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The elusive fiscal commons: Examining fiscal interaction among overlapping governments

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Abstract

Vertically overlapping governments share the same tax base, which may generate fiscal spillovers in tax, spending, or debt decisions. Various studies have tested the fiscal commons effect in this context, though the findings remain inconclusive. This paper uses a regression discontinuity design to address empirical challenges in fiscal spillover research. We focus on bond referendums of Texas cities, counties, school districts, and water districts and identify the effect of exogenous increases in debt and property taxes induced by successful bond measures. We find no evidence of spillovers for bond measures, election results, property taxes, or overall revenue, spending, and indebtedness. Successful bond measures do not affect the shared property tax base, which suggests that the benefit view of property tax may explain the lack of vertical spillover.

Key Takeaways

- Vertically overlapping governments share the same tax base, which may generate fiscal spillovers in tax, spending, or debt decisions.
- This could potentially lead to excessive fiscal expansion, known as the fiscal commons effect.
- We focus on the bond referendums of Texas cities, counties, school districts, and water districts and identify the effect of exogenous increases in debt and property taxes induced by successful bond measures.
- Following property tax increases induced by successful bond measures, overlapping governments do not change their tax efforts, spending levels, or debt issuances.
- In sum, we find little support for vertical fiscal interaction.

INTRODUCTION

Overlapping governments are common in many countries. Overlapping governments could be hierarchically related as seen in a federal-state-local structure or result from service specialization, where on the same administrative level, special-purpose governments (e.g., water districts, school districts, and fire districts) provide a single public good while taxing the same base as the general purpose governments (e.g., cities and towns).[1] Among the 89,000 local governments in the United States, close to 13,000 school districts provide primary and secondary education services and 37,000 special districts offer specialized public goods (Maciag, 2019). They overlap, but are not necessarily coterminous, with general-purpose localities such as counties and municipalities.

When multiple fiscally autonomous governments operate within the same region, how their decisions affect one another is a question of academic and policy interest (Goodman, 2019). The literature has focused on two types of interactions among governments. Horizontal fiscal interactions describe how the fiscal decisions by one government affect other governments on the same level. For example, the extensive tax competition literature shows that neighboring governments strategically set tax rates to attract mobile tax bases, which leads to the race-to-the-bottom phenomenon of under-taxation; the yardstick competition literature shows governments to mimic the revenue and spending decisions of one another to improve perceived legitimacy by voters (Besley & Case, 1995; Wilson, 1999). The smaller vertical fiscal interactions, on the other hand, focus on overlapping governments taxing and serving the same area and the interactions therewithin.

Using a regression discontinuity (RD) design, we examine the vertical fiscal interactions among Texas local governments as induced by passing bond measures. We find little evidence of vertical interaction. While winning localities are unsurprisingly more likely to issue additional general obligation (GO) debt and increase property tax efforts, their overlapping local governments do not respond by changing bond proposals, bond issuance/retirement decisions, or property tax efforts. These null effects may be due to the constraining effects of the voter-approval requirements. However, when examining bonds not subject to voter approval and other sources of government revenue for which local governments have more discretion over, we find that overlappers of winning localities do not change their overall revenue, spending, and indebtedness.

This paper contributes to the fiscal interaction literature in several ways. First, we advance the empirical literature on vertical interactions by providing a quasi-experimental estimate. Scholars have tested the vertical interactions among overlapping governments regarding tax, spending, and debt (Brien & Yan, 2020; Goodman & Carroll, 2024; Greer et al., 2018; Wu & Hendrick, 2009). Yet, as is common in observational studies, endogeneity remains a concern. Joining Choi (2022), we offer a causal test of vertical fiscal interactions.

Second, we examine vertical interaction in the context of direct democracy, where voters approve government borrowing, and consequentially, tax decisions. The theoretical work in Keen (1998) and empirical work in Brülhart and Jametti (2006) show that compared to leviathan governments, benevolent lower-level governments, such as those operating in a direct democracy, are more likely to increase tax rates following an increase by the higher-level government. However, it is not clear if the same conclusion will hold for overlapping localities not in an administrative hierarchy. For example, when the public goods provided by two coterminous or largely overlapping localities are substitutes, a benevolent government

[1]Two overlapping governments can co-occupy the same geographic area without co-occupying the same tax base if they tax different economic activities (Hoyt, 2017). For example, while local governments in the United States rely heavily on property tax, this tax is not a major revenue source for state governments. Local governments examined in this study all rely on property taxes and thus do share tax bases.

concerned with welfare rather than just revenue may lower taxes in response to the other's tax and spending increases. Our finding of a null effect of vertical tax interaction among overlapping governments calls for revisiting the theories of vertical interaction.

Finally, this study advances our understanding of the special-purpose or special-function governments that are prevalent in the United States. While the public choice school argues that special-purpose governments spur local government competition and increase the number of public service bundles available, others postulate that they constitute a source of wasteful duplication and may lead to diseconomies of scale and overexpansion of the local public sector (Berry, 2008; Luca & Modrego, 2021). Focusing on cities, counties, school districts, and water districts in Texas, we find no evidence of vertical fiscal interaction and thus provide no support for the fiscal commons concern.

