Do local government fiscal spillovers exist? Evidence from counties, municipalities, and school districts

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ARTICLE INFO

Article history:
Received 21 July 2011
Received in revised form 18 November 2013
Accepted 13 December 2013
Available online 25 December 2013

JEL classification:
H7
H4
H3
H2

Keywords:
Fiscal spillovers
Inter-jurisdictional competition
Taxes and spending

ABSTRACT

Numerous theories posit that the fiscal decisions of one jurisdiction influence the fiscal decisions of its neighbors. The main contribution of this paper is to address empirical difficulties in testing for spillovers using a regression discontinuity design on a newly collected dataset. I utilize close elections from this large dataset of local referenda in Ohio to isolate the effect of exogenous increases in taxation and spending of one jurisdiction on neighbors’ fiscal decisions. For all jurisdictional types and referenda revenue sources (bonds, income, property, and sales tax), there is no evidence of spillovers, and relatively small effects can be ruled out.

Published by Elsevier B.V.

1. Introduction

A fundamental question about governments is to what extent they are influenced by one another. A large theoretical literature presumes interactions and has identified several pathways by which the fiscal decisions of one jurisdiction influence the fiscal decisions of its neighbors (such as via inter-jurisdictional tax competition, yardstick competition, conventional spillovers, or Tiebout resorting).1 A key empirical issue in this theoretical literature is to what degree do the fiscal decisions of one jurisdiction influence its neighbors’ fiscal decisions. Are they of a large magnitude, or are they small or nonexistent? A failure to find any effect would raise questions about the importance of those theoretical channels. In this paper, I empirically explore this question of fiscal spillovers on the local level.

There are several challenges to identifying the effect of fiscal spillovers. Unobserved determinants of fiscal decisions might be correlated across neighbors, and neighbors’ decisions are jointly determined in equilibrium. To provide a strong research design that addresses these challenges, I collect a new dataset. In Ohio, local governments often require the explicit approval of voters to raise taxes.2 My dataset consists of tens of thousands of these tax referenda that are economically significant and span multiple types of government and tax instruments. The elections for tax increases lend themselves to a regression discontinuity design that exploits the underlying continuity in jurisdiction characteristics around the threshold for measure approval to produce approximate random assignment.

I examine whether jurisdictions respond to exogenous referendum passage by their neighbors. The analysis covers counties, municipalities, and school districts, and bonds, income tax, property tax, and sales tax measures. I first explore the issue graphically to
determine whether there is evidence of any discontinuous jumps at the threshold for voter approval and then run formal econometric analyses.

Previewing the results, there is no evidence that spillovers exist for any jurisdictional type or revenue source. Plots yield no jump in the dependent variables at the threshold for voter approval. Formal analyses never find a statistically significant effect, and the estimates are reasonably precise. The main measure of neighborliness is spatial proximity (i.e. where I test for the existence of spillovers), and the results are robust to alternative ways of defining neighbors. The results are also robust to focusing only on the largest of measures as well as limiting the analysis to geographic areas where spillovers are most likely to be present. Lastly, no effect of referendum passage on mobility or sorting is found. The results therefore call into question theoretical models that presume spillovers on the local level.

The previous empirical literature on fiscal spillovers has grown in recent years. Most studies in the literature test for spillovers by instrumenting for neighbor fiscal behavior using neighbor characteristics, such as demographics, as well as neighbor lags, in taxes and spending. Papers have examined strategic fiscal behavior among countries (Devereux et al., 2008), states (Case et al., 1993; Figlio et al., 1999; Saavedra, 2000; Weaton, 2000; Devereux et al., 2007; Chirinko and Wilson, 2008), municipalities (Brueckner and Saavedra, 2001; Buettner, 2003; Bordignon et al., 2003; Bruihart and Jametti, 2006), and school districts (Millimet and Rangaprasad, 2007; Reback, 2009). Several recent studies attempt to more directly confront the identification problem by looking for natural experiments in taxes and spending (Baicker, 2004, 2005). The empirical literature has tended to find large positive spillovers across jurisdictions, and I replicate those results with my data using neighbor characteristics as instruments. Relative to the previous studies, the main innovation in this paper is to use a regression discontinuity design that plausibly isolates exogenous variation in taxes and spending.

The remainder of the paper is organized as follows. Section 2 covers the institutional background and referendum process in Ohio. Section 3 describes the data collected for the project. Section 4 describes the research design. Section 5 investigates the validity of the research design. Section 6 presents a graphical analysis of the results. Section 7 presents the econometric results. Section 8 concludes.

2. Background

Dating back to a law passed during the Great Depression, local jurisdictions in Ohio have been restricted in their level of taxation without explicit voter approval. As a result, a significant portion of government revenue must be voter approved. Ohio state law restricts the unvoted property tax to be no more than 1% of assessed taxable value (which itself is approximately 35% of market value). This constraint is binding in practically every community, and as a result, forces the local governing body to turn to the voters for tax increases. This constraint became even more pronounced in 1976 when state legislators passed HB 920, which subsequently froze voter approved property tax increases to the amount collected in the first year the levies were in effect. This meant that jurisdictions no longer received increased tax receipts from those levies when property values increased and therefore had to the resort to the ballot even more often. Similarly, Ohio law also generally requires a majority of voters to agree on bonds, income, or sales tax increases. In this setting, I can apply a regression discontinuity design around the threshold for voter approval to isolate exogenous increases in taxes and spending. School districts, municipalities, and counties, which are legally separate entities, each have their own specific set of rules regarding which tax measures may be placed on the ballot.

The Ohio school system is served by a foundation system that mandates an expenditure floor but allows local districts to supplement the minimum through voter approved tax increases. School boards can place on the ballot 1) bond measures to finance school construction and 2) property tax and/or income tax increases to finance any other type of school expense. Of the latter two revenue sources, property taxes are the predominant source of school funding as less than two percent of local funding comes from the income tax. Of the 612 school districts currently in the state, 517 proposed bond measures and 535 proposed property tax increases in the sample period. Conditional on proposing at least one, the average number of measures considered was 2.5 and 7.7, respectively. Table 1 shows the number of measures proposed and passed, along with the amount of revenue they are intended to generate, the percent of the budget they constitute, the mean of the vote share, years the data span, and if applicable, the number of years the levy is to stay in effect. As is apparent from the table, property tax measures are especially commonplace and the majority are to stay in effect for five years. And important to the purpose of this analysis, the amount of revenue raised by the measures is a significant portion of school district budgets.

Turning to municipalities, the major sources of local revenue are property and income taxes. Because of the limited amount of revenue that can be generated from the unvoted property tax, municipalities must continually place referendums on the ballot to raise the property tax rate. Of the 2016 total municipalities, 1883 proposed a property tax increase, with an average of 8.9 measures per municipality. Income tax measures on the other hand are less frequent and are only available to incorporated municipalities, which excludes the townships that serve the municipality function for unincorporated areas. Cities are allowed to levy an unvoted one percent tax rate but must receive voter approval for any increase beyond that. While the majority of jurisdictions levying an income tax are above the one percent unvoted ceiling, many municipalities when initially deciding whether to enact an income tax of no more than 1% still put the issue up for a vote (for perhaps political reasons given the unpopularity of the income tax). The income tax applies to the wages of all individuals working within the jurisdiction as well as net profits for businesses operating within the jurisdiction. Of the 845 municipalities eligible to levy an income tax, 402 proposed an income tax measure with an average of 3.1 proposals. Table 1 contains more detailed information on these referenda. Income tax measures raise a greater amount of revenue than property tax measures, yet both are of significance to the overall fiscal situation of municipalities.

Counties also have two main sources of revenue from the local level: property and sales taxes. Given the same constraint on property tax rates, counties must also turn to elections to increase the property tax. Counties are also entitled to levy sales and use tax on top of the statewide rate. They can either do this through the normal referendum process or can enact an emergency sales tax increase after which the

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3 While the referendum process is used more frequently in Ohio than in some other states (a notable exception is Massachusetts), at least 40 other states require localities to gain voter approval to raise taxes in certain circumstances.

4 According to the 2008 Ohio Department of Taxation data on all property tax levies in effect, less than 0.2% of taxing districts (an area of land that uniquely shares the same county, municipality, school district, and special districts) were below the maximum rate at which new levies could be enacted without voter approval. The revenue generated from this 1% unvoted maximum tax rate is then divided among the county, municipality, and school district based on the division of the property tax that existed between 1929 and 1933.

5 One of the main costs of putting a referendum on the ballot is the process of collecting enough signatures for the measure to qualify (at least 10% of the number of voters who voted for governor within the last gubernatorial election).

6 School bonds are repaid by a long term property tax increase specified on the ballot.

7 Ohio is one of the few states to have widespread local income taxation.
from 1990 to 2007, all bond measures from 1993 to 2007, and all in-
municipalities, and school districts from several hundred Ohio Secretary
jurisdiction, which funds on an annual basis an average of 4% to
scal spillovers.

