set of actions with the principal's approval. Second, as we reduce the politician's bias it is not necessarily the case that the optimal limit with the override provision becomes more permissive. This contrasts with findings in the literature on the delegation problem which show that the optimal permissible set of actions for the agent is increased when bias is reduced (the so-called *Ally Principle*).

In addition, the examples provide evidence of when voter stands to benefit from the override provision. While the optimal limits with and without the provision differ the most at very low levels of bias, it must be the case that the voter has relatively little to gain from a limit – with or without overrides at these levels of bias. The benefit of the override

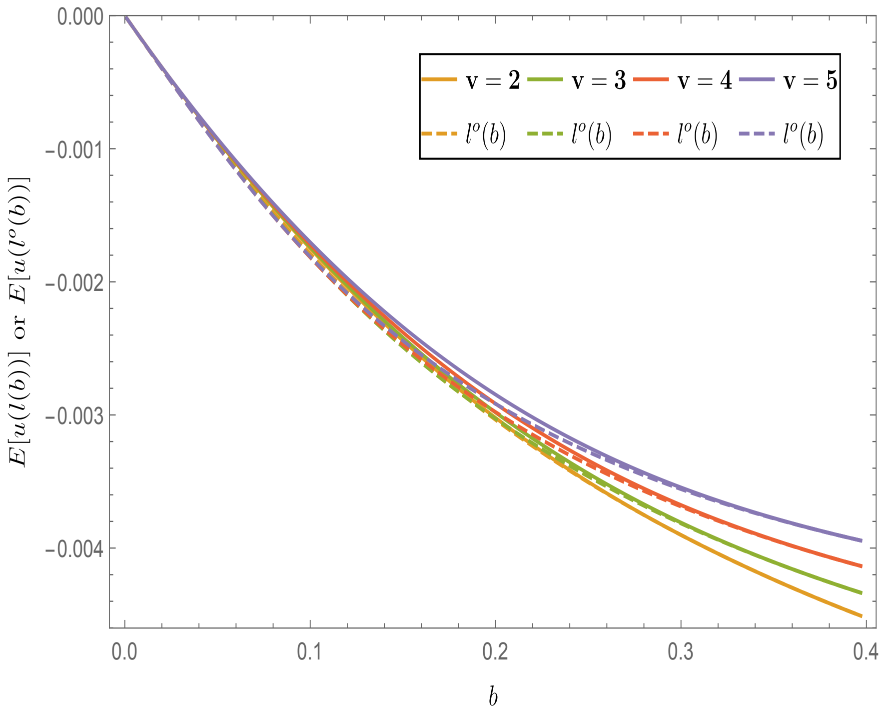


Figure IX: Voter's Expected Welfare for the Beta Distribution

provision in terms of expected voter welfare is largest at moderate levels of bias. Whether this welfare gain increases or decreases as the uncertainty in the voter's preferred level increases depends upon the exact distribution.

3.7 Conclusion

Fiscal limits are commonplace in state and local government in the US. It is therefore important to understand how they should be designed. The contract theory literature on the delegation problem is a natural place to look for guidance. However, this literature ignores a key feature of how limits are structured in reality: namely, the possibility of overrides. As we have shown, overrides are not only possible in principle, but are widely used in practice. Accordingly, this paper has studied the optimal design of fiscal limits in a

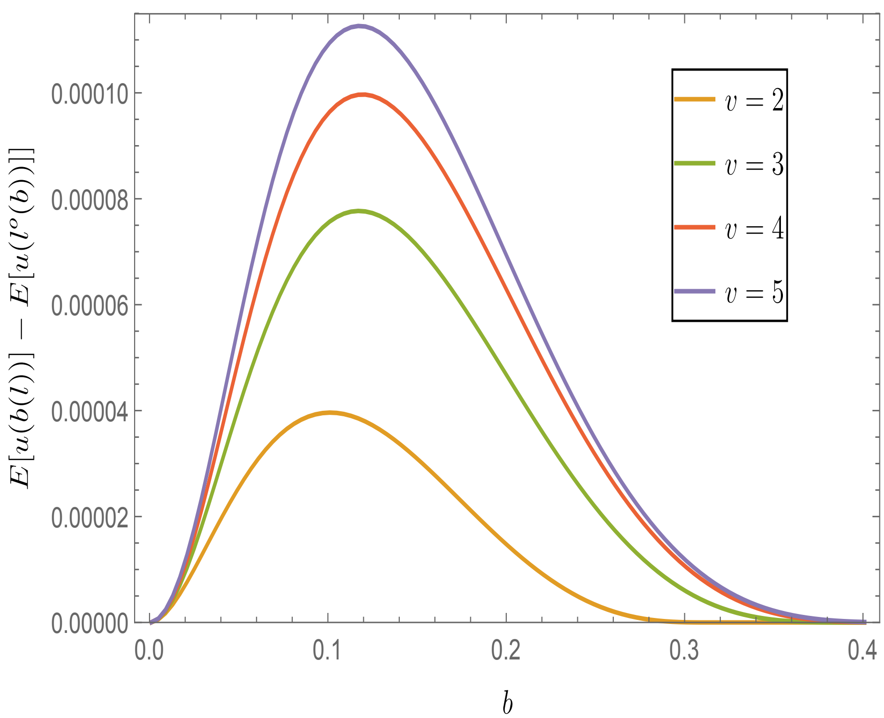


Figure X: Expected Welfare Benefit to the Voter of the Override Provision for the Beta Distribution model that extends the standard framework of the delegation problem to incorporate the possibility of overrides.

The analysis of the model sheds light on how the optimal limit depends on the possibility of overrides, the level of bias towards higher taxes in the political system, and the nature of uncertainty concerning voters' policy preferences. When the level of bias is high, the optimal limit is the same with or without the possibility of overrides. At sufficiently high levels of bias, the limit just equals the level of taxation the representative voter expects to want. For smaller levels of bias, more permissive limits are optimal. Under certain conditions, the optimal limit with overrides does indeed differ from that with no overrides. The voter, not just the politician, must be able to benefit from a potential override. When this is the case, optimal limits with overrides are more stringent than those without. With overrides, it is not necessarily the case that the limit becomes increasingly more permissive as the level of bias falls. This will depend on the distribution of preferred tax levels. Our examples suggest that, for low bias levels, more uncertainty in the voter's preferred tax level results in a more permissive optimal limit.