BACKGROUND

This study focuses on fiscally autonomous local governments in Texas, which consist of three types. County governments have no "home rule" authority, meaning that all their powers are specifically granted by the state. There are 254 counties in Texas, the largest number of any state. Areas within a county that are incorporated become cities, which means that counties always serve a geographic area larger than that of the municipalities within the county. Texas has more than 1200 incorporated cities. Cities with a population over 5000 may elect home rule status, enabling them to take actions not specifically restricted by the state. Otherwise, a city maintains general law status and has limited powers. The last type of local government is special districts, providing not a package of general government services but a single type of public good. The special district boundaries often do not align with county or city boundaries. The most common type of special district is school districts that provide elementary and secondary education. Almost all of the more than 1200 school districts are independent of city or county jurisdictions.[2] This study also includes about 1900 special districts working on water and waste management (later referred to as water districts for short), and many of them frequently issue bonds to finance capital projects. These districts provide services ranging from flood control, waste treatment, and freshwater supply, to sewage services.[3]

Like in many other states, Texas statutes provide explicit authority for local governments to issue bonds to finance capital projects. Not all bonds require voter approval. Tax-supported GO bonds are backed by a pledge of property taxes levied within the issuer's boundaries and generally must be approved by voters. The only GO bond that does not require a referendum is the certificate of obligation for emergency capital needs, which is backed by property taxes but does not require a referendum. Additionally, school districts can issue maintenance and operations (M&O) tax-supported debt for operation but not construction or renovation; voters approve the maximum tax rate for M&O debt, and once approved, M&O debt may be issued without an election. In contrast to GO bonds, revenue bonds are supported by the revenue streams generated from capital projects or earmarked special taxes and do not require a referendum.

Local governments in Texas may order an election at least 78 days before the election. Most elections occur on a uniform election date in May or November, but exceptions abound, especially among special districts. The ballot generally includes information regarding the purpose and principal amount of the proposed bond.[4] The elections require a simple majority

[2]The only exception is the Stafford Municipal School District which is controlled by the city of Stafford.

[3]Other special districts that issue bonds include college districts, economic development districts, hospital districts, and transportation districts. We exclude these special districts from our analysis because they collectively account for less than 5% of the total debt proposed in bond referendums and because there is no consistent mapping of their boundaries.

for approval. Once approved, the state attorney general's office reviews and approves the bond and the authorization proceedings for legal compliance. The local governments could then issue the bond on the municipal bond market but face no mandatory issuance schedule. Local governments may issue less than what was approved or forego bond issuance completely. For instance, data from the Texas Bond Review Board (TBRB) show that among city governments, about $2.2 billion in bond authority were approved by voters between 2001 and 2010, but only $1.5 billion have been issued as of 2020.

Property taxes are the most significant source of taxation for local governments in Texas. According to the 2017 Census of Government Finance, 83% of local government tax revenue in Texas comes from property taxes and 16% comes from local sales taxes.[5] The overall property tax rate is composed of two parts: the debt service tax rate, which is affected by bond referendums, and the operations tax rate.[6] The operations tax rate does not require voter approval and is set by the local governments on an annual basis. However, it is subject to two restrictions. First, the state constitution set the maximum property tax rate at $2.5 per $100 taxable value for home rule cities, and depending on the population, at $0.25 to $2.5 per $100 taxable value for general law cities. Second, if the operations tax levy increases by more than 8% over the previous year's level, a certain percentage of registered voters in the jurisdiction can request a rollback election to seek tax reductions.[7] The most recent FY2019 property tax data show that only about 4% of localities are at risk of a rollback election, while most have ample space for a rate increase if deemed necessary by the local governing body.

THEORY AND EMPIRICAL STRATEGY

Literature Review and Theoretical Expectations

The effect of one government's tax increase on the fiscal decision of an overlapping government is theoretically ambiguous. On the one hand, a fiscal commons effect may be present, where each government ignores how its tax increases shrink the shared tax base and thus generates negative externalities onto overlapping governments (Flowers, 1988; Keen & Kotsogiannis, 2002). The result of the vertical externality is excessively high taxes. However, the direction of the relationship also depends on whether the governments are leviathan or benevolent, and on the use of the tax. Keen (1998) shows that with a leviathan state government, when the price elasticity of the demand for taxed goods is constant, the state tax rate increases with the federal rate increases, but the opposite is true with a linear demand curve. The case is further complicated considering the spending effect of a benevolent government. For example, the contraction in the shared tax base due to federal tax increases means a reduction in state public goods, which increases the marginal valuation of such goods and renders raising state rates more attractive. Related, Turnbull and Djoundourian (1993) show that when the services of overlapping governments are substitutes, the expansion effect at one level

[4]For example, the city of El Paso's Proposition A from November 2019 reads the following: "The issuance of $413,122,650 general obligation bonds for public safety facilities including police department and fire department motor vehicles and equipment and that taxes sufficient to pay the principal of and interest on the bonds will be imposed." Voter turnout for local bond elections is generally low in the US (Cai, 2020; Devine, 2022). But literature shows that voter approval may be affected by the information provided on the ballot, such as tax wording (Brunner et al., 2018, 2021), as opposed to by voter turnout (Gong & Rogers, 2014).

[5]Cities, counties, and some special-purpose districts (not including water districts) can collect sales taxes not to exceed 2%. Once they are approved to levy the tax, the rate-setting that follows does not require voter approval. As of 2020, more than 40% of cities contain areas within which the maximum sales tax rate has been reached.