Notes: Mean amounts in 2006 real dollars and are per pupil for schools and per capita otherwise. They are all on an annual basis, except for school bonds [*], which are counted as a one-
time expenditure. “Amount as % of total revenue” represents the mean amount of a measure divided by the total revenue of the average jurisdiction (multiplied by 100) calculated from a sample of 2002 budgets. Data for the median length of income and sales tax measures is unavailable.

voters are entitled to vote for a repeal of the increase in the next election subsequent to that action being taken. Of the 88 counties in Ohio, all of them proposed property tax measures for an average of 19 measures, while 70 of them proposed sales tax measures for an average of 4.1 measures. More information on these measures is in Table 1.

All referenda can be placed on the ballot by the jurisdiction up to four times a year: two special elections and the primary and general elections. The costs of putting a referendum on the ballot are seemingly low given that most measures that are not approved are put on a subse-
quent ballot shortly thereafter. Tax measures can either be to renew a
current levy that is set to expire or to enact an altogether new levy. Any proposed tax measure can either be for general expenses or a
specific type of spending which by law must be spent on that category of
spending, except for bonds which must be spent on facility construc-
tion. The following is a sample language for a representative school
property tax measure:

Proposed Tax Levy (Addition): An additional tax for the benefit of the Grandview Heights City School District for the purposes of current operating expenses at a rate not exceeding 3.9 mills for each one dollar of
valuation, which amounts to $0.39 for each one hundred dollars of
valuation, for five years, commencing in 2010, first due in calendar year 2011.

Thus residents will know how the funds will be spent (ignoring any flypaper effects), what the tax rate will be, and for how long the tax will
stay in effect.

The referendum process is a widespread and important determin-
ant of the taxation and spending patterns of Ohio local govern-
ment. The average measure is economically important to the
jurisdiction, which funds on an annual basis an average of 4% to
22% of the budget depending on the measure (with bonds constituting
an even larger increase). This institution is leveraged to investigate fiscal spillovers.

3. Data

I hand compiled a database of these local tax referendum for counties, municipalities, and school districts from several hundred Ohio Secretary
of State election reports. The sample includes all property tax measures
from 1990 to 2007, all bond measures from 1993 to 2007, and all in-
come tax and sales tax measures from 1983 to 2007. For each
referendum, the data include the type of tax change, the intended
purpose of the measure, the tax value change, and the election results.

Among the measures were then merged with jurisdiction level demog-
graphics and centroid latitude and longitudes from the 1990 Census
as well as IRS County-to-County Migration data, all of which are used
to construct several definitions of neighborhood. Lastly, various fiscal
data were merged, which are discussed below in more depth.11

For schools, Common Core Data are available from 1992 to 2007, which include annual district level enrollment and finances. A panel of
annual school district property tax rates (the almost exclusive means of raising revenue on the local level) from 1989 to 2008 was constructed from Ohio Department of Taxation files.

For municipalities and counties, annual comprehensive fiscal data are not available. The Census of Governments surveys most jurisdictions about their financial situation only once every five years, and for a non-
negligible percent of jurisdictions is not even available that often. As a
result, I turn to extensive data from the Ohio Department of Taxation. By
jurisdiction, a large majority of the locally raised revenue side of munic-
ipal and county budgets is reconstructed. These rich data include all
major revenue sources and most of the minor sources of revenue. Annual property tax rates are merged with the annual assessed value
of aggregate property to calculate property tax revenue from 1996 to
2009. Income tax and sales tax collections were available from 1984 to
data were also available for a number of state transfer programs to local
government, most importantly those for which counties have discretion
in how they allocate funds among themselves and their municipalities
(as opposed to the ones where fees are allocated back from the State
and based on factors credibly outside the control of local jurisdictions,
such as cigarette sales, motor vehicle registration, gasoline purchases,
population, land value, and the poverty rate). These include the Local
Government Fund and Local Government Revenue Assistance Fund
from 1984 to 2008. The constructed budgets were then divided by
their respective 2000 population. Even though the municipal and coun-
try budgets are incomplete (focusing only on the revenue side and miss-
ing federal transfers and some transfers from the state government: cigarette license fee, gasoline tax, and statewide motor vehicle fee), sev-
"There are a couple of minor local revenue sources missing, namely estate tax revenue (variation of which is due solely to deaths) and zoning permit fees for municipalities, the utility service excise tax for counties, and the sale of government land for both (Ohio re-
vised code title III, V, and VII).

Table 1
Measure descriptive statistics.

<table>
<thead>
<tr>
<th>Level</th>
<th>Tax type</th>
<th>Total measures</th>
<th>Mean amount</th>
<th>Amount as % of total revenue</th>
<th>Median length</th>
<th>Passage rate</th>
<th>Mean vote share</th>
<th>Date range</th>
</tr>
</thead>
<tbody>
<tr>
<td>Municipal</td>
<td>Property</td>
<td>17,884</td>
<td>29</td>
<td>6%</td>
<td>5</td>
<td>81.9%</td>
<td>0.614</td>
<td>1990–2007</td>
</tr>
<tr>
<td></td>
<td>Income</td>
<td>1247</td>
<td>190</td>
<td>22%</td>
<td>–</td>
<td>41.1%</td>
<td>0.472</td>
<td>1983–2007</td>
</tr>
<tr>
<td>Schools</td>
<td>Bonds</td>
<td>1278</td>
<td>7871*</td>
<td>91%*</td>
<td>–</td>
<td>43.1%</td>
<td>0.489</td>
<td>1993–2007</td>
</tr>
<tr>
<td></td>
<td>Property</td>
<td>4105</td>
<td>744</td>
<td>9%</td>
<td>5</td>
<td>55.0%</td>
<td>0.513</td>
<td>1990–2007</td>
</tr>
<tr>
<td></td>
<td>Sales</td>
<td>2264</td>
<td>24</td>
<td>4%</td>
<td>5</td>
<td>66.7%</td>
<td>0.539</td>
<td>1990–2007</td>
</tr>
<tr>
<td></td>
<td></td>
<td>256</td>
<td>48</td>
<td>7%</td>
<td>–</td>
<td>34.7%</td>
<td>0.455</td>
<td>1983–2007</td>
</tr>
</tbody>
</table>

Notes: Mean amounts in 2006 real dollars and are per pupil for schools and per capita otherwise. They are all on an annual basis, except for school bonds [*], which are counted as a one-
time expenditure. “Amount as % of total revenue” represents the mean amount of a measure divided by the total revenue of the average jurisdiction (multiplied by 100) calculated from a sample of 2002 budgets. Data for the median length of income and sales tax measures is unavailable.

8 Since HB 920 was passed, jurisdictions have two renewal options for property tax measures. They can renew an expiring levy at the (frozen) amount of revenue which was raised when the levy was first passed or can replace the expiring levy with a measure that levies the same tax rate, but on the current assessed value of the property.
9 The reason the election data span different years is simply that the Secretary of State’s office could only successfully locate election reports in their archive for those years listed above. The results are robust to limiting the sample to 1993–2007, when data for all the measures are available.
10 There are also a few missing elections for certain measures during those time periods, most notably, the entire 1996 primary election for property measures.

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that this is contributing to any bias. The table presents estimates of whether measure passage crowds out other revenue sources; for jurisdictions that narrowly pass a tax increase relative to those that narrowly do not, the evidence indicates no statistically significant effects on other local revenue sources or state transfers to the government.

Appendix Table 2 displays descriptive statistics for the fiscal data by measure type which are in 2006 real dollars. Column (1) contains the mean and standard deviation for all jurisdiction-year observations in the data. Column (2) specifically examines only the jurisdictions holding at least one referendum. Column (3) focuses on their “neighbor” from the main specification, which are all jurisdictions within 10 miles for schools and municipalities and contiguous jurisdictions for counties. The few jurisdictions that did not propose a referendum have somewhat lower taxes and spending for municipalities and schools.

4. Estimation strategy

In this section, I describe my estimation strategy and several particular issues associated with its implementation. First, I explain my basic setup and discuss how a RD design isolates exogeneity. Second, I discuss the definitions of neighborliness that are constructed. Third, I detail the specific outcomes and specifications used in the analyses.

4.1. Basic setup and regression discontinuity design

I am interested in estimating the spillover effect of a jurisdiction’s taxes and spending on its neighbors’ fiscal decisions (whether neighborliness is defined by spatial proximity or some other classification). In general, in a regression of own and neighbor taxes, the coefficient on neighbor taxes could be biased in either direction. Factors that lead jurisdictions to have higher levels of taxation and spending might also be influencing their neighbors to do the same (e.g. a regional economic shock). On the other hand, factors that lead one jurisdiction to decrease taxation or spending might be causing an increase in their neighbors (e.g. a more localized economic shock that induces the tax base to move to neighboring areas). Further, if spillovers do exist, then the fiscal decisions of all jurisdictions are jointly determined. As a result, a correlation between the fiscal behavior of neighbors cannot be interpreted causally.