There is considerable scope for further work on the practically important and theoretically interesting problem of designing fiscal limits. Extensions of the model readily suggest themselves. It would be interesting to go beyond the simple preferences assumed here by, for example, introducing convexity into the voter's loss function. We could then understand how greater convexity influences the permissiveness of the limit. Relaxing the assumptions on the distribution of preferred tax levels may also prove instructive. Richer uncertainty in the degree of politician bias could also be introduced. Finally, recognizing that it is costly to hold an override election might yield interesting results.

In addition, there are interesting questions that a static model like that presented here cannot answer. Most fundamentally, how should the limit evolve from one period to the next? Current caps differ in at least two ways. First, some caps are based on the previous year's revenue while others are based on the previous year's limit. How does this difference affect the behavior of politicians and the optimal limit? Second, some caps allow politicians to propose both temporary and permanent increases in their limit. How does this additional choice effect the voter's welfare?

Beyond these extensions, considering optimal limits in a legislative setting would be interesting. In such a setting, taxation is determined by the collective decisions of legislators rather than the decision of a single politician. Consistent with practice, it would be natural to consider override provisions which allow the limit to be overridden by a super-majority of legislators rather than by direct appeal to the voters. Continually undertaking referenda is likely to prove administratively costly and it may be that the same function can be achieved by appropriate choice of super-majority override.

Of course, even if all these extensions were undertaken, the analysis would only provide insight into what limits should depend on *in principle*. Moving in the direction of being able to say what limits should be in concrete situations would require effort in two directions. First, developing models like the ones presented here into credible models for thinking about limits empirically. This requires developing models whose testable implications are consistent with the data. Second, with such models in hand, trying to measure the key determinants of the optimal limit. For example, how could we measure the extent of politicians' bias and the nature of uncertainty in voters' preferred tax levels?

Appendix A

Appendices to Chapter 1

A.1 Further tests of RD validity

In section 1.5, I present estimates of the discontinuity in pre-referenda fiscal outcomes. Here, I provide additional "placebo test" results using the full panel data estimation strategy discussed in Section 1.3. I alter the date of the referenda to be two or more years prior to its actual date and estimate the first stage model. Table I shows estimates from this analysis. Some fiscal variables show some evidence of a discontinuity at the cutoff of the subsequent referenda.

Were this evidence of a true discontinuity, it might call into question the validity of the election. However, unlike the evidence on the post-referenda outcomes, these effects do not hold up to other specifications and are likely remnants of the relationship between voting and revenues that the linear controls were unable to accommodate. Table II shows results from the same models but with the addition of referenda specific time trends. Unlike in the first stage models, this addition reduces these estimated discontinuities to be statistically indistinguishable from zero.

Table I: Placebo test estimates of passing a referendum on prior outcomes

	expPS	trevPS	loctaxPS	pctlocrev	contrPS
$win_1^{\tau=1}$	436.3	316.4	144.7	0.0	-9.6
	(195.4)*	(127.0)*	(61.4)*	(0.4)	(4.4)*
$win_1^{\tau=2}$	131.9	287.9	162.9	0.2	-4.3
	(217.2)	(121.7)*	(82.5)*	(0.4)	(4.2)
$win_1^{\tau=3}$	-189.9	52.8	90.6	0.4	7.2
	(209.3)	(117.9)	(69.0)	(0.4)	(3.9)
$win_1^{\tau=4}$	-48.9	87.4	3.3	-0.1	4.8
	(207.0)	(111.0)	(63.2)	(0.4)	(4.5)

Note: Regression discontinuity estimates of the effect of passing a referendum as if the referenda had occurred τ years prior to its actual date. Each estimate comes from a separate regression that includes only referenda within 10 percentage points of the passage threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations from the six years prior to the referenda and one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

Table II: Placebo test estimates of passing a referendum on prior outcomes with referenda specific trends

	expPS	trevPS	loctaxPS	pctlocrev	contrPS
$win_1^{\tau=1}$	639.5	205.2	66.2	-0.2	-13.4
	(323.9)*	(180.7)	(76.9)	(0.4)	(7.5)
$win_1^{\tau=2}$	77.4	155.3	135.5	0.2	-3.7
	(335.3)	(186.1)	(94.1)	(0.5)	(6.4)
$win_1^{\tau=3}$	-283.2	-57.6	50.9	0.2	5.0
	(344.7)	(182.0)	(79.7)	(0.5)	(8.3)
$win_1^{\tau=4}$	93.3	142.8	69.8	0.0	0.9
	(345.2)	(160.1)	(81.6)	(0.5)	(10.1)

Note: Regression discontinuity estimates of the effect of passing a referendum as if the referenda had occurred τ years prior to its actual date including referenda specific trends. Each estimate comes from a separate regression that includes only referenda within 10 percentage points of the passage threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations from the six years prior to the referenda and one year following. Models include referenda fixed effects, referenda specific time trends, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

A.2 Effects of state versus local revenues

In the main analysis described above, I instrument for local tax revenues with whether districts have passed a referendum. However, passing a referendum can impact not only local tax revenues, but also revenues from the state. This is because not all districts face a tax price of one for either operational or capital expenditures, as discussed in Section 1.2. If state revenues have their own impact on private contributions, the instrument, passing a referendum, is improperly excluded from the second stage regression. This conflates the impact of state revenues and local tax revenues.

Potential contributors to a local district pay a far smaller share of the taxes that fund state transfers than they do local taxes. As a result, while the classical models of contributions suggest that both state revenue and local revenue would crowd-out private contributions, since contributors pay less of the state revenue themselves a standard model suggests that state revenues would contributions to a lesser extent.

Because some I estimate this model using data from multiple states and multiple types of referenda in which districts each face differing tax prices, my instrument varies in its relative impacts on local versus state aid. By allowing the instrument to vary by state and type of referenda in Equation 1.2, I can use this variation to separately identify their impacts.

As shown in Section 1.5, passing a referendum impacts revenues from both state and local sources. This is because in some states, districts face tax prices that are less than one. In some cases, these differ by whether it is a referendum for operational or capital expenditures. Since this is only true for some states and types of referenda, by allowing the instrument to vary by state and type, I can turn one instrument into seven and instrument for both local and state revenues. Table III and Table IV show the first stage results of passing an election by state and by state and referenda type respectively.