[6]The operations tax rate is formally referred to as the "maintenance and operations tax rate." It is different from the tax to support school M&O debt mentioned earlier because not all maintenance and operation are financed by debt.

[7]A law enacted on June 12, 2019, placed further restrictions, barring local governments from raising operations property tax levies by more than 3.5% above the previous year's level without an election.

is offset by the demand contraction effect at the other level; but complementary public services provided by overlapping governments will lead to an expansionary public sector as a whole. The ambiguity in the theoretical relationship highlights the importance of empirical research on this topic. The empirical literature on vertical fiscal interactions has focused on three dimensions: tax, expenditure, and debt, which largely generate inconclusive findings. Empirical tests on tax interactions among overlapping governments have shown mixed results. Besley and Rosen (1998) find that the state excise rates on gasoline and cigarettes respond positively to federal rate increases. A similarly positive effect has been found in the case of personal income taxation in the United States (Esteller-Moré & Solé-Ollé, 2001) and Canada (Esteller-Moré & Solé-Ollé, 2002), as well as property taxes of counties and independent schools (Brien, 2018). In contrast, Goodspeed (2000) reports a negative relationship between federal income tax rates and local income tax revenues across OECD countries. The negative correlation between high- and lower-level government tax rates has also been found regarding corporate taxes (Hayashi & Boadway, 2001), cigarette taxes (Fredriksson & Mamun, 2008), and local sales taxes (Agrawal, 2016). Observing that municipal property tax rates negatively correlate with county rates but positively with school district rates, Wu and Hendrick (2009) infer municipal and county services to be substitutes but school and municipal services to be complements. Still, Leprince et al. (2007) find no evidence of business tax interactions between departments and upper-tier regions in France.

On the expenditure side, while some studies find that public services provided by overlapping governments complement each other, others find them to be substitutes. Campbell (2004) reports that municipal and county expenditures are complementary. Turnbull and Djoundourian (1993) provide evidence for complementary relationships between county and city general expenditures but not for road and police expenditures. By contrast, Goodman and Carroll (2024) find that special district spending substitutes county government spending, mainly for public services covering large land areas, including fire protection, sewerage, and solid waste management.

Similarly, empirical evidence for the fiscal commons effect concerning government debt remains mixed. Jimenez (2015) show that overlapping local governments are associated with larger sizes of own-source revenues and debt. Greer et al. (2018) find that the number of special districts in a county increases the total local public debt. Shi and Hendrick (2020) find that jurisdictional overlap between state and local governments increases total state and local debt. In addition to the stock of debt, Greer (2015) find that total debt issued by overlapping governments increases the true interest cost paid by county governments, suggesting a market response to the fiscal commons effect. On the other hand, Martell (2007) finds that jurisdictional overlap is negatively correlated with local debt. Brien and Yan (2020) find that the shared property tax base between county and school district only has a weak and positive effect on municipal government GO bond issuance.

The main empirical challenge is the endogeneity of fiscal policies. Strategic rate setting means that tax efforts are determined concurrently and endogenously by overlapping governments. In particular, as overlapping governments are exposed to the same set of factors affecting their fiscal decisions, not controlling for these factors exhaustively could lead to findings of spurious correlations. For example, overlapping governments share the same voters, and the voters' preference for the level and mix of public goods may commonly affect all governments. The empirical literature largely relies on using government fixed effects and instrumental variable regression to mitigate endogeneity concerns.[8] As one exception, Choi (2022) uses an RD design focusing on bond referendums of school districts in Texas and finds

[8]For example, Besley and Rosen (1998) and Fredriksson and Mamun (2008) instrument for federal excise tax rates by federal deficit. Esteller-Moré and Solé-Ollé (2001) and Esteller-Moré and Solé-Ollé (2002) instrument for federal income tax rates by the share of the US population over 65 years and a binary variable for Democrat presidents.

that a school district's bond referendum success leads to higher property tax rates for debt services among overlapping cities. Given that local officials cannot perfectly manipulate the results of bond referendums, comparing fiscal changes of the overlapping governments of a winning locality and those of a losing locality provides causal evidence of vertical interactions.

Like Choi (2022), we leverage bond referendums in Texas with an RD design but expand in several ways, leading to differing results.[9] First, we expand the scope of the empirical study to cover a comprehensive set of overlapping local governments in Texas to gain a full picture of vertical fiscal interaction. While Choi (2022) focuses on school district referendums (74% of all), we include bond elections of all Texas local governments, including cities, counties, school districts, and water districts. We examine not only a city's response but all overlapping localities' responses. Covering multiple government types is important, given that prior studies have found differing results across types of governments and areas of public services (Goodman & Carroll, 2024; Turnbull & Djoundourian, 1993).

Second, Choi (2022) tests whether passing a referendum causes changes in an overlapper's tax rate, which may be considered a reduced form result but does not provide a direct measure of vertical interaction. We estimate how an average overlapper's tax effort changes with regard to a rate increase by the focal locality winning an election. Third, we study the effective tax rates, that is, the property tax levy as a share of the market value of a property, while Choi (2022) examines the statutory tax rates. As Bocci et al. (2019) point out, the effective tax rate measures the actual tax burden and is more policy-relevant. It also captures any fiscal responses through property assessment, as the assessment power rests within a county. Lastly, since Cellini et al. (2010), scholars have recognized the importance of accounting for the dynamic effect of elections, that is, a government is more likely to hold additional elections after losing one. Choi (2022) estimates only the intent-to-treat (ITT) effects that do not account for election dynamics; our paper additionally estimates the treatment-on-the-treated (TOT) effects.