However, close elections can be used to overcome these endogeneity concerns. As long as there is some randomness in the vote share, the outcome of close elections provides quasi-random assignment of spending (Lee, 2008). The RD design exploits the fact that once controlling for differences in the number of votes with a polynomial of the vote share, measures that pass with slightly more than 50% of the vote relative to those that just miss 50% of the vote are approximately no different on average (due to the arbitrary cutoff of 50%) in terms of observables and unobservables.

I specify an econometric model that allows one to transparently examine the effect of a jurisdiction’s fiscal behavior on its neighbors. Throughout the analyses, the dependent variable is the mean outcome considering measure i, and ui represent all other determinants of spending. A naive regression of the following is likely to be biased.

\[ n_i = \alpha + b \beta + u_i \]  

Denoting vi as the vote share for measure i and assuming the conditional expectation of the unobserved component of neighbor spending given that the realized vote share (E[ui|vi]) is continuous, it can be approximated with a polynomial of the vote share with order g and coefficients \( \gamma_k (f_k(v_i; \gamma_k)) \), which becomes approximately accurate as g increases. Assuming this holds, the following regression will lead to a consistent estimate of measure passage on neighbor spending.

\[ n_i = \alpha + b \beta + f_g(v_i; \gamma_g) + u_i \]  

Because I am operating in a dynamic framework, both an intent to treat model (exogenously passing a measure and then leaving each jurisdiction the ability to pass subsequent measures if its voters so choose) as well as a treatment on the treated model (exogenously passing a measure and prohibiting voters from passing future ones) can be estimated. The distinction is important given that a jurisdiction that is narrowly below the threshold for voter approval is likely to try to pass the measure in a subsequent election. In the above regression (where an outcome is directly regressed on measure passage), the dynamics of jurisdictions that narrowly did not pass a measure and then pass subsequent measures will bias the estimated effect towards zero. On the other hand, any instrumental variable regression where measure passage is used as the instrument (e.g. if neighbor fiscal outcome is regressed on own fiscal outcome and the above regression represents the reduced form) will not be biased towards zero as the first stage will appropriately scale the second stage. Nonetheless, even in an instrumental variable context, the first stage and therefore the power of the analysis may be weakened by these dynamics.

To deal with these dynamics, a “recursive” estimator for treatment on the treated effects developed in Cellini et al. (2010) is estimated, which utilizes an estimate of the causal effect of measure passage on subsequent measure passage \( \pi \). Relating treatment on the treated (TOT) effects to intent to treat (ITT) effects by time period since the election (the subscript, \( \tau \)),

\[
\begin{align*}
B_0^{TOT} &= B_0^{ITT} \\
B_1^{TOT} &= B_1^{ITT} - \pi_1 B_0^{TOT} \\
B_2^{TOT} &= B_2^{ITT} - \pi_1 B_1^{TOT} - \pi_2 B_0^{TOT} \\
& \vdots \\
B_{\tau}^{TOT} &= B_\tau^{ITT} - \sum_{t=1}^{\tau-1} \pi_t B_{\tau-t}^{TOT} \quad \text{and generally}
\end{align*}
\]

I first estimate the intent to treat parameters as well as the causal effect on subsequent measure passage using the same intent to treat regression with number of measures passed as the dependent variable. I can then solve for the treatment on the treated coefficients using

\[ 16 \text{ The approach used here focuses explicitly on the RD experiment occurring at each jurisdiction considering a measure, but the specification parts ways with some of the previous literature, which typically regresses a jurisdiction’s outcome on its composite neighbor’s outcome. Using the composite neighbor specification, if the spillover effect is positive, the first stage will include the spillover effect on other neighboring jurisdictions, and depending on the preferred interpretation, could scale the second stage down by too much. If the spillover effect is zero, there may be no first stage. If the spillover effect is negative, then the first stage will scale down the second stage by too little and may even be of the wrong sign. The comparability of the composite approach to the mean and total effect in my specification is mediated by the number of total and shared neighbors for each neighbor and focal jurisdiction pair. Additional comparison of the approaches is discussed in footnote 41.} \\
17 \text{ The paper is subsequently referred to as CFR. While the “recursive” estimator can be used in the context of this paper, the CFR one-step estimator does not accommodate environments where a jurisdiction votes on more than one measure a year, which is not infrequent in Ohio.}
those results and the above set of equations. One limitation is that, relative to the ITT estimator, the TOT estimator can increase imprecision since the estimate at \( \tau \) relies on the full history of \( \tau \) and \( \delta^{\text{IT}} \) from 0 through \( \tau \).

4.2. Neighbor classification

There is no straightforward guide for how to identify the “right” neighbors. However, theory can to some degree inform their selection. Among the theories of spillovers discussed earlier, spatial proximity is particularly relevant as well as similarities to jurisdictions in other dimensions, while some explicitly focus on the mobility of businesses and residents. Consideration of the salience of and availability of information on other jurisdictions also supports the use of these criteria. Previous empirical studies have matched jurisdictions based on spatial proximity, similar demographic characteristics, and state residential mobility patterns.

The motivating idea behind the selection of neighbors is to maximize the possibility that if spillovers do exist, they would most strongly manifest themselves in my choice of neighbors. For the main analysis, I use all jurisdictions that are within 10 miles of the focal jurisdiction (i.e., the one holding the referendum) for municipalities and school districts, and all contiguous jurisdictions for counties.\(^{19}\) This selection procedure yields a seemingly reasonable number of spillover candidates which on average are 5 for schools, 12 for municipalities, and 5 for counties.

To address the possibility that spillovers might occur between jurisdictions close in other dimensions, especially vis-à-vis potential mobility concerns, I use an alternative neighbor index to check the robustness of the results. For counties, the straightforward candidate is the actual mobility patterns of individuals. I use the 1989 IRS County-to-County Migration data and include a county as a neighbor if the focal county serves as one of the top 5 within state recipients of its out-migrants. For municipalities and school districts, mobility data do not exist. I instead match jurisdictions that are closest on the 1990 Census education and income characteristics within a well-defined area that is likely to capture where most mobility that is relevant to local spillovers occurs. The natural catchment area for municipalities and school districts are census statistical areas which tend to have a relatively high population density at its core and close economic ties throughout the area. Ohio has 16 metropolitan statistical areas, 29 micropolitan statistical areas, and 19 rural counties that do not belong in any statistical area. The analysis is run both with and without those rural counties (which serve as their own catchment area).\(^{20}\) I match each focal jurisdiction to all jurisdictions for which it is one of the five closest within the catchment area in terms of percent college and per capita income.

4.3. Outcomes and specifications

I use a variety of fiscal outcome measures to examine spillovers, which all have their own strengths and weaknesses. The dependent variables used in the analyses are neighbors’ 1) total referenda revenue amount approved within five years, 2) tax rate or tax revenue (depending on what is available and specific to the type of tax that is being voted on in the focal jurisdiction), and 3) total expenditures or total revenue. The mean and standard deviation for each variable are listed in Appendix Table 2. All regressions are run separately by revenue source and government level, as there is no strong theoretical reason why spillovers need to be the same across measure type (though the estimates are sometimes pooled as well).

I first plot all the results by vote share, narrowing in around the cutoff for measure passage. In formal regression specifications, I follow CFR which has a similar context to this paper; their paper examines the effect of school bond referendum on housing prices. As CFR does, to improve precision, I exploit the panel nature of the data to control for jurisdiction level heterogeneity and retain all of the data in the sample but absorb non-close variation with flexible controls for the vote share. Standard errors across all specification are clustered at the level of the focal jurisdiction.\(^{21}\) The following is a discussion of each dependent variable along with their slightly different specifications separately.

The first outcome measure is the cumulative amount of revenue raised by neighboring jurisdictions through the referendum process. Using the universe of spending referenda, I calculate for each measure the per capita (or for school measures, per pupil) amount of annual revenue that should be raised (if the referendum data does not already include the amount). I then calculate the amount of revenue approved by election period for each referendum’s set of neighbors, which is denoted as amount. The main advantages of this outcome are that it is likely to be quite precisely estimated and is at the most disaggregated level possible (election by election) which is useful for estimating the treatment on the treated model. The main disadvantage is that even though referenda are the primary way to change taxing and spending levels, there are some exceptions as previously discussed.