Table III: Effect of passing a referendum on expenditures and revenues by state.

	Yr of ref	1 yr. later	2 yrs later	3 yrs later	4 yrs later
MI Rev. PS	175	448	-129	28	81
	(312)	(256)	(313)	(227)	(250)
MI St. rev PS	236	101	186	8	82
	(249)	(158)	(148)	(159)	(207)
MI Loc. tax PS	-44	208	-241	41	71
	(133)	(143)	(293)	(245)	(277)
MN Rev. PS	133	718	325	420	27
	(110)	(128)**	(188)	(248)	(228)
MN St. rev PS	24	172	49	110	-25
	(92)	(107)	(126)	(147)	(153)
MN Loc. tax PS	-11	413	248	264	129
	(82)	(95)**	(112)*	(159)	(163)
OH Rev. PS	444	952	650	269	47
	(185)*	(253)**	(314)*	(291)	(292)
OH St. rev PS	191	544	400	60	-110
	(159)	(210)**	(274)	(259)	(252)
OH Loc. tax PS	208	315	227	145	183
	(66)**	(74)**	(96)*	(108)	(118)
WI Rev. PS	422	750	402	304	249
	(104)**	(194)**	(140)**	(165)	(184)
WI St. rev PS	120	54	149	191	309
	(73)	(71)	(83)	(99)	(117)**
WI Loc. tax PS	158	408	222	154	5
	(85)	(118)**	(126)	(151)	(161)

Note: Regression discontinuity estimates of the effect of passing a referendum. Each estimate comes from a separate regression that includes only referenda within 10 percentage points of the passage threshold. Each model has one observation per referenda and year relative to that referenda and include all available observations from the six years prior to the referenda and one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

Table IV: Effect of passage on expenditures and revenues by state and type of referenda

	Yr. of ref	1 yr. later	2 yr. later	3 yr. later	4 yr. later
MI, Bond, Rev. PS	175	448	-129	28	81
	(312)	(256)	(313)	(227)	(250)
MI, Bond, St. rev PS	236	101	186	8	82
	(249)	(158)	(148)	(159)	(207)
MI, Bond, Loc. tax PS	-44	208	-241	41	71
	(133)	(143)	(293)	(245)	(277)
MN, Non-Bond, Rev. PS	85	674	165	330	-1
	(119)	(127)**	(184)	(250)	(233)
MN, Non-Bond, St. rev PS	2	142	-29	61	-82
	(100)	(119)	(139)	(161)	(165)
MN, Non-Bond, Loc. tax PS	-10	424	179	207	111
	(89)	(104)**	(114)	(155)	(161)
MN, Bond, Rev. PS	494	871	1,741	1,213	308
	(298)	(453)	(558)**	(630)	(600)
MN, Bond, St. rev PS	224	445	792	648	712
	(212)	(238)	(289)**	(323)*	(408)
MN, Bond, Loc. tax PS	-6	237	873	782	244
	(232)	(288)	(450)	(551)	(570)
OH, Non-Bond, Rev. PS	326	444	106	-204	-142
	(223)	(258)	(254)	(318)	(339)
OH, Non-Bond, St. rev PS	162	123	14	-219	-154
	(194)	(219)	(212)	(290)	(299)
OH, Non-Bond, Loc. tax PS	230	283	129	32	78
	(75)**	(81)**	(106)	(116)	(133)
OH, Bond, Rev. PS	926	2,460	2,123	1,760	539
	(267)**	(630)**	(1,013)*	(730)*	(623)
OH, Bond, St. rev PS	334	1,928	1,393	991	-28
	(180)	(519)**	(936)	(658)	(519)
OH, Bond, Loc. tax PS	133	417	496	474	437
	(144)	(189)*	(235)*	(279)	(278)
WI, Non-Bond, Rev. PS	466	457	263	475	60
	(205)*	(268)	(261)	(323)	(369)
WI, Non-Bond, St. rev PS	146	79	288	433	484
	(127)	(139)	(152)	(181)*	(206)*
WI, Non-Bond, Loc. tax PS	279	324	-8	42	-119
	(180)	(263)	(267)	(328)	(346)
WI, Bond, Rev. PS	301	842	367	113	243
	(103)**	(274)**	(158)*	(171)	(191)
WI, Bond, St. rev PS	113	87	139	108	266
	(77)	(85)	(98)	(113)	(133)*
WI, Bond, Loc. tax PS	-25	303	168	77	-75
	(77)	(105)**	(125)	(134)	(155)

Note: Regression discontinuity estimates of the effect of passing a referendum. Each estimate comes from a separate regression that includes only referenda within 10 percentage points of the passage threshold. Each model has one observation per referenda X year relative to that referenda and include all available observations from the six years prior to the referenda and one year following. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

Table V: Regression discontinuity - instrumental variables estimates of the effect of local tax revenues on private contributions.

	Main	w/timetrends	No Zeros	Above Median Contr
Loc. tax PS	0.0043	0.0004	0.0034	0.0006
	(0.0094)	(0.0142)	(0.0120)	(0.0119)
St. rev PS	0.0027	-0.0061	0.0037	-0.0003
	(0.0036)	(0.0042)	(0.0044)	(0.0053)

Note: The table shows fuzzy regression discontinuity estimates of the effect of local tax and state tax revenue per student on contributions per student. Each column represents a separate regression, where the first stage is given by Equation 1.2 with $y_{is,t+\tau}$ being local taxes per student, and the second stage excludes the instrument, p_{ist}, whose effect is allowed to vary by state and whether it is a bond. Both stages include only referenda within 10 percentage points of the passage threshold and include linear relationships between vote share and the outcomes on either side of the threshold. Each model has one observation per referenda by year relative to that referenda and include all available observations from six years prior to the referenda. Models include referenda fixed effects, state by year fixed effects, and state by relative year fixed effects. Standard errors are clustered at the school district level. All numbers are in 2014 dollars.

I display these results in Table V. The first column shows the main estimates from this analysis. The crowd-out effect of local taxes is nearly unchanged, showing that an increase in taxes by one dollar increases contributions by .43 cents. I cannot reject that residents react the same to state and local revenues.

A.3 Alternative specifications of regression discontinuity

Table VI: Regression discontinuity - instrumental variables estimates of the effect of local tax revenues on private contributions with alternative bandwidths.