In sum, to the extent that a fiscal commons effect exists, passing a bond referendum by one local government will increase its overlapping governments' tax, spending, and debt. However, empirical literature does not consistently support the fiscal commons effect and theoretical prediction is ambiguous (Keen, 1998; Turnbull & Djoundourian, 1993). Thus, the direction and magnitude of the vertical fiscal interaction are ultimately an empirical question. We now turn to our empirical strategy.

Basic setup of RD

The fiscal decisions of one local government are likely to be endogenous to that of the overlappers. For example, if one regresses own tax rates on that of the overlapping jurisdictions, the coefficient on the overlapper rate could be biased upwards if both governments increase tax efforts in response to a regional economic shock. To address the endogeneity concerns, we employ an RD design as the main identification strategy. The RD design has been widely applied in the context of local voter referendums in various states, including California, Michigan, Ohio, and Texas (Barr & Dee, 2016; Cellini et al., 2010; Isen, 2014; Martorell et al., 2016).

Causal identification in an RD design hinges on the assumption that treatment is assigned on one side of a cutoff in a running variable but not on the other side, and the assignment of treatments is "as good as random" near the cutoff (Lee & Lemieux, 2010). In the case of bond elections, the vote share in support of the measure serves as the running variable and the cutoff for treatment status (i.e., being approved for bond issuance) is 50%. Though studies show

[9]We attempted to replicate Choi's (2022) analyses but were unable to achieve the same results. This is likely due to different ways of addressing outliers in the property tax rate data. Choi (2022) does not provide details regarding the data-cleaning process.

evidence of local officials influencing the setup of elections to maximize chances of passage (Barr & Dee, 2016), the RD succeeds in causal identification if they cannot precisely calibrate the vote share within a sufficiently small band around the threshold for passage. Therefore, measures that pass with slightly more than 50% of the vote relative to those that just miss the threshold will on average be comparable in terms of observables and unobservables.

A "focal" local government is the one making a policy change, in our case, holding a bond referendum that increases property tax effort. We then observe the outcome among the overlapping governments to identify any overlapper responses to the focal locality's policy change.[10] We model the impact of passing bond measures by a "focal" locality on the fiscal responses of all of its overlapping governments. For an overlapper outcome y observed τ years after an election was held in focal locality j in year t, we estimate the following:

$$y_{j,t+\tau} = \theta_\tau w_{j,t} + f(v_{j,t}, \lambda_\tau) + u_{j,t+\tau}, \quad (1)$$

where $w_{j,t}$ is an indicator of whether the local government wins the election. Assuming that the conditional expectation of the unobserved component of y given the vote share v_{jt} is continuous, one can approximate the unobserved component with f, a polynomial function of the vote share with coefficient λ. The approximation accuracy of the function increases with the polynomial order. The specification also allows the parameters of the polynomial function to differ depending on the length of time between the election and the outcome (as captured by the subscript τ on λ), and the effect of measure passage to have different effects depending on the length of time after the election (the subscript τ on θ); when $\tau < 0$, θ_τ and λ_τ are restricted to zero. As a result, the coefficient θ_τ will consistently estimate the effect of measure passage on overlapper outcomes. We use a cubic function of the vote share in the baseline estimation but test its robustness to other polynomial orders.

A downside of this "global" RD approach is that the polynomial approximation may deliver a poor fit at boundary points and lead to unreliable RD estimates. We choose the global RD approach as the baseline analysis for two reasons. First, local RD approaches focusing on the sample close to the cutoff do not allow us to, as explained later, account for election dynamics and estimate the TOT effects. Second, local RD approaches greatly limit the sample size and the precision at which we can reject a null effect. Nevertheless, as a robustness check, we also report the local point estimates based on the MSE-optimal bandwidth as suggested by Calonico et al. (2019).

In a "dynamic" RD setting, failed bond measures can be re-proposed and passed in subsequent years. In other words, some observations in the control group would eventually be treated. One may be interested in the impact of approving a bond measure with the possibility that subsequent bond measures may be voted on, i.e. the "intent to treat" (ITT) effect. Alternatively, one can focus on the effect of exogenously passing a bond measure and prohibiting the focal locality from approving bonds in any subsequent year. This is the "treatment on the treated" (TOT) effect. Denoting the marginal effect of passing a measure on approving future elections in the focal locality by π, Cellini et al. (2010) show that the ITT effect is different from the TOT effect by the cumulative product of the TOT effect and π. That is, if a focal locality is less likely to win later measures after an earlier measure is approved ($\pi < 0$), which we show later, and if the TOT effect of vertical spillover is positive, then we expect that the ITT estimates will be too conservative for estimating the true interaction effect.

[10]Isen (2014) uses a similar strategy to define a focal locality and its neighboring governments when studying horizontal fiscal interactions.