Similarly to CFR, to estimate an ITT I take each ballot measure and stack data from the 8 election periods prior (which is equivalent to 2 years prior to the election) to 20 election periods subsequent to the focal election (equivalent to 5 years out) and estimate the following model:

\[
\text{amount}_{i,t,c} = \beta_0 + \beta_1\text{f}_t + \beta_2\text{type}_c + \beta_3\tau + \beta_4\tau + \delta^\text{IT} + \varepsilon_{i,t,c}
\]

where the new subscript consists of \( t \) denoting the election period and \( c \) denoting the tax change type if applicable (renewal, replacement, and increase). The window of analysis is through five years subsequent to the focal election in order to flexibly give neighbors sufficient time to respond and because over 80% of the measures expire within five years. While the prior empirical literature almost exclusively uses a static estimation framework which implicitly implies an immediate response of jurisdictions, this paper explores the possibility that it takes time for neighboring jurisdictions to respond to fiscal changes and can pinpoint the dynamics of the response (if it occurs).\(^{22}\)

The regression controls for election period effects (\( \text{election}_t \)), election periods relative to the focal election effects (\( \alpha_t \)), type of tax change effects (\( \text{type}_c \)), a cubic polynomial in the vote share (\( \text{f}_t(\text{vote}_t, \gamma) \)), and a three way interaction of the latter three.\(^{23}\) The coefficients on vote

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18 It is similar to the static “fuzzy” RD design whereby if subjects on the opposite side of the discontinuity are treated, the discontinuous indicator for measure passage can be used as an instrument for the realized treatment status to scale up the reduced form estimate. While the usual fuzzy strategy cannot be used here because measure passage in year \( t + 1 \) does not have the same effect on an outcome in \( t + 1 \) as a measure passed in \( t \), the “fuzzy” approach can be adapted to the case of dynamic treatment effects given that passage in year \( t + 1 \) should have the same effect on an outcome in \( t + 1 \) as passage in \( t \) in year \( t + \). Intuitively, Eq. (3) effectively scales up the ITT estimate by the degree to which measure passage decreases the likelihood for future measure passage in a way that accounts for the time specific way those future measures affect the outcome variable (by estimating the exact timing of the effects on subsequent measure approval). For example, the year 1 ITT estimate of measure passage is a function of the (unobserved) year 1 TOT estimate minus the decreased probability of a measure passage in year 1 times the effect of those new measures passed in year 1 (year 0 TOT effect). For a more in-depth discussion, see CFR (2010).

19 The 10 mile criterion is based on the distance of a straight line from one centroid to another. The results do not change when varying the distance used to define neighbors for municipalities and school districts.

20 Because this neighborhood definition as well as the main ten mile cutoff definition may not be appropriate neighbor classifications for jurisdictions in very rural counties (where there are few if any neighbors), in the Appendix, I also explore an alternative classification where it is based off of the five closest jurisdictions distance-wise.

21 The standard errors are similar when clustered on the county level.

22 In fact, the one paper that finds evidence of negative spillovers does so partially because the authors allow for lagged responses (Chirinko and Wilson, 2008).

23 I free up the fixed effects since they are not well suited to the referendum revenue outcome and only increase the standard errors; passage of a measure in one period makes a jurisdiction less likely to pass a measure in the following periods. The results are robust to their inclusion.
share and measure passage are restricted to 0 in the years preceding the election. Totaling up $\beta_\tau$ across the 20 election periods and then scaling the number down by the average estimated revenue generated from the focal referendum yields the ITT estimate.\textsuperscript{24}

To estimate a TOT model, I take the individual $\beta_\tau$ s from Eq. (4) along with the estimated $\pi_\tau$s and plug those values into Eq. (3). I then total the $\beta^{TOT}_\tau$s and again scale the result down by the average focal referendum’s estimated revenue. Standard errors are calculated by bootstrapping the entire estimation procedure.

The second and third outcomes have similar specifications but with different left-hand side variables and are on a yearly basis (where the subscript $t$ now denotes year). The second outcome is tax instrument specific: the neighbor property tax rate in millage for property tax measures, per pupil long term debt for school bond measures, and per capita income and sales revenue for income and sales tax measures respectively. The advantage of this measure is that it will also tend to have less noise than using total expenditures/revenue. The disadvantages are that the level of disaggregation is annual while elections can be held up to four times a year, and there could be a response from other revenue sources. The third outcome is per pupil total expenditures for neighbor schools and per capita total constructed revenues for neighbor municipalities and counties. The main disadvantage in addition to being on the annual level is that the data can be particularly noisy, including in ways that have nothing to do with the tax burden or generosity of

\textsuperscript{24} The estimated revenue generated by the focal referendum is calculated by replacing the dependent variable in Eq. (4) with the focal referendum’s calculated revenue if approved, and 0 otherwise, in the focal election period. This is practically equivalent to regressing neighbor referenda revenue approved on focal referenda revenue approved and instrumenting for the latter with measure approval but is computationally easier.
spending for the average resident or business (e.g. measurement error, changes in the number of special needs children, or for municipalities, population changes, which are problematic since the per capita numbers are calculated using the 2000 census numbers).

The ITT model is estimated using standard instrumental variable procedures. I take each ballot measure and stack data for 2 years prior to the election through 5 years subsequent to the election. In practice, some measure types exhibit less persistence, so I use as many years as possible before the strength of the first stage falls below conventional significance levels.25 Relating neighbor fiscal behavior to the focal jurisdiction’s own behavior

\[ \text{neighbor}_{i,t,c} = \text{own}_{i,t} + f_{3}(v_{i,t}, \gamma_{t,c}) + \text{type}_{c} \times \alpha_{c} + \text{vote}_{t} + \text{year}_{t} + \text{ballot}_{i,t} + e_{i,t,c} \]  

where \( \text{ballot}_{i,t} \) represents measures fixed effects, \( \text{year} \) represents year effects, and the coefficients on vote share is restricted to 0 in the years preceding the election. I then instrument for the focal jurisdiction’s fiscal behavior with a variable for focal measure passage which is also restricted to 0 preceding the election

\[ \text{own}_{i,t,c} = b_{i,t} \hat{o} + f_{3}(v_{i,t}, \gamma_{t,c}) + \text{type}_{c} \times \alpha_{c} + \text{vote}_{t} + \text{year}_{t} + \text{ballot}_{i,t} + e_{i,t,c} \]  

25 This issue specifically applies to sales tax measure. By almost an order of magnitude, there are far fewer sales tax measures considered by voters than any other measure type, which can sometimes present statistical power problems. The number of years used is highlighted for each measure type in Section 6. Further, the results are completely robust to using as many years as possible before the F statistic of the first stage falls below 10.

The TOT model is implemented somewhat differently.26 The neighbor TOT and own TOT are estimated separately and then the latter is used to scale the former. First, I estimate Eq. (6) for both neighbor and own but allow the \( \delta \) to vary by \( \gamma \), and then also use that equation to estimate the probability of subsequent measure passage \( \pi_{t} \). Second, I solve for the TOT effects using Eq. (3) and calculate their respective annual averages. Finally, I divide the TOT effect for the neighbors by the TOT effect for the focal jurisdiction.

5. Validation of research design

In this section, I test some of the assumptions of the regression discontinuity design. My empirical strategy is intended to approximate random assignment. As a result, anything that cannot be influenced by measure passage should be random to the treatment conditional on the vote share. To lend support to the validity of the regression discontinuity design, I take two different approaches.

First, I examine the distribution of the vote share. Any discontinuous changes in the density around the threshold for voter approval can be evidence of sorting that would violate the assumptions of a RD model (McCrary, 2008). Fig. 1 plots histograms of the measure

\[ \text{Spending/Revenue change}_{t-2 \text{ to } t-1} \]

\[ \text{Spending/Revenue change}_{t} \]

\[ \text{Spending/Revenue change}_{t} \]

\[ \text{Spending/Revenue change}_{t-1} \]
vote share, where the red vertical lines indicate the 50 percent threshold. Consistent with it being difficult to manipulate election outcomes, there does not appear to be any evidence of discontinuous changes around the threshold for voter approval in any of the panels.

Second, I exploit the panel nature of the dataset to test whether there are any differences in pre-election treatment and outcome variable means and trends conditional on the vote share. I examine, in both cross sections and changes over time, whether fiscal variables look similar for both focal jurisdictions and their neighbors in the years before the focal election (i.e. whether there is balance in the own and neighbor treatment and control groups). Because these variables are measured in the periods before the election is held, a regression discontinuity estimate of measure passage should yield no statistically significant effects.

Table 2 presents regressions of fiscal behavior of own jurisdictions and their neighbors in the years before the election. Column (1) tests whether there are any differences in the amount of revenue raised through referenda in the four elections preceding the focal election using Eq. (4). The results indicate no evidence of a correlation between treatment and pre-election revenue raised for any measure type. The remaining columns consider the pre-treatment balance for tax outcomes (columns 2 and 3) and spending/revenue outcomes (columns 4 and 5). Columns (2) and (4) look solely at the one year preceding the election while columns (3) and (5) take as the dependent variable the change in each outcome between two years and one year before the election. All of these regressions use the same control variables as in Eq. (5). Across all measure types and specifications, there is no evidence of a correlation between treatment and these pre-election fiscal variables. To summarize, out of the sixty regressions (although the tests should not be considered “independent”), no estimate is statistically significant at the 5% level or below. The evidence on net therefore supports the validity of the research design.