	Contr. PS
bw=5.58	0.0049
	(0.0142)
bw=6.58	0.0100
	(0.0124)
bw=7.58	0.0072
	(0.0113)
bw=8.58	0.0098
	(0.0106)
bw=9.58	0.0034
	(0.0105)
bw=10.58	0.0062
	(0.0104)
bw=11.58	0.0070
	(0.0095)
bw=12.58	0.0059
	(0.0095)
bw=13.58	0.0023
	(0.0098)
bw=14.58	-0.0014
	(0.0101)
bw=15.58	-0.0000
	(0.0098)

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

Note: Table V presents results with bandwidths set according to Imbens and Kalyanaraman (2011). This table shows the same models with alternative bandwidths.

Appendix B

Appendices to Chapter 2

B.1 Additional Results

Table I presents intent to treat estimates estimated using Equation 2.7 while the results in the main paper use Equation 2.8. While this is less efficient, it allows for testing that there is no impact on housing prices preceding the vote. The effect of passing a referendum one or two years later is both insignificant and very small in magnitude.

Figure I, presents additional visual interpretations of the intent to treat effect for time

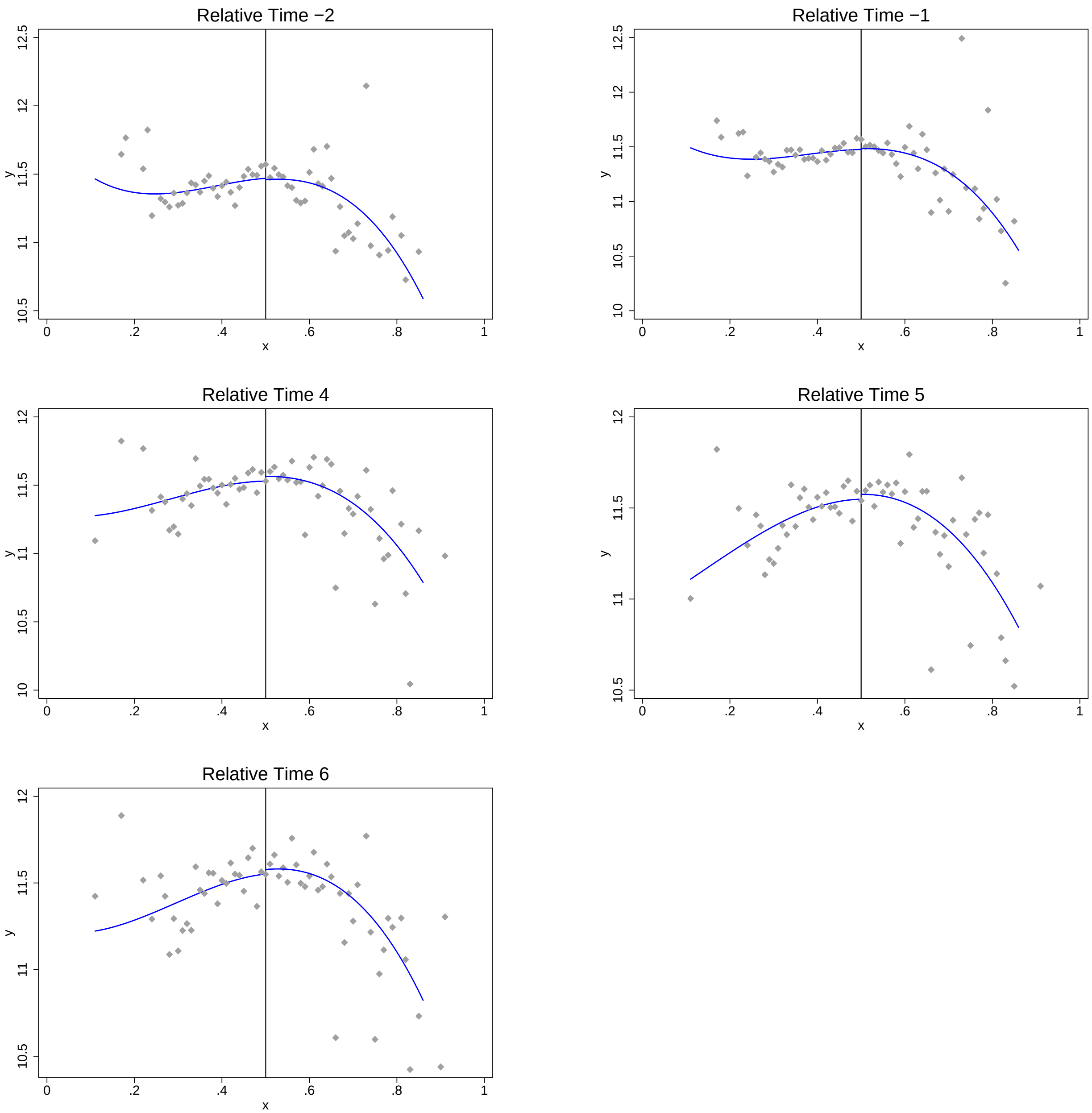
Table I: Unpooled estimates of ITT effect of bond passage on housing prices

	θ_{-2}^{ITT}	θ_{-1}^{ITT}	θ_{0}^{ITT}	θ_{1}^{ITT}	θ_{2}^{ITT}	θ_{3}^{ITT}	θ_{4}^{ITT}	θ_{5}^{ITT}	θ_{6}^{ITT}
Passed	-0.0101	0.0078	0.0321	-0.0075	0.0044	0.0577	0.0350	0.0269	0.0262
	(0.058)	(0.053)	(0.050)	(0.052)	(0.052)	(0.058)	(0.050)	(0.054)	(0.056)
Observations	799	834	841	849	862	854	838	809	791

Note: θ_{τ}^{ITT} represents the total effect of bond passage τ years prior on current house prices. Each column represents a separate regression estimated as in Equation 2.7 using observations only from τ years relative to a referendum. Each models contain a cubic polynomial in the vote share. Standard errors robust to clustering at school district level.**p<0.01 * p<0.05, + p<0.10

periods not shown in the main paper. There is no visually apparent discontinuity in any year and those years following the passage of the bond appear very similar to those prior to it. This is consistent with the main finding.

Figure I: CEF of House Price by Vote Share



Note: Figures show the conditional expectation of house prices by vote share. Line fitted with cubic polynomial and discontinuity at cutoff. Scatter plot show average house prices in one percentage point bins of the share of votes in favor of the bond.