Intent to treat specification

Cellini et al. (2010) show that Equation (1) is not efficient because the error term $u_{j,t+\tau}$ contains a component that varies at the focal locality level but is fixed within the locality over time. Following their approach for more precise effect estimates, we create a block of locality-year observations around each focal election within the time window from $t-2$ to $t+5$, and then stack the blocks together.[11] For example, if a focal locality held an election in 2005, all observations of overlapper outcomes from 2003 to 2010 will form a block; if the same focal locality held an election again in 2009, we stack to the earlier block the observations from 2007 to 2014. As a result, when a local government has multiple elections in close succession, the same year of overlapper observation is used more than once in the final stacked panel data. We estimate the following regression using this stacked data set:

$$y_{j,t+\tau} = \theta_{\tau}^{ITT} w_{j,t} + f(v_{j,t}, \lambda_{\tau}) + \alpha_{\tau} + \delta_{t} + \gamma_{jt} + e_{j,t+\tau}, \tag{2}$$

where comparing to the earlier equation we add in α_{τ}, δ_{t}, and γ_{jt} representing respectively the fixed effects for years relative to the election, for years, and for focal elections. The precision of θ_{τ} likely improves with γ_{jt} absorbing any across-locality variations. Even though vote share does not vary within each focal election, it is possible to control for the election-specific fixed effects because the coefficients on election passage and on vote share are allowed to vary based on the relative year to an election but are set to zero for the pre-election period. Robust standard errors are clustered at the focal locality level.[12]

Treatment on the treated specification

We applied the "one-step" estimator developed by Cellini et al. (2010) to estimate the TOT effects.[13]

$$y_{jt} = \sum_{\tau=0}^{\tau=10} \left(\theta_{\tau}^{TOT} w_{j,t-\tau} + \varphi_{\tau} h_{j,t-\tau} + f(v_{j,t-\tau}, \lambda_{\tau}) \right) + \alpha_{t} + \gamma_{j} + e_{jt}, \tag{3}$$

where $w_{j,t-\tau}$ indicates bond passage at time t ($\tau = 0$) and in each prior year up to $\tau = 10$. The indicator variable $h_{j,t-\tau}$ represents whether the focal locality j holds a bond election in year $t-\tau$. As before, $f(v_{j,t-\tau}, \lambda_{\tau})$ is a polynomial function of the vote share from the election in that year. If no election occurred in year $t-\tau$, the vote share variable is set to zero. We choose $\tau = 10$ to balance two goals: first, to control for as long of a bond election history as possible, and second, to avoid the losses of too many observations by taking lags of the bond election variables. To test how long of an election history is sufficient, we estimate Equation (2) using the focal locality's self-outcomes as the dependent variable. Results as graphed in Appendix Figure D3 show that the impact of a successful focal election on focal jurisdiction's future bond elections fades out within 10 years.[14] Finally, α_{t} and γ_{j} indicate year and focal locality fixed effects. The

[11]The window of analysis is chosen to cover 5 years after the focal election to give overlappers sufficient time to respond. Additionally, a government's fiscal policy might be constrained by the terms in which the policymakers are in office, and the common term lengths for Texas local officials are 4 years or less.

[12]Findings are robust to double clustering by focal locality and by election year. Available upon request.

[13]Cellini et al. (2010) also suggest a recursive estimator that is subject to less stringent assumptions but is also less precise. Our findings are robust to the recursive estimates, which are available upon request.

[14]To examine the self impact of measure passage on the focal government's future bond measures, we estimate the ITT regression following Equation (2) with the focal jurisdiction's self outcome as the dependent variable. If the focal jurisdiction is less likely to hold and pass future bond elections after winning a measure, the ITT estimates for the self-effects, which reflect the cumulative effects of past

equation is estimated on a standard focal locality-year panel. Robust standard errors are clustered at the focal locality level.

In a nutshell, the TOT estimator controls for the influence of earlier bond measures on later measures of the focal jurisdiction by including the election and vote share history. The θ_{τ}^{TOT} will be a consistent TOT estimate if such controls are sufficient, that is, if the effects of measure approval on later referendums and outcomes depend only on the time elapsed since the focal election and not on the time at which the focal election occurred or on the treatment history.

DATA

Data sources

GO bond election data are from the TBRB. The data cover the date, proposed bond amount, and result of elections from 1997 to 2019. The TBRB also maintains data on outstanding bonds from 2003 to 2019, that is, GO and nonGO bonds issued by local governments that have not been paid by the end of a given fiscal year. Data that further breaks GO bonds down by whether they are subject to voter approval is not available until 2007. Property tax data covering 1999 to 2019 are from the Texas Comptroller's Office, providing information on the market and taxable values of real properties in each jurisdiction, the total levy, and the statutory rates for operations and debt-service property taxes in each fiscal year. In Texas, the county-level appraisal offices assess the value of properties within each county. Per the Texas Property Tax Code, all properties must be valued at 100% of market value but the taxable value may be lower due to exemptions and abatements granted by state laws and by local governments. Finally, the Census Survey of Government Finances provides data on local government revenue, including property taxes but also other sources of tax and nontax revenues. The data further contain information on the total and subcategories of spending, as well as total outstanding debt that includes but is not limited to bonded debt. The Census conducts a survey every 5 years and samples local governments for the rest of the years. We interpolate the Survey of Government Finances data to fill in for localities not covered in survey years.