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**Fig. 2.** Neighbor and own tax specific outcomes: average through five years post-election. Notes: Graphs show the difference in the annual average tax in the five years subsequent to the focal election relative to the two years prior by the vote share for both neighbors and own. Units for the y axis are normalized property tax rate for property measures and per capita income for municipal income measures. Vertical lines indicate the threshold for measure approval. Dashed lines indicate 95% confidence intervals which are clustered at the jurisdiction level.
6. Graphical analysis

Before turning to the formal econometric analysis, it is useful to consider the evidence nonparametrically. Specifically, I examine whether there is any evidence of a discontinuous change in neighbor fiscal behavior around the 50% threshold for measure approval. All of the figures narrow in on a 40–60% bandwidth. I compute the average value of the dependent variable within two percentage point bins as defined by the vote share from its corresponding focal election, yielding a total of ten bins. The leftmost point in each graph represents outcomes associated with measures that received 40–42% of the vote (and hence failed by between 8 and 10 points), the next bin contains the average value of outcomes associated with measures that received 42–44% of the vote, and so forth. The values are all conditioned on time period, time period since focal election, and tax type changes if applicable. The purpose of doing this is to smooth out the graph with the aim of increasing the chances of detecting a discontinuity. However, the results are fully robust to not subtracting out these averages. 95% confidence intervals are included in the figures, which are clustered at the county level and constructed through bootstrapping.

Appendix Fig. 1 investigates the amount of revenue raised by neighboring jurisdictions within five years of referendum passage, as measured by the dollar figure of all referenda passed. Across all measure types, there is no sign of a discontinuity. The variability of the graphs also appears to be limited and hence quite precise.

Fig. 2 explores whether there are any discontinuous changes in the neighbor tax around the voter approval threshold after the election compared to before. Each value subsequent to the focal election is calculated by differencing out its value in the two years preceding the focal election. The figure corresponding to the change for own tax (the jurisdictions considering the referendum in the focal election, i.e. the first stage) is to the right. For the sake of clarity, the y axis is the same for each set of own and neighbor graphs. Fig. 2 displays the average annual tax change across the five years subsequent to the focal election. There
is no evidence of a decrease or increase in neighbor taxes at the threshold across all neighbor panels. Any noise in the graphs is completely dwarfed by the large discontinuous increase in tax for own jurisdictions. This contrast would be even starker when limiting the time frame to fewer years post-election. The discontinuity decreases with time because jurisdictions that experience a narrow failure subsequently propose and pass measures shortly thereafter, and because some of the measures themselves are expiring.

Finally, Fig. 3 investigates the average annual change in total revenue/spending by vote share. As with the preceding graphs, this figure compares the change in revenue/spending in the five years after the focal election relative to the years before. The graph of the own jurisdiction’s revenue/spending change is once more plotted to the right of the neighbor response. A large discontinuous jump is apparent for own jurisdictions and there is no evidence of a discontinuity for the neighbor’s reactions by the 50% threshold for any measure type. Again, while not included in the paper, focusing on fewer years after measure passage provides much clearer evidence of an even larger discontinuous increase in revenue/spending for own jurisdictions, and no evidence for neighbor jurisdictions whatsoever. While it admittedly weakens the effect (and discontinuity) for own jurisdictions because of dynamics and measures expiring, focusing through five years post-election is meant to allow for the possibility that the neighbor response lags significantly. However, there is no evidence that it does.

It is clear from the set of graphs that there is no jump for neighbor fiscal decisions at the threshold for measure approval, which is strong evidence of no spillovers. The benefit of a graphical analysis is that it is a more transparent and less restrictive way to test for discontinuous movements. However, to address the possibility that it is difficult to detect small effects with the graphs and to get confidence intervals from a formal hypothesis test, I now turn to the econometric analysis.

7. Results

Table 3 presents estimates of the effect of own referenda revenue approval in the focal election on the amount of neighbor referenda
revenue approved in the following five years. Column (1) displays the ITT analysis using Eq. (4). The TOT analysis using Eqs. (3) and (4) is displayed in column (2). The estimates are quite close to 0, no significant effects are found, they are all fairly precisely estimated with small standard errors, and pooling the estimates across tax instruments and level of government yields an insignificant effect of about \(-0.02\).

While the ITT and TOT estimates are similar, measure dynamics are occurring. Fig. 4 plots the number of measures passed by vote share in the two years subsequent to the focal election, again using two percentage point bins. Jurisdictions with measures that are just shy of the majority threshold are significantly more likely to pass a measure shortly thereafter. In Appendix Table 3, I examine the effect of measure passage on the five year cumulative number of measures passed beginning and subsequent to the election. While the TOT estimator holds constant the number of measures that a jurisdiction passes in the years after the election, the table indicates that from an intent to treat standpoint (so net of the election dynamics), measure passage still leads to a sizeable increase in the number of measures passed.

Table 4 presents the estimates of the effect of annual own tax outcomes on its neighbor tax outcomes where own tax is instrumented for by measure passage. The specifications use data through five years subsequent to the focal election and Eqs. (5) and (6) for ITT estimates and Eqs. (3), (5), and (6) for TOT estimates. The results indicate a strong first stage and no statistically significant coefficients in the second stage. The point estimates are all close to 0 or somewhat negative, with a pooled effect of \(-0.09\). Further, the confidence intervals are reasonably tight.

---

27 See Appendix Table 4a for the raw election period by election period estimates. While the estimates reported in the main tables come from a specification that cumulates through five years subsequent to the elections, neither immediate nor lagged effects are found in neighbors, which can be seen when examining the period by period or year by year raw ITT estimates in the appendix.

28 Meant only as an illustration of the effect, a two year window is shown because the majority of dynamics occur within that time period. Using the total amount of measure revenue approved instead of the total number of measures yields almost the exact same quantitative result.

29 See also Appendix Table 4b and c for the raw year by year estimates for tax outcomes and budgets.
Table 5 examines the effect of annual own budget on the budgets of its neighbors, where own budget is instrumented for by measure passage. Other than replacing the dependent and endogenous variables, the specifications are exactly the same as in Table 4. Municipal property, municipal income, school bonds, school property, and county property are all examined through five years post-focal election. The analysis of county sales measures uses a smaller window (three years out respectively) since the first stage does not persist as long. The results indicate a relatively large increase in revenues/spending in the focal jurisdiction.30 In line with the prior findings however, none of the spillover estimates are statistically significant.31 While the estimates are less precisely estimated as the tax specific outcome estimates, the point estimates are all nearly 0 or slightly negative, with a pooled magnitude of $−0.07.

School bonds fund durable goods and financed by debt, and I therefore investigate the long run effect of school bonds on neighbor spending. In Fig. 5, I plot by year through ten years out, the effect on cumulative neighbor expenditures scaled by the amount of an average school bond. If anything, the trend in neighbor school spending is modestly negative from years six through ten. However, a limitation is that the confidence interval relatively increases the longer the lag (e.g. examining ten years after measure passage).32 Additionally, while most measures have expired in the five years following their approval, I explore the extent to which the measures exhibit persistence in taxes and spending. Pooling across tax and jurisdiction type, Appendix Fig. 2 investigates the long run year by year first stage impacts of measure approval, and there is evidence of substantial persistence beyond the first five years. Consequently, I examine as a robustness check the longer run spillover effects of the non-school bond measures in Appendix Fig. 3, and similar null results are found.33

To explore the overall robustness of the results, I undertake several exercises. Table 6 limits the analysis to the measures that if passed would constitute the (ex ante) 25% largest of tax increases. By focusing on the largest measures, a null effect here is especially strong since they are the ones most expected to show evidence of spillovers, if they do in fact exist. Focusing on the largest of measures also addresses the possibility that the fiscal behavior enters into the neighbor response nonlinearly whereby small effects have no impact but larger ones do.34 As expected, the first stages are economically quite significant in terms of their effects on the budgets of the focal jurisdictions. As a percentage of the full budget, measure passage is equivalent to a five year long increase of 8% for municipal property measures, 22% for school bond measures, 15% for school property measures, and 5% for county property measures, and a two year 17% increase for municipal income measures and 7% increase for county sales measures.35 Just as with the full sample, all the estimates are statistically insignificant and either quite close to zero or somewhat negative.