Appendix C

Appendices to Chapter 3

C.1 Proof of Proposition 1

We need to show that $V(\tau_m)$ exceeds $V(\ell)$ for any limit ℓ in the range $[\underline{\tau}, \tau_m)$ or $(\tau_m, \overline{\tau}]$. Since b exceeds $(\overline{\tau} - \tau_m)/\underline{\tau}$, $\ell/(1+b)$ is less than $\underline{\tau}$ for any limit ℓ in the range $[\underline{\tau}, \tau_m]$ and, if b is less than 1, $\ell/(1-b)$ exceeds $\overline{\tau}$ for any limit ℓ in the range $[\tau_m, \overline{\tau}]$. Thus, from (3.3), we have that

$$V(\tau_m) = \int_{\underline{\tau}}^{\tau_m} [(1+b)\tau - \tau_m]\, h(\tau)d\tau + \int_{\tau_m}^{\overline{\tau}} [\tau_m - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau. \quad \text{(C.1)}$$

Recall from the analysis in the text, that we know already that for any ℓ in the range $[\underline{\tau}, \tau_m)$ or $(\tau_m, \overline{\tau}]$ we have that

$$V(\tau_m) > \int_{\underline{\tau}}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau + \int_{\ell}^{\overline{\tau}} [\ell - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau. \quad \text{(C.2)}$$

Consider a limit ℓ in the range $[\underline{\tau}, \tau_m)$. If b exceeds 1 or if $\ell/(1-b)$ exceeds $\overline{\tau}$, then from (3.3), $V(\ell)$ is equal to the right hand side of (C.2) and thus the desired inequality holds. If $\ell/(1-b)$ is less than $\overline{\tau}$, then from (3.3), we have that

$$V(\ell) = \int_{\underline{\tau}}^{\ell} \left[(1+b)\tau - \ell\right] h(\tau) d\tau + \int_{\ell}^{\frac{\ell}{1-b}} \left[\ell - (1-b)\tau\right] h(\tau) d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau) d\tau. \qquad \text{(C.3)}$$

From (C.1) and (C.3), we have that

$$\begin{aligned} V(\tau_m) - V(\ell) &= \int_{\underline{\tau}}^{\tau_m} \left[(1+b)\tau - \tau_m\right] h(\tau) d\tau + \int_{\tau_m}^{\overline{\tau}} \left[\tau_m - (1-b)\tau\right] h(\tau) d\tau \\ &\quad - \int_{\underline{\tau}}^{\ell} \left[(1+b)\tau - \ell\right] h(\tau) d\tau - \int_{\ell}^{\frac{\ell}{1-b}} \left[\ell - (1-b)\tau\right] h(\tau) d\tau. \end{aligned}$$

We can write this difference as

$$\begin{aligned} V(\tau_m) - V(\ell) &= \int_{\underline{\tau}}^{\ell} \left[(1+b)\tau - \tau_m\right] h(\tau) d\tau + \int_{\ell}^{\tau_m} \left[(1+b)\tau - \tau_m\right] h(\tau) d\tau + \\ &\quad \int_{\tau_m}^{\frac{\ell}{1-b}} \left[\tau_m - (1-b)\tau\right] h(\tau) d\tau + \int_{\frac{\ell}{1-b}}^{\overline{\tau}} \left[\tau_m - (1-b)\tau\right] h(\tau) d\tau \\ &\quad - \int_{\underline{\tau}}^{\ell} \left[(1+b)\tau - \ell\right] h(\tau) d\tau - \int_{\ell}^{\frac{\ell}{1-b}} \left[\ell - (1-b)\tau\right] h(\tau) d\tau, \end{aligned}$$

which simplifies to

$$\begin{aligned} V(\tau_m) - V(\ell) &= \int_{\underline{\tau}}^{\ell} \left[\ell - \tau_m\right] h(\tau) d\tau + \int_{\ell}^{\tau_m} \left[2\tau - (\tau_m + \ell)\right] h(\tau) d\tau + \\ &\quad \int_{\tau_m}^{\frac{\ell}{1-b}} \left[\tau_m - \ell\right] h(\tau) d\tau + \int_{\frac{\ell}{1-b}}^{\overline{\tau}} \left[\tau_m - (1-b)\tau\right] h(\tau) d\tau. \end{aligned}$$

This in turn can be rewritten as

$$\begin{aligned} V(\tau_m) - V(\ell) &= \int_{\underline{\tau}}^{\tau_m} \left[\ell - \tau_m\right] h(\tau) d\tau + \int_{\ell}^{\tau_m} \left[2\tau - (\tau_m + \ell) - \ell + \tau_m\right] h(\tau) d\tau + \\ &\quad \int_{\tau_m}^{\overline{\tau}} \left[\tau_m - \ell\right] h(\tau) d\tau + \int_{\frac{\ell}{1-b}}^{\overline{\tau}} \left[\tau_m - (1-b)\tau - \tau_m + \ell\right] h(\tau) d\tau, \end{aligned}$$

which simplifies to

$$V(\tau_m) - V(\ell) = 2\int_{\ell}^{\tau_m} [\tau - \ell]\, h(\tau) d\tau - \int_{\frac{\ell}{1-b}}^{\overline{\tau}} [(1-b)\tau - \ell]\, h(\tau) d\tau.$$

Thus, we need to show that

$$2\int_{\ell}^{\tau_m} [\tau - \ell]\, h(\tau) d\tau > \int_{\frac{\ell}{1-b}}^{\overline{\tau}} [(1-b)\tau - \ell]\, h(\tau) d\tau.$$

Because $h(\tau)$ is non-decreasing on $[\ell, \tau_m]$, we know that

$$2\int_{\ell}^{\tau_m} [\tau - \ell]\, h(\tau) d\tau \geq 2\left[\frac{\tau_m + \ell}{2} - \ell\right] \int_{\ell}^{\tau_m} h(\tau) d\tau = (\tau_m - \ell) \int_{\ell}^{\tau_m} h(\tau) d\tau.$$