To empirically examine interactions in fiscal outcomes, we adjust for different government sizes and measure the "fiscal effort." The alternative is to focus on the statutory tax rate, which is the ratio between the property tax levy and taxable value. Local governments have discretion over granting property tax exemptions, which change the taxable value. That is, localities may collect more property tax by redefining the tax base without increasing statutory tax rates. Using statutory tax rates as the outcome would miss such fiscal responses, but using the ratio between the property tax levy and market value, that is, the effective tax rate, will not. Moreover, no statutory rates are readily available for measuring the outstanding debt burden. We scale all outcome variables of dollar amounts—including requested and approved bond amounts in elections, bond outstanding, property tax, and overall tax, spending, and debt—by the market value of properties in each jurisdiction.[15] Essentially, we measure each local government's fiscal effort based on their ability to pay as proxied by the market value of properties within the jurisdiction (Hildreth & Miller, 2002). We also show later that successful bond

measures, will be larger in magnitude than the TOT estimates. Therefore, examining how long the ITT self-effects last helps us control for as long of an election history as needed.

[15]Some jurisdictions exhibit large swings in the market value. Some of these may be due to data entry errors; others occur in water districts in areas of new developments. Developers petition to form water districts, which issue public debt to finance local utility investments before much of the real estate has been constructed or sold. To mitigate the impact of outliers, we set these fiscal variables to missing for all observations associated with local governments where the property market values more than double from 1 year to another. This affects less than 5% of cities, counties, and school districts, but roughly one-third of water districts.

measures do not change the market value of properties within a jurisdiction. Water districts require special consideration in this scaling process. Many were established through efforts of developers in areas with a nascent property base but large infrastructure needs. As a result, they exhibit very high borrowing and taxing efforts after scaling by the small property market value, which becomes outliers that could heavily affect our estimates. Therefore, we exclude water districts in the baseline analyses but report results based on water district-inclusive data in the appendix.

Panel A of Table 1 presents the summary statistics of the panel of local governments in Texas, instead of focusing on only the localities that had bond measures. The unconditional probability of any given local government to hold a bond election in a year is highest among school districts at 11.5% and lowest among cities at 1.89%. The unconditional average requested amount in bond elections is very high for water districts, showing again the uniqueness of the areas served by some water districts and small tax bases within. Besides water districts, school districts represent the largest source of GO bond liabilities for properties subject to the taxation of overlapping localities: a property owner bears on average $2.2 in school district debt for $100 in the market value of the property, and the numbers are respectively $1.99 and $0.24 for city and county debt. Cities, on the other hand, have higher nonGO bond liabilities. School districts also have the highest operations property tax effective rate at 7.46 mills, followed by 3.79 mills for cities, 3.21 mills for counties, and 2.29 mills for water districts. Operations property tax rates dwarf debt-service property tax rates for all types of localities except for water districts, which are capital-investment heavy. On average, city governments collect $3.26 and school districts collect $2.59 in revenue per $100 of property base. Notice that these numbers are much larger than the combined property tax revenue discussed earlier because they include other types of taxes, fees, and charges, intergovernmental transfers, and other revenues. In contrast, the total debt numbers are very close to the sum of GO bond and nonGO bond outstanding, indicating that most of Texas local governments' debt is bonded debt.

Bond election

The election data contain 3648 elections by 1118 localities during the 23-year period. Among the total, 1674 are simultaneous measures held on the same day by the same locality for different purposes. For example, a local government may put on the ballot one bond measure for building a civic center and another measure for street maintenance, effectively holding two elections at the same time. In contrast, a locality is much less likely to call for multiple elections on different dates during the same year (145 out of 2499 locality-years).

We need to identify focal elections, that is, the election of a focal locality whose success may affect overlapping government outcomes, based on which the RD design can be applied. The first challenge in identifying such a focal election is that the fiscal outcomes, such as bond outstanding, property taxes, and total spending, are measured at the end of each fiscal year and localities in Texas have different fiscal years. In contrast, bond elections can occur throughout the year. We identify one focal election within each calendar year so that any fiscal responses from overlappers should not be expected until the fiscal year that follows the calendar year, regardless of the overlapping governments' fiscal year schedules.[16] For bond election-related outcomes, we focus on overlappers' elections during the 12 months after the focal election. Second, we need to identify only one focal election among multiple measures occurring during the same calendar year for the same focal locality. The selection must balance the

[16]For example, if a focal election occurs in May of 2010, no matter what the fiscal year calendars look like for the overlapping localities, the fiscal outcome from the end of the fiscal year 2011 and thereafter reflect the overlapping governments' responses after the focal election.

need to uncover potential effects, such as those caused by important bond measures of large proposed amounts, and the potential of local officials in manipulating the timing of elections to improve the probability of measure passage (Hong & Zimmer, 2016; Martorell et al., 2016). We focus on the first election during the calendar year with the largest amount, rendering 2424 focal elections. Because most multi-election cases are due to having more than one measure on the same ballot as opposed to holding elections on different days during the same year, our approach of identifying focal elections essentially focuses on large measures. As shown in Appendix Figure D1, the density of the vote share is relatively smooth at the 50% threshold. A formal test based on McCrary (2008) shows that the estimated discontinuity at the threshold is 0.07 with a standard error of 0.13, and another test based on Cattaneo et al. (2018) generates a *p*-value of 0.41. Both tests fail to reject that the density is continuous at the cutoff.