The types of areas where interjurisdictional competition is theoretically most relevant are undoubtedly not sparsely populated ones. Focusing on all neighbors within a ten mile radius drops several percentage points of the sample, i.e. the very rural areas. Appendix Table 5 explores this issue further by dropping more regions. I first limit the analysis to municipalities and school districts that are located in combined statistical areas in column (1). Combined statistical areas include metropolitan and any adjacent micropolitan statistical areas that enjoy a moderate degree of employment interchange. I then limit the analysis solely to jurisdictions in metropolitan statistical areas in column (2). Finally, in column (3), I limit the analysis to the “suburbs” of metropolitan statistical areas, which are defined as all jurisdictions except for the center cities with a population above 75,000. These results are consistent with the full sample, with no specification rejecting the null hypothesis.36

Table 7 presents estimates using the alternative measures of neighborliness but with the exact same specifications from above. Counties are matched to their five closest counties based off of mobility patterns, and schools and municipalities are matched to their five closest counterparts in a census statistical area on the basis of education and income (the dependent variable is averaged across those two dimensions). No estimate is statistically significant, and most are similar to those found when using the main neighbor designations.37

Given the failure to find any evidence of spillovers and how it diverges with much of the previous literature, I provide some evidence in Table 8 that the lack of a result is not due to the specific data and institution being analyzed by investigating what happens when using the specification and identification strategy commonly used in the earlier literature.38 For each fiscal variable, I regress own jurisdiction on the average of the jurisdiction’s neighbors (based off of the main neighbor designation in the paper), own jurisdiction fixed effects, year effects, and own demographic covariate(s), instrumenting for the average neighbor with the average neighbor’s demographic covariate(s).39 Across the spending, revenue, and tax outcomes I find large positive and mostly statistically significant estimates of spillovers, which is in line with the results from the majority of the previous literature (prior

<table>
<thead>
<tr>
<th>Table 3</th>
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<tr>
<td>Effect of referenda revenue approval on neighbor referenda revenue approved in the five years subsequent to the election.</td>
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</table>

<table>
<thead>
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<th>(2)</th>
</tr>
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<tbody>
<tr>
<td><strong>ITT</strong></td>
<td><strong>TOT</strong></td>
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<td>(0.10)</td>
</tr>
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<td>Municipal income</td>
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</tr>
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<td>(0.07)</td>
</tr>
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<td>School bonds</td>
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</tr>
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<td>(0.04)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>School property</td>
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<td>(0.09)</td>
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<tr>
<td>County property</td>
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<tr>
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</tr>
<tr>
<td>County sales</td>
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<tr>
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<td>(0.22)</td>
</tr>
<tr>
<td>Pooled</td>
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</tr>
<tr>
<td>(0.02)</td>
<td>(0.07)</td>
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</tbody>
</table>

Notes: Each entry comes from a separate regression. The specification for column (1) is Eq. (4) with indicators for election period, indicators for election periods relative to the focal election, indicators for tax type (if applicable), a cubic in the vote share, a three way interaction of the previous three, and an indicator for measure passage. The specification for column (2) also utilizes Eq. (3). Sample sizes from top to bottom are 296,194, 23,190, 21,858, 70,781, 38,279, and 4952. Standard errors are in parentheses and clustered at the jurisdiction level. *5% level significance, ** 1% level significance.

30 The first stage yields a cumulative per person revenue (or per pupil spending) increase of $141 for municipal property tax measures, $362 for municipal income tax measures, $6314 for school bond, $2614 for school property, $99 for county property, and $89 for county sales. Note that local governments spend per capita much less than states or the federal government.

31 Estimates with the dependent and endogenous variables in logs instead of levels yields nearly identical (null) results.

32 Examining ten year lags throws out over two thirds of the sample.

33 Pooling the figures, as is done for the estimates in the main tables, would substantially lower the confidence interval.

34 Though to my knowledge, linearity is implicitly assumed in all empirical studies in the literature.

35 The full budgets for municipalities and counties are taken from an audit of the local government by the state government in 2002.

36 The few positive but still statistically insignificant coefficients are just noise; plots of them by vote share indicate no discontinuity.

37 In Appendix Table 6, I also explore whether any effects can be detected based off of a neighbor classification comprised of the five closest jurisdictions distance-wise (as some prior neighbor classifications may be inappropriate for rural municipalities or school districts). Again, no statistical significant evidence is found.

38 I am unable to use identifying variation from the papers that use “natural experiments” as they are specific to the data and institutions being analyzed.

39 For school districts and counties, per capita income is available every year and is used as the demographic covariate. For municipalities, demographic data is only available from the 1990 Census, the 2000 Census, and the 2005–2009 American Community Survey 5 year estimates and so only three years of fiscal data are used. The municipality demographic variables I use are per capita income, percent with a college degree, and percent with some college.
estimates approximately range from a high of 1.2 to a low of 0.2.\textsuperscript{40} Because the specification in this table is different from the RD specification, I transform the estimates from the table into comparable values, which are presented in Appendix Table 7.\textsuperscript{41} Pooling these estimates, I recover a spillover effect on spending/revenue of 0.23 and on tax specific outcomes of 0.28, and the null hypotheses that these estimates are the same as the RD estimates can both be rejected.\textsuperscript{42} One way to reconcile the neighbor characteristics estimates with the regression discontinuity estimates would be if the former suffer from omitted variable bias. While it is difficult to conclusively demonstrate this, I develop some suggestive evidence to that effect in Appendix Table 13 (the discussion of which is in Appendix 1).\textsuperscript{43} Finally, as indicated in Appendix Table 9, taking the normal approach in the paper (using measure passage as an instrument) but excluding controls for vote shares also yields generally positive and statistically significant estimates. However, these results cannot be interpreted as evidence of fiscal spillovers, as the discontinuity is no longer leveraged. Instead, they highlight the importance of the regression discontinuity design in generating approximate random variation in fiscal behavior.\textsuperscript{44}

While no evidence of spillovers is found, I turn to whether increases in taxes and spending influence future mobility and sorting in the focal jurisdictions in Appendix Table 10. For counties, I observe the number of individuals moving to and from each county as well as their income from the IRS migration data. Columns (1) and (3) examine the effect of measure passage on mobility, which is defined as the log number of individuals moving to and from the focal county, for county property and sales measures respectively. Columns (2) and (4) examine whether there is any evidence for sorting on income (the per capita income of counties, outer (inner) counties), which can be thought of as a proxy for public good demand. For schools, the only data that is available is the annual number of income tax filers and their dependents and the average per capita income of each school district from the Ohio Department of Taxation.\textsuperscript{45} Columns (5) and (7) consider the effect of measure passage on total log population and columns (6) and (8) consider the effect on log average income, both sets for school property and bond measures respectively. All the estimates are close to zero and statistically insignificant.\textsuperscript{46} As a final test, I examine in Appendix Table 11 whether enrollment changes in grades K-1st, 6th, and 9th in response to school measure passage (as mobility decisions based on school choice are arguably most likely to occur prior to entry of the child into school as well as during the transitions between elementary and middle school and middle school and high school). Again, no statistically significant effects are found. These analyses therefore suggest no resorting after exogenous changes in taxation and public good levels.

While no evidence of spillovers is found, it is still possible that elected officials might (unsuccessfully) attempt to increase taxes in response to neighbor tax increases. To shed more light on this issue, Appendix Table 12 examines the effect of referenda revenue approval on the amount of neighbor referenda revenue placed on the ballot in the five years following the focal election. Five of the six measure types are close to zero or negative and statistically insignificant. However, the coefficient for school bond measures is positive and statistically significant at the 5% level.\textsuperscript{47} Returning to the theoretical types of spillovers briefly discussed in the introduction to this paper, while most of the literature suggests that spillovers are positive in nature (tax and benefit competition, yardstick competition, and complementarity in public good consumption), there are several mechanisms that could lead to negative spillovers (substitutability in public good consumption and Tiebout resorting). The null effect finding permits all spillovers to be zero, or some to be nonzero but offsetting. However, different tests throughout the analysis call into question the applicability of different theories. Tests on mobility and sorting on income yielded no evidence of any Tiebout resorting (which itself suggests that the elasticity of mobility to spending and taxes is low). At least for school spending, it is implausible that individuals from nearby school districts consume their neighbor’s spending. Additionally, neighbor metrics based off of mobility patterns (or expected mobility patterns, i.e. matching neighbors in a census statistical area based off of demographics), revealed no effects, and consumptions spillovers are less likely to be operating based on these definitions of neighborliness relative to ones based off of physical proximity. Finally, in Appendix Table 13, I undertake a suggestive analysis that tests for effects of measure passage on the variance in the changes of fiscal variables (where the dependent variable is the variance of the pre- and post-periods). While I do not detect any mean changes in taxes or revenue/spending, the idea behind this analysis is that if it is the case that certain spillovers are more important in certain areas or in certain time periods but that they offset each other overall, effects on the variance should still be detected. Yet, as the table indicates, no significant effects are found. Taken together, the evidence suggests that none of the spillovers are present.\textsuperscript{48}

The previous empirical literature has mostly found evidence of positive spillovers. While this paper has not found such a result, the question of how large of a positive effect can be ruled out remains (though as aforementioned, the paper’s estimates statistically differ from estimates produced from replicating the main prior identification strategy as well as OLS estimates and estimates without vote share controls). Because the point estimates for each measure type are extremely

\textsuperscript{40} That is, among those that found positive and statistically significant results (though not all do). These results are robust to using the alternative neighbor designations from Table 7.