Similarly, because $h(\tau)$ is non-increasing on $[\frac{\ell}{1-b}, \overline{\tau}]$, we know that

$$\int_{\frac{\ell}{1-b}}^{\overline{\tau}} [(1-b)\tau - \ell]\, h(\tau) d\tau \leq \left[(1-b)\left(\frac{\overline{\tau} + \frac{\ell}{1-b}}{2}\right) - \ell\right] \int_{\frac{\ell}{1-b}}^{\overline{\tau}} h(\tau) d\tau = \left(\frac{(1-b)\overline{\tau} - \ell}{2}\right) \int_{\frac{\ell}{1-b}}^{\overline{\tau}} h(\tau) d\tau.$$

Since $\tau_m \geq (1-b)\overline{\tau}$, it therefore suffices to show that

$$\int_{\ell}^{\tau_m} h(\tau) d\tau \geq \int_{\frac{\ell}{1-b}}^{\overline{\tau}} h(\tau) d\tau.$$

Given the assumed properties of h, a sufficient condition for this is that

$$\tau_m - \ell \geq \overline{\tau} - \frac{\ell}{1-b} \Leftrightarrow \frac{b\ell}{1-b} > \overline{\tau} - \tau_m.$$

But we know that

$$\frac{b\ell}{1-b} \geq \frac{b\underline{\tau}}{1-b} \geq \frac{(\overline{\tau} - \tau_m)}{1-b} > \overline{\tau} - \tau_m.$$

Now consider a limit ℓ in the range $(\tau_m, \overline{\tau}]$. If $\ell/(1+b)$ is less than $\underline{\tau}$, then from (3.3),

$V(\ell)$ is equal to the right hand side of (C.2) and thus the desired inequality holds. If $\ell/(1+b)$ exceeds $\underline{\tau}$, then from (3.3), we have that

$$V(\ell) = \int_{\frac{\ell}{1+b}}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau + \int_{\ell}^{\overline{\tau}} [\ell - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau. \qquad \text{(C.4)}$$

Note that $\ell/(1+b) < \tau_m$ and thus using (C.1) and (C.4) we can write

$$\begin{aligned} V(\tau_m) - V(\ell) \;\; = \;\; & \int_{\underline{\tau}}^{\frac{\ell}{1+b}} [(1+b)\tau - \tau_m]\, h(\tau)d\tau + \int_{\frac{\ell}{1+b}}^{\tau_m} [(1+b)\tau - \tau_m]\, h(\tau)d\tau \\ & + \int_{\tau_m}^{\ell} [\tau_m - (1-b)\tau]\, h(\tau)d\tau + \int_{\ell}^{\overline{\tau}} [\tau_m - (1-b)\tau]\, h(\tau)d\tau \\ & - \int_{\frac{\ell}{1+b}}^{\tau_m} [(1+b)\tau - \ell]\, h(\tau)d\tau - \int_{\tau_m}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau - \int_{\ell}^{\overline{\tau}} [\ell - (1-b)\tau]\, h(\tau)d\tau. \end{aligned}$$

This equals

$$\begin{aligned} V(\tau_m) - V(\ell) \;\; = \;\; & \int_{\underline{\tau}}^{\frac{\ell}{1+b}} [(1+b)\tau - \tau_m]\, h(\tau)d\tau + \int_{\frac{\ell}{1+b}}^{\tau_m} [\ell - \tau_m]\, h(\tau)d\tau + \int_{\tau_m}^{\ell} [\tau_m + \ell - 2\tau]\, h(\tau)d\tau \\ & + \int_{\ell}^{\overline{\tau}} [\tau_m - \ell]\, h(\tau)d\tau, \end{aligned}$$

which simplifies to

$$V(\tau_m) - V(\ell) = 2\int_{\tau_m}^{\ell} [\ell - \tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\frac{\ell}{1+b}} [\ell - (1+b)\tau]\, h(\tau)d\tau.$$

This is the difference illustrated in Panel D of Figure I. Thus, we need to show that

$$2\int_{\tau_m}^{\ell} [\ell - \tau]\, h(\tau)d\tau > \int_{\underline{\tau}}^{\frac{\ell}{1+b}} [\ell - (1+b)\tau]\, h(\tau)d\tau.$$

Because $h(\tau)$ is non-increasing on $[\tau_m, \ell]$, we know that

$$2\int_{\tau_m}^{\ell} [\ell - \tau]\, h(\tau) d\tau \geq 2\left[\frac{\ell + \tau_m}{2} - \tau_m\right] \int_{\tau_m}^{\ell} h(\tau) d\tau = [\ell - \tau_m] \int_{\tau_m}^{\ell} h(\tau) d\tau.$$

Similarly, because $h(\tau)$ is non-decreasing on $[\underline{\tau}, \frac{\ell}{1+b}]$, we know that

$$\int_{\underline{\tau}}^{\frac{\ell}{1+b}} [\ell - (1+b)\tau]\, h(\tau) d\tau \leq \left[\ell - (1+b)\left(\frac{\frac{\ell}{1+b} + \underline{\tau}}{2}\right)\right] \int_{\underline{\tau}}^{\frac{\ell}{1+b}} h(\tau) d\tau = \left(\frac{\ell - (1+b)\underline{\tau}}{2}\right) \int_{\underline{\tau}}^{\frac{\ell}{1+b}} h(\tau) d\tau.$$

We know that $\tau_m \leq (1+b)\underline{\tau}$, so it suffices to show that

$$\int_{\tau_m}^{\ell} h(\tau) d\tau \geq \int_{\underline{\tau}}^{\frac{\ell}{1+b}} h(\tau) d\tau.$$

Given the assumed properties of h, a sufficient condition for this is that

$$\ell - \tau_m \geq \frac{\ell}{1+b} - \underline{\tau} \Leftrightarrow \frac{b\ell}{1+b} > \tau_m - \underline{\tau}.$$

But we know that

$$\frac{b\ell}{1+b} \geq \frac{b\underline{\tau}}{1+b} \geq \frac{(\overline{\tau} - \tau_m)}{1+b} > \tau_m - \underline{\tau}.$$

■

C.2 Proof of Lemma 1

Proposition 1 implies that the result is true for b larger than $(\overline{\tau} - \tau_m) / \underline{\tau}$. Thus, we just need to show that the result is true for b smaller than $(\overline{\tau} - \tau_m) / \underline{\tau}$. Consider some limit $\ell < \tau_m$. We will show that marginally increasing ℓ will increase the voter's payoff.