Table 2 presents the descriptive statistics for the focal elections, including 375 measures from cities, 117 from counties, 1790 from school districts, and 136 from water districts. Voters were more likely to approve measures from cities and water districts, and the average approval rate across all elections was 80%. The average vote share in support of the measures ranges from 59.4% to 66%. School districts had the highest average measure amount of $54.63 million in the year-2000 dollars, while cities had the lowest at $24.44 million. This is in line with our expectation that cities tend to be smaller and thus have less capital investment needs. After scaling by property market value, water districts however show an average borrowing effort higher than that of the school districts, which is then higher than the values for cities and counties. As explained earlier, this is likely due to the small tax bases of water districts that serve newly-developed or pre-development communities.

Measuring overlapping governments

We use geospatial data to measure overlap among jurisdictions. Specifically, 2015 general-purpose government boundary files are from the Texas State Department of Transportation. The Texas Education Agency provides the 2017 boundary files of school districts, and the Texas Commission on Environmental Quality provides the 2017 files on water districts.[17] We overlay the boundary shapefiles of local governments on one another and calculate the area of overlap. For each focal locality where a focal election occurred, we calculate the share the overlapping area represents among the total geographic area of an overlapping locality. For the main analysis, we consider a local government an overlapper if more than 50% of its jurisdiction is in the overlapping area with the focal locality, as the focal election is likely to affect a majority of property owners within its jurisdiction under the assumption that taxable properties are spread evenly across the geographic area.[18] We later test the sensitivity of this threshold by identifying overlappers based on two extreme definitions: localities that are coterminous with or completely within the focal locality (a "complete overlap" threshold), or localities that have any area of overlap with the focal locality (an "any overlap" threshold). Appendix A illustrates the different overlapper definitions using an example.

Panel B of Table 1 reports the baseline-scenario average number of overlappers by government type, with the median reported in the parentheses to mitigate the influence of outliers. As expected, due to the smaller sizes, cities are less likely to have any overlapping local

[17]The shapefiles are from recent years. It is possible that governments' boundaries may change over time or a local government could start/cease to exist due to annexations or consolidations. However, it is difficult to obtain boundary files updated on an annual basis, especially for special districts. In addition, boundary changes are likely to affect the magnitude of overlap between two jurisdictions on the intensive margin but not on the extensive margin of whether two localities are overlappers. The later robustness checks based on different thresholds for the overlap definition may also mitigate concerns related to boundary changes.

[18]Real properties are of course not evenly distributed across geographic areas. Lacking any comprehensive data on development density, we proxy for tax base overlap using area overlap. This problem effectively disappears when we examine complete overlappers.

TABLE 1 Descriptive statistics of outcome variables and overlapping relations.

	City	County	ISD	WD
Panel A: mean of outcome variables				
Has election	0.0189	0.0240	0.115	0.0213
Win election	0.0163	0.0197	0.0924	0.0206
Requested amount	0.0266	0.0129	0.299	1.699
Approved amount	0.0204	0.0103	0.227	1.696
GO bond outstanding	1.991	0.243	2.201	3.552
NonGO bond outstanding	0.517	0.0992	0.0491	0.342
Operations property tax	0.379	0.321	0.746	0.229
Debt service property tax	0.0871	0.0222	0.105	0.287
Total revenue	3.258	0.640	2.592	1.413
Total spending	3.020	0.674	2.678	1.197
Total debt	2.683	0.286	2.046	4.047
Panel B: number of overlappers				
No. overlapper city	–	4.752	1.392	7.207
	–	(3)	(1)	(1)
No. overlapper county	0	–	0.137	1.077
	(0)	–	(0)	(0)
No. overlapper ISD	0.475	3.984	–	5.244
	(0)	(3)	–	(1)
No. overlapper WD	2.468	7.378	2.069	–
	(1)	(1)	(0)	–

Note: ISD stands for independent school district. WD stands for water district. All variables of dollar amounts are measured on the per-$100-market-value basis, that is, scaled by the market value of properties located in each jurisdiction. Debt outstanding (2003–2019) and bond election (1997–2019) data are from Texas Bond Review Board. The first four variables represent the average unconditional election-related outcomes; most local governments do not hold a bond election in a given year. "GO bond outstanding" represents the average amount of general obligation bond outstanding per $100 market value. Property tax data are from Texas Comptroller's Office, covering years 1999 through 2019. Numbers in panel B represent the average number, or median number in parentheses, of each type of overlapping jurisdictions for a type of focal jurisdiction. Overlapping jurisdictions have at least half of its area located within the focal jurisdiction.

TABLE 2 Focal bond election descriptive statistics.

	City	County	ISD	WD
Passage rate	0.843	0.786	0.773	0.882
Mean vote share	0.660	0.594	0.611	0.659
Mean amount ($million)	24.44	51.91	54.63	38.76
Mean amount-to-market value	0.0087	0.0048	0.0232	0.0958

governments potentially affected by the cities' bond measures. On the other hand, counties cover large geographic areas and at the median have 3 overlapping cities, 3 overlapping school districts, and 1 overlapping water district. A median school district has 1 overlapping city. Water districts encompass a wide range of entities, from small municipal utility districts to large regional water recreational districts. Their sizes vary greatly. As a result, the average and median numbers of overlappers for water districts differ significantly. A median water district has 1 overlapping city and 1 overlapping school district.