\textsuperscript{41} The transformation is done as follows and uses information on the total number of neighbors (nt) and the number of shared neighbors in each jurisdiction pair (ns, which is approximately a little under a half of nt) where iv refers to the composite neighbor representation (nt−ns) and the number of shared neighbors in each jurisdiction pair (ns, which is given by one (the jurisdiction that passed the measure passage on a focal jurisdiction’s characteristics). However, while average neighbor characteristics are always correlated with the individual jurisdiction’s characteristics, due to measurement error) and the null hypotheses that these estimates are the same as the RD estimates can both be rejected.\textsuperscript{42} One way to reconcile the neighbor characteristics estimates with the regression discontinuity estimates would be if the former suffer from omitted variable bias. While it is difficult to conclusively demonstrate this, I develop some suggestive evidence to that effect in Appendix Table 13 (the discussion of which is in Appendix 1).\textsuperscript{43} Finally, as indicated in Appendix Table 9, taking the normal approach in the paper (using measure passage as an instrument) but excluding controls for vote shares also yields generally positive and statistically significant estimates. However, these results cannot be interpreted as evidence of fiscal spillovers, as the discontinuity is no longer leveraged. Instead, they highlight the importance of the regression discontinuity design in generating approximate random variation in fiscal behavior.\textsuperscript{44}

\textsuperscript{42} Another way to make the estimates comparable is to switch the dependent and endogenous variables in Table 8, whereby a neighbor composite fiscal variable is regressed on the jurisdiction’s fiscal variable and the instrument for the jurisdiction’s fiscal variable is the jurisdiction’s characteristics. However, while average neighbor characteristics are always significantly correlated with average neighbor fiscal variables, individual jurisdiction’s characteristics are not always correlated with the individual jurisdiction’s fiscal variables (by definition, due to measurement error) and the first stage is therefore sometimes not significant. Nonetheless, carrying out this strategy (and ignoring the lack of a first stage) yields a pooled estimate for spending/revenue of 0.27 with a standard error of 0.08, which is very comparable to the approach used in the paper.

\textsuperscript{43} Another explanation that reconciles the RD and neighbor demographic approaches is that the local average treatment effects are different.

\textsuperscript{44} The hypothesis that the pooled RD estimates and the pooled estimates without the vote share controls can also be rejected at standard significance levels. Additionally, a regression that directly regresses neighbor fiscal outcomes on focal jurisdiction fiscal outcomes produces very similar positive and significant results. These results are in Appendix Table 8, and the estimates statistically differ from the RD estimates as well.

\textsuperscript{45} No good data on mobility or sorting for municipalities is available.

\textsuperscript{46} No significant effect on neighbor mobility or sorting is found either, which holds for both the main neighbor designation as well as the alternative neighbor designation from Table 7.

\textsuperscript{47} If one were to correct for multiple hypothesis testing given the tens of hypotheses tested, the coefficient would lose its statistical significance.

\textsuperscript{48} The analysis also rules out the case where there is a spillover effect on the tax base but no actual policy response.
Fig. 4. Measure dynamics. Notes: Graphs show the number of measures passed by the focal jurisdiction in the two years subsequent to the focal election by the vote share. Units for the y axis are the total number of measures passed. Vertical line indicates the threshold for voter approval. Dashed lines indicate 95% confidence intervals which clustered at the jurisdiction level.

Table 4
Impact of fiscal behavior on neighbor tax specific outcomes.

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<td>0.99**</td>
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<td></td>
<td></td>
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<td>(0.15)</td>
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</tr>
<tr>
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<td></td>
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<td>County property</td>
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<td>(0.21)</td>
<td>(0.09)</td>
</tr>
<tr>
<td>County sales</td>
<td>5</td>
<td>-0.22</td>
<td>14.11**</td>
<td>-0.20</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.16)</td>
<td>(5.02)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Pooled</td>
<td>-</td>
<td>-0.010</td>
<td>-</td>
<td>-0.09</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.07)</td>
<td>(0.06)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Each entry comes from a separate regression. The specification for columns (1)–(2) comes from Eqs. (5) and (6) with indicators for year, indicators for years relative to the focal election, indicators for tax type (if applicable), a cubic in the vote share, a three way interaction of the previous three, measure fixed effects, and an indicator for measure passage. The specification for columns (3)–(4) also utilizes Eq. (3). Sample sizes from top to bottom for all columns are 87,851, 8971, 8725, 27,997, 11,277, and 1878. Standard errors are in parentheses and clustered at the jurisdiction level. *5% level significance, ** 1% level significance.
Note also that very small spillovers cannot be ruled out. This is out of the general range of prior papers that call into question the theories that presume such spillovers. That they do not call into question the theories that presume such spillovers.

The analysis is subject to some limitations. The environment examined is limited to the local level and tax instruments covered in the paper. The effect of referendum passage on the efficacy enhancing effects is limited to the local level and tax instruments covered in the paper.

The results should not imply that local jurisdictions are never constrained or influenced by the fiscal behavior of their neighbors. It is quite possible that very small spillovers cannot be ruled out.

8. Discussion

A sizeable theoretical literature has examined the implications of fiscal spillovers across jurisdictions. This paper uses a regression discontinuity design and finds no evidence of spillovers for multiple local tax instruments, and this holds across a variety of robustness checks. The result suggests that any distortions (or efficiency enhancing effects) possibly generated by fiscal spillovers are small. Even in a world with high mobility costs, limited attention, and other adjustment costs, there still should be some marginal jurisdictions that would respond to neighbor fiscal changes if spillovers are a mechanism. That they do not call into question the theories that presume such spillovers.

The analysis is subject to some limitations. The environment examined is limited to the local level and tax instruments covered in the paper. The effect of referendum passage on the efficacy enhancing effects is limited to the local level and tax instruments covered in the paper.

The results should not imply that local jurisdictions are never constrained or influenced by the fiscal behavior of their neighbors. It is quite possible that very small spillovers cannot be ruled out.

Table 5
Impact of fiscal behavior on neighbor spending/revenues.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Municipal property</td>
<td>5</td>
<td>0.00</td>
<td>18.71**</td>
<td>−0.00</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.20)</td>
<td>(5.84)</td>
<td>(0.17)</td>
</tr>
<tr>
<td>Municipal income</td>
<td>5</td>
<td>−0.09</td>
<td>41.13*</td>
<td>−0.01</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.32)</td>
<td>(19.98)</td>
<td>(0.30)</td>
</tr>
<tr>
<td>School bonds</td>
<td>5</td>
<td>−0.09</td>
<td>515.04**</td>
<td>−0.07</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.26)</td>
<td>(174.41)</td>
<td>(0.25)</td>
</tr>
<tr>
<td>School property</td>
<td>5</td>
<td>−0.12</td>
<td>208.09*</td>
<td>−0.11</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.34)</td>
<td>(103.89)</td>
<td>(0.31)</td>
</tr>
<tr>
<td>County property</td>
<td>5</td>
<td>−0.13</td>
<td>10.34**</td>
<td>−0.12</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.15)</td>
<td>(4.41)</td>
<td>(0.19)</td>
</tr>
<tr>
<td>County sales</td>
<td>3</td>
<td>−0.25</td>
<td>16.60*</td>
<td>−0.13</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.22)</td>
<td>(6.39)</td>
<td>(0.20)</td>
</tr>
<tr>
<td>Pooled</td>
<td>–</td>
<td>−0.10</td>
<td>–</td>
<td>−0.07</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.10)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Each entry comes from a separate regression. The specification for columns (1)–(2) comes from Eqs. (4) and (5) with indicators for year, indicators for year relative to the focal election, indicators for tax type (if applicable), a cubic in the vote share, a three way interaction of the previous three, measure fixed effects, and an indicator for measure passage. The specification for columns (3)–(4) also utilizes Eq. (3). Sample sizes from top to bottom are 87,851, 8926, 8285, 25,131, 11,277, and 1878. Standard errors are in parentheses and clustered at the jurisdiction level. *5% level significance, ** 1% level significance.

Fig. 5. Long term effect of school bond passage on neighbor school spending. Notes: Graph shows the coefficients and 95% confidence interval for the effect of school bond passage on neighbor school spending scaled down by the estimated effect of an average school bond.
Table 6
Impact of fiscal behavior with the largest 25% of measures (TOT).