Suppose first that $\ell \geq (1-b)\overline{\tau}$. If $\ell \geq (1+b)\underline{\tau}$, then, from (3.3), we have that

$$V(\ell) = \int_{\frac{\ell}{1+b}}^{\ell} \left[(1+b)\tau - \ell\right] h(\tau) d\tau + \int_{\ell}^{\overline{\tau}} \left[\ell - (1-b)\tau\right] h(\tau) d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau) d\tau.$$

Note that

$$V'(\ell) = -\int_{\frac{\ell}{1+b}}^{\ell} h(\tau) d\tau + \int_{\ell}^{\overline{\tau}} h(\tau) d\tau = 1 + H(\frac{\ell}{1+b}) - 2H(\ell) > 0,$$

which implies that raising the limit slightly will increase the voter's payoff. If $\ell < (1+b)\underline{\tau}$, then, from (3.3), we have that

$$V(\ell) = \int_{\underline{\tau}}^{\ell} \left[(1+b)\tau - \ell\right] h(\tau) d\tau + \int_{\ell}^{\overline{\tau}} \left[\ell - (1-b)\tau\right] h(\tau) d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau) d\tau.$$

Note that

$$V'(\ell) = -\int_{\underline{\tau}}^{\ell} h(\tau) d\tau + \int_{\ell}^{\overline{\tau}} h(\tau) d\tau = 1 - 2H(\ell) > 0,$$

which again implies that raising ℓ marginally benefits the voter.

Now suppose that $\ell < (1-b)\overline{\tau}$. If $\ell \geq (1+b)\underline{\tau}$, then, from (3.3), we have that

$$V(\ell) = \int_{\frac{\ell}{1+b}}^{\ell} \left[(1+b)\tau - \ell\right] h(\tau) d\tau + \int_{\ell}^{\frac{\ell}{1-b}} \left[\ell - (1-b)\tau\right] h(\tau) d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau) d\tau.$$

Note that

$$V'(\ell) = -\int_{\frac{\ell}{1+b}}^{\ell} h(\tau) d\tau + \int_{\ell}^{\frac{\ell}{1-b}} h(\tau) d\tau.$$

Given that $\frac{\ell}{1+b} < \ell < \tau_m$ and that

$$\ell - \frac{\ell}{1+b} = \frac{b\ell}{1+b} < \frac{b\ell}{1-b} = \frac{\ell}{1-b} - \ell,$$

the assumption that h is symmetric and non-decreasing on $[\underline{\tau}, \tau_m]$ implies that

$$\int_{\ell}^{\frac{\ell}{1-b}} h(\tau)d\tau > \int_{\frac{\ell}{1+b}}^{\ell} h(\tau)d\tau.$$

To see this, note that for any $\tau \in [\frac{\ell}{1+b}, \ell]$ we can associate a unique $\tau' \in [\ell, \frac{\ell}{1-b}]$ (e.g., $\tau' = 2l - x$) which has a higher density. Thus, it must be the case that $V'(\ell) > 0$ which implies that raising the limit slightly will increase the voter's payoff. If $\ell < (1+b)\underline{\tau}$, then, from (3.3), we have that

$$V(\ell) = \int_{\underline{\tau}}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau + \int_{\ell}^{\frac{\ell}{1-b}} [\ell - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau.$$

Note that

$$V'(\ell) = -\int_{\underline{\tau}}^{\ell} h(\tau)d\tau + \int_{\ell}^{\frac{\ell}{1-b}} h(\tau)d\tau.$$

Given that $\underline{\tau} < \ell < \tau_m$ and that

$$\ell - \underline{\tau} < l - \frac{\ell}{1+b} = \frac{b\ell}{1+b} < \frac{b\ell}{1-b} = \frac{\ell}{1-b} - \ell,$$

the assumption that h is symmetric and non-decreasing on $[\underline{\tau}, \tau_m]$ implies that

$$\int_{\ell}^{\frac{\ell}{1-b}} h(\tau)d\tau > \int_{\underline{\tau}}^{\ell} h(\tau)d\tau.$$

Again, to see this note that for any $\tau \in [\underline{\tau}, \ell]$ we can find a unique $\tau' \in [\ell, \frac{\ell}{1-b}]$ (e.g., $\tau' = 2l - x$) which has a higher density. Thus, $V'(\ell) > 0$ which again implies that raising ℓ marginally benefits the voter. ■

C.3 Proof of Proposition 2

For limits $\ell \in [\tau_m, \overline{\tau}]$, we have that $\ell/(1+b)$ is greater than or equal to $\tau_m/(1+b)$ which, since b is less than $(\tau_m - \underline{\tau})/\underline{\tau}$, exceeds $\underline{\tau}$. In addition, if $b < 1$, we have that $\ell/(1-b)$ is greater than or equal to $\tau_m/(1-b)$ which, since b exceeds $(\overline{\tau} - \tau_m)/\overline{\tau}$, exceeds $\overline{\tau}$. Thus, for limits $\ell \in [\tau_m, \overline{\tau}]$, (3.3) implies that

$$V(\ell) = \int_{\frac{\ell}{1+b}}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau + \int_{\ell}^{\overline{\tau}} [\ell - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau.$$

This means that

$$V'(\ell) = -\int_{\frac{\ell}{1+b}}^{\ell} h(\tau)d\tau + \int_{\ell}^{\overline{\tau}} h(\tau)d\tau = 1 + H\left(\frac{\ell}{1+b}\right) - 2H(\ell).$$

It follows that at the optimal limit

$$1 + H\left(\frac{\ell}{1+b}\right) = 2H(\ell),$$

which is (3.6). To see that this equation has a solution, note that

$$1 + H(\frac{\tau_m}{1+b}) > 1 = 2H(\tau_m),$$

and that

$$1 + H\left(\frac{\overline{\tau}}{1+b}\right) < 2H(\overline{\tau}) = 2.$$

Thus, by the *Intermediate Value Theorem*, there exists a solution to equation (3.6). ■

C.4 Proof of Proposition 3

For limits $\ell \in [\tau_m, \overline{\tau}]$, we have that $\ell/(1+b)$ is greater than or equal to $\tau_m/(1+b)$ which, since b is less than $(\overline{\tau} - \tau_m)/\overline{\tau}$, exceeds $\underline{\tau}$. Moreover, since $\tau_m/(1-b)$ is less than $\overline{\tau}$ which is less than $\overline{\tau}/(1-b)$, we have that

$$\frac{\ell}{1-b} \gtrless \overline{\tau} \text{ as } l \gtrless (1-b)\,\overline{\tau}.$$

It follows from (3.3) that the voter's welfare with limit $\ell \in [\tau_m, \overline{\tau}]$ is

$$V(\ell) = \begin{cases} \int_{\frac{\ell}{1+b}}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau + \int_{\ell}^{\frac{\ell}{1-b}} [\ell - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau & \text{if } \ell < (1-b)\,\overline{\tau} \\ \int_{\frac{\ell}{1+b}}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau + \int_{\ell}^{\overline{\tau}} [\ell - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau & \text{if } \ell \geq (1-b)\,\overline{\tau} \end{cases}.$$

Thus, the impact on welfare of a small increase in the limit is

$$V'(\ell) = \begin{cases} H(\frac{\ell}{1-b}) + H(\frac{\ell}{1+b}) - 2H(\ell) & \text{if } \ell < (1-b)\overline{\tau} \\ 1 + H(\frac{\ell}{1+b}) - 2H(\ell) & \text{if } \ell \geq (1-b)\overline{\tau} \end{cases}.$$

It follows that the optimal limit is either such that $\ell \in [\tau_m, (1-b)\overline{\tau})$ and solves

$$H(\frac{\ell}{1-b}) + H(\frac{\ell}{1+b}) = 2H(\ell),$$

or is such that $\ell \in [(1-b)\overline{\tau}, \overline{\tau}]$ and solves

$$1 + H(\frac{\ell}{1+b}) = 2H(\ell).$$

It is straightforward to show that at least one of these equations must have a solution in the relevant range. The assumption that b is less than $(\overline{\tau} - \tau_m)/\overline{\tau}$ implies that b is less than $(\tau_m - \underline{\tau})\,/\,\underline{\tau}$ which means that $\tau_m/(1+b)$ exceeds $\underline{\tau}$. Thus,

$$1 + H(\frac{\tau_m}{1+b}) > 1 = 2H(\tau_m).$$

Since

$$1 + H(\frac{\overline{\tau}}{1+b}) < 2 = 2H(\overline{\tau}),$$

there exists $\ell \in (\tau_m, \overline{\tau})$ such that

$$1 + H(\frac{\ell}{1+b}) = 2H(\ell).$$

Suppose that for all such ℓ it is the case that $\ell < (1-b)\overline{\tau}$, then it must be the case that

$$1 + H(\frac{(1-b)\overline{\tau}}{1+b}) < 2H((1-b)\overline{\tau}) \Leftrightarrow H(\frac{(1-b)\overline{\tau}}{1+b}) + H(\frac{(1-b)\overline{\tau}}{1-b}) < 2H((1-b)\overline{\tau}).$$

If

$$H(\frac{\tau_m}{1-b}) + H(\frac{\tau_m}{1+b}) > 1 = 2H(\tau_m), \tag{C.5}$$

this implies that there exists $\ell \in [\tau_m, (1-b)\overline{\tau}]$ such that

$$H(\frac{\ell}{1-b}) + H(\frac{\ell}{1+b}) = 2H(\ell).$$

It suffices, therefore, to prove that (C.5) holds. Note that symmetry implies that

$$H(\frac{\tau_m}{1+b}) = 1 - H(\tau_m + \frac{b\tau_m}{1+b}).$$

Moreover, we have that

$$\frac{\tau_m}{1-b} = \tau_m + \frac{b\tau_m}{1-b} > \tau_m + \frac{b\tau_m}{1+b}.$$

This means that

$$H(\frac{\tau_m}{1+b}) + H(\frac{\tau_m}{1-b}) > H(\frac{\tau_m}{1+b}) + H(\tau_m + \frac{b\tau_m}{1+b}) = 1.$$

■

C.5 Proof of Proposition 5

First, consider the case when the politician's bias exceeds $(\overline{\tau} - \tau_m)/\underline{\tau}$. We need to show that $V_N(\tau_m)$ exceeds $V_N(\ell)$ for any limit ℓ in the range $[\underline{\tau}, \tau_m)$ or $(\tau_m, \overline{\tau}]$. As in the with override case, since b exceeds $(\overline{\tau} - \tau_m)/\underline{\tau}$, $\ell/(1+b)$ is less than $\underline{\tau}$ for any limit ℓ in the range $[\underline{\tau}, \tau_m]$. Thus, from (3.9), we have that

$$V_N(\tau_m) = \int_{\underline{\tau}}^{\tau_m} [(1+b)\tau - \tau_m]\, h(\tau)d\tau + \int_{\tau_m}^{\overline{\tau}} [\tau_m - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau, \tag{C.6}$$

which is equal to $V(\tau_m)$. Recall from the analysis in the text, that we know already that for any ℓ in the range $[\underline{\tau}, \tau_m)$ or $(\tau_m, \overline{\tau}]$ we have that

$$V(\tau_m) > \int_{\underline{\tau}}^{\ell} [(1+b)\tau - \ell]\, h(\tau)d\tau + \int_{\ell}^{\overline{\tau}} [\ell - (1-b)\tau]\, h(\tau)d\tau - \int_{\underline{\tau}}^{\overline{\tau}} b\tau h(\tau)d\tau. \tag{C.7}$$

Consider a limit ℓ in the range $[\underline{\tau}, \tau_m)$. $V_N(\ell)$ is equal to the right hand side of (C.7) and thus the desired inequality holds. For ℓ greater than τ_m, Since $\ell/(1-b)$ exceeds $\overline{\tau}$, $V_N(\ell) = V(\ell)$ and by the argument in Proposition 1, the inequality holds and the optimal limit is τ_m.

Second, consider the case when the politician's bias is less than $(\overline{\tau} - \tau_m)/\underline{\tau}$. For limits

$\ell \in [\tau_m, \overline{\tau}]$, we have that $\ell/(1+b)$ is greater than or equal to $\tau_m/(1+b)$ which, since b is less than $(\tau_m - \underline{\tau})/\underline{\tau}$, exceeds $\underline{\tau}$. As a result, from (3.9),

$$V_N'(\ell) = 1 - 2H(\ell) + H(\frac{\ell}{1+b}),$$

and the optimal limit will satisfy

$$1 + H\left(\frac{\ell}{1+b}\right) = 2H(\ell),$$

which by the argument in the proof to proposition 2, has a solution. ■

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