<table>
<thead>
<tr>
<th></th>
<th>Years post-election</th>
<th>(1) Tax</th>
<th>(2) Tax first stage</th>
<th>Years post election</th>
<th>(3) Spending/Revenue</th>
<th>(4) Spending/Revenue first stage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Municipal property</td>
<td>5</td>
<td>−0.00</td>
<td>1.97</td>
<td>5</td>
<td>−0.05</td>
<td>39.17*</td>
</tr>
<tr>
<td>Municipal income</td>
<td>1</td>
<td>−0.19</td>
<td>118.6*</td>
<td>2</td>
<td>−0.01</td>
<td>146.76*</td>
</tr>
<tr>
<td>School bonds</td>
<td>5</td>
<td>−0.05</td>
<td>8771.35**</td>
<td>5</td>
<td>−0.02</td>
<td>1914.29**</td>
</tr>
<tr>
<td>School property</td>
<td>5</td>
<td>−0.38</td>
<td>2.75**</td>
<td>5</td>
<td>−0.14</td>
<td>1320.97**</td>
</tr>
<tr>
<td>County property</td>
<td>5</td>
<td>0.01</td>
<td>1.43*</td>
<td>5</td>
<td>−0.19</td>
<td>27.66**</td>
</tr>
<tr>
<td>County sales</td>
<td>5</td>
<td>0.02</td>
<td>67.17**</td>
<td>2</td>
<td>−0.01</td>
<td>43.95*</td>
</tr>
<tr>
<td>Pooled</td>
<td>−</td>
<td>−0.10</td>
<td>−</td>
<td>−</td>
<td>−0.07</td>
<td>−</td>
</tr>
</tbody>
</table>

Notes: Each entry comes from a separate regression. The specification comes from Eqs. (3), (4), and (5) with indicators for year, indicators for years relative to the focal election, indicators for tax type (if applicable), a cubic in the vote share, a three way interaction of the previous three, measure fixed effects, and an indicator for measure passage. Sample sizes for columns 1 and 2 are 17,418, 2158, 2081, 6702, 2938, and 461 and for columns 3 and 4 are 17,418, 2158, 2081, 7315, 2938, and 461. Standard errors are in parentheses and clustered at the jurisdiction level. *5% level significance, ** 1% level significance.

Table 7
Fiscal spillovers using alternative neighbor definitions (TOT).

<table>
<thead>
<tr>
<th></th>
<th>(1) Referenda revenue</th>
<th>(2) Referenda revenue (excl. non census)</th>
<th>(3) Tax outcome</th>
<th>(4) Tax outcome (excl. non census)</th>
<th>(5) Spending/Revenue</th>
<th>(6) Spending/Revenue (excl. non census)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Municipal property</td>
<td>0.03</td>
<td>0.02</td>
<td>0.01</td>
<td>0.02</td>
<td>−0.05</td>
<td>−0.08</td>
</tr>
<tr>
<td>Municipal income</td>
<td>−0.11</td>
<td>−0.11</td>
<td>−0.04</td>
<td>0.02</td>
<td>0.01</td>
<td>0.11</td>
</tr>
<tr>
<td>School bonds</td>
<td>0.02</td>
<td>0.04</td>
<td>−0.04</td>
<td>−0.04</td>
<td>−0.07</td>
<td>−0.04</td>
</tr>
<tr>
<td>School property</td>
<td>0.03</td>
<td>−0.02</td>
<td>0.00</td>
<td>0.01</td>
<td>−0.64</td>
<td>−0.48</td>
</tr>
<tr>
<td>County property</td>
<td>−0.10</td>
<td>−0.18</td>
<td>−0.18</td>
<td>−0.22</td>
<td>−</td>
<td>−</td>
</tr>
<tr>
<td>County sales</td>
<td>0.01</td>
<td>−0.08</td>
<td>−</td>
<td>−0.13</td>
<td>−</td>
<td>−</td>
</tr>
</tbody>
</table>

Notes: Each entry comes from a separate regression. The 19 rural counties that do not belong in any statistical area are excluded from columns (2), (4), and (6). The specification for columns (1)–(2) comes from Eqs. (3) and (4) with indicators for election period, indicators for election periods relative to the focal election, indicators for tax type (if applicable), a cubic in the vote share, a three way interaction of the previous three, and an indicator for measure passage. The specification for columns (3)–(6) comes from Eqs. (3), (4), and (5) with indicators for year, indicators for years relative to the focal election, indicators for tax type (if applicable), a cubic in the vote share, a three way interaction of the previous three, measure fixed effects, and an indicator for measure passage. Sample sizes for column 1 are 288,641, 22,662, 22,219, 69,273, 38,279, and 4944, column 2 are 240,405, 21,325, 20,241, and 21,760. Standard errors clustered at the jurisdiction level are in parentheses. *5% level significance, ** 1% level significance.

large and persistent changes in taxes and benefits could have spillover effects. The state variables that provoke responses may be limited to near permanent features of neighbors.52

However, Ferriera and Gyourko, 2009 show that there is no fiscal impact in cities from having a Democratic versus Republican mayor. They argue that this result is most consistent with competition from nearby jurisdictions. The evidence provided here suggests that at sensible policy option margins, local jurisdictions do not appear to be influenced by their neighbors, which does not support the existence of such competition. And the results are identical when using only the largest measures. Ultimately, theories that presume spillovers may identify real mechanisms, but the extent to which they are empirically relevant or apply to realistic policy parameters may be limited.

While there is little reason to believe that the environment in Ohio is the exception to the rule and that the theoretical spillover channels operate in different contexts from this paper, yardstick competition is an exception, which raises the issue of how far the results generalize to representative democracies. The role of elected officials is important to the conventional story of yardstick competition (which arises to correct an asymmetry of information between voters and politicians), and the specific institutions in Ohio prevent politicians for the most part from directly determining the level of taxes and spending. On the other hand, there are guards to expect the results to be similar. In a yardstick model in this environment, voters may still interpret neighbors’ behavior as a signal of the potential benefits of tax and spending changes. Further, local officials through their role as agenda setters in direct democracy settings may have some power to influence tax and spending levels (Meredith, 2009).53 More broadly understanding the differences

52 However, it is important to note that the prior empirical literature focuses almost exclusively on short run (yearly) fiscal changes brought about by annual changes in the instrument(s). While in principle these year to year changes could still lead to somewhat permanent fiscal changes in the home jurisdictions, evidence from the replication of the conventional approach presented in Table 8 indicates that the first stage fiscal changes brought out by the instrument(s) do not appear to persist for more than a few years. On the other hand, the mode tax measure in this analysis lasts for five years (with approximately 20% of measures having no expiration date and the time frame for school bonds also longer since they finance durable goods and are funded by debt). Further, evidence presented in Section 7 indicates that the pooled first stage impacts persist beyond that time frame.

53 And in fact, the one paper in the previous literature that specifically compared (school) fiscal spillovers for direct versus representative democracies (Reback, 2009) found a larger effect for direct democracies. Note that the agenda-setting power of local officials is larger in the theoretical environment of Romer and Rosenthal (1979) relative to in Ohio.
between representative and direct democracies should be the subject of future work.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2013.12.005.

References


Table 8

<table>
<thead>
<tr>
<th></th>
<th>Spending/Revenue</th>
<th>Pooled tax specific outcomes</th>
<th>Property tax</th>
<th>Income tax</th>
<th>Sales tax</th>
<th>Long term debt</th>
</tr>
</thead>
<tbody>
<tr>
<td>Municipalities</td>
<td>0.89**</td>
<td></td>
<td>1.50**</td>
<td>0.86**</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.15)</td>
<td></td>
<td>(0.38)</td>
<td>(0.14)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Schools</td>
<td>0.82*</td>
<td></td>
<td>1.04**</td>
<td>–</td>
<td>–</td>
<td>0.37</td>
</tr>
<tr>
<td></td>
<td>(0.35)</td>
<td></td>
<td>(0.09)</td>
<td></td>
<td></td>
<td>(0.19)</td>
</tr>
<tr>
<td>Counties</td>
<td>0.83*</td>
<td></td>
<td>0.33</td>
<td>–</td>
<td>0.36</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.41)</td>
<td></td>
<td>(1.09)</td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Pooled</td>
<td>0.85**</td>
<td></td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.19)</td>
<td></td>
<td>(0.20)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Each entry comes from a separate regression. The specification contains jurisdiction fixed effects, year effects, characteristics, and the average neighbor outcome, which is instrumented for by neighbor characteristics: annual per capita income for schools and counties, and percent college, percent some college, and per capita income from 1990, 2000, and 2005–2009 estimate for municipalities. Sample sizes for municipalities are 5831, for schools are 11,105 for the property tax and 10,046 for spending/debt, and for counties are 1408. Standard errors clustered at the jurisdiction level in parentheses. *5% level significance, ** 1% level significance.