GRAPHICAL ANALYSES

To examine the spillover effect of a focal election on overlapping localities' fiscal decisions, we focus on the mean outcome across all overlapping jurisdictions as the dependent variable.[19] We specifically examine four sets of outcome variables. The first set focuses on how passing a focal election affects overlapping jurisdictions' referendum proposals and results. This includes whether any of the overlappers hold a bond election, the average requested amount, and the average approved amount, all measured within the calendar year after the focal election date (and the years thereafter). Second, we examine the average GO and nonGO debt outstanding of overlapping localities, which reflect the net effects of debt competition that incorporate both new debt issuance and old debt retirement. As it takes time to actually issue a bond on the municipal market and some localities never issue a bond that has been approved in a referendum, these outcome variables capture actual bond issuance responses which may lag behind bond referendum responses. Third, we investigate property tax responses by examining the operations tax and the debt service tax. If overlapping governments interact with each other only on the dimension of debt and capital spending, then one would observe an effect on debt service tax but not operations tax. On the other hand, local governments may be more likely to respond through changing operations tax efforts as it is not subject to the same voter approval requirements as GO bonds and the resulting debt-service tax. Lastly, to the extent that the local budget is fully fungible, we examine the overall revenue, spending, and indebtedness of the average overlapping jurisdiction. Appendix B shows that before a focal election, local characteristics are balanced between treatment (passed election) and control (failed election) groups, which supports the validity of the RD design.

[19]Alternatively, a total effect can be inferred by scaling the estimates up by the average number of overlappers reported in Table 1. The total effect may be more relevant if the overlappers also overlap with each other, while the average effect is appropriate when overlappers largely cover different areas within the focal jurisdiction.

Figure 1 presents graphical analyses of the differences in each outcome variable between the 5 years after the focal election and 2 years prior, by the margin of victory. We show the average outcomes, conditional on year-fixed effects, in two-percentage-point vote share bins and present the 10 bins representing the 40% to 60% window.[20] For example, the bin of 50 represents focal elections that receive between 48% and 50% of the vote in favor of the measure, that is, narrowly failing to pass. We normalize the other estimates to this bin. The vertical lines represent the 95% confidence interval, clustered at the focal locality level. The left panels show the outcomes in the focal locality while the right panels show outcomes from the average overlapping locality. For ease of comparison, the *y* axis is the same for each set of own and overlapper graphs. These figures show nonparametrically and unconditionally whether there is a discontinuous change in own and overlapper fiscal behavior around the 50% threshold for measure approval.

Large discontinuous jumps are observed for focal local governments that win bond measures. For example, Figure 1a shows that the focal locality has a lower probability of proposing a bond measure after winning a focal election. This could reflect that capital needs have been met by the approved measure and the focal locality is less likely to have additional needs in the subsequent 5 years. Similarly, the amount requested in bond referendums and the amount approved also decrease following a successful measure, as shown in Figure 1c,e. Taken together, the own referendum responses show evidence for the expected bond measure dynamics. Local governments with measures that are just shy of the approval threshold are significantly more likely to propose and pass another measure shortly after than those with approved measures. The finding sheds light on the importance of a dynamic RD design to distinguish between the TOT and ITT effects.

Figure 1g shows that new bonds are indeed issued within 5 years after bond measures were approved in an election. Localities that have won a focal election increase their GO bond outstanding after the approval, relative to the changes in debt outstanding experienced by localities with failing measures. This aligns with our expectations because most GO bonds require voter approval. There is no evidence that localities with failing measures compensate by issuing bonds that do not require voter approval, as Figure 1i shows no statistically significant difference between failing and successful localities in terms of the change in nonGO bond outstanding. As expected, the increased GO bond outstanding translates into a higher property tax effort for debt services, as shown in Figure 1k.[21] We do not observe any differential changes in the operations tax rate for localities winning bond measures as compared to those with failing measures. This suggests no fungibility in local government's property tax revenue, as debt service taxes are legally restricted to paying back principal and interest on GO debt, and no evidence for an income effect due to approved bond measures.

In contrast to the own responses, there is no evidence of a discontinuous change in the average overlapping local government's fiscal decisions by the 50% threshold. Most of the estimates for overlappers are also more precisely estimated than the self effects. Overall, the graphical analyses provide no evidence of vertical spillovers. We present in the next section

[20]That is, each subfigure is estimated based on the following regression: $\Delta y = \lambda_b + \theta_t + e$ where Δy is the change in outcome between the 5 years after the focal election and 2 years prior, λ_b are indicators for each two-percentage-point vote share bin with the 48–50% bin being the omitted group, and θ_t is the year fixed effects.

[21]The figure shows that the changes in the property tax rate for debt services are different for the bins representing a vote share between 40% and 44% than that for 48%–50%. When we separately examine the pre- and post-election rates instead of the change, this is shown to be due to districts relatively reducing their property tax effort after elections with a vote share between 40% and 44%. This is curious because the other three bins for failed elections do not show a statistically significant difference from the 48%–50% bin. Given that, as shown in the other self-effect figures, districts with a vote share between 40% and 44% do not appear to differentially change future referendum results or bond outstanding levels, this could arise out of chance. It also does not invalidate the identification strategy given the control for the polynomial function of the vote share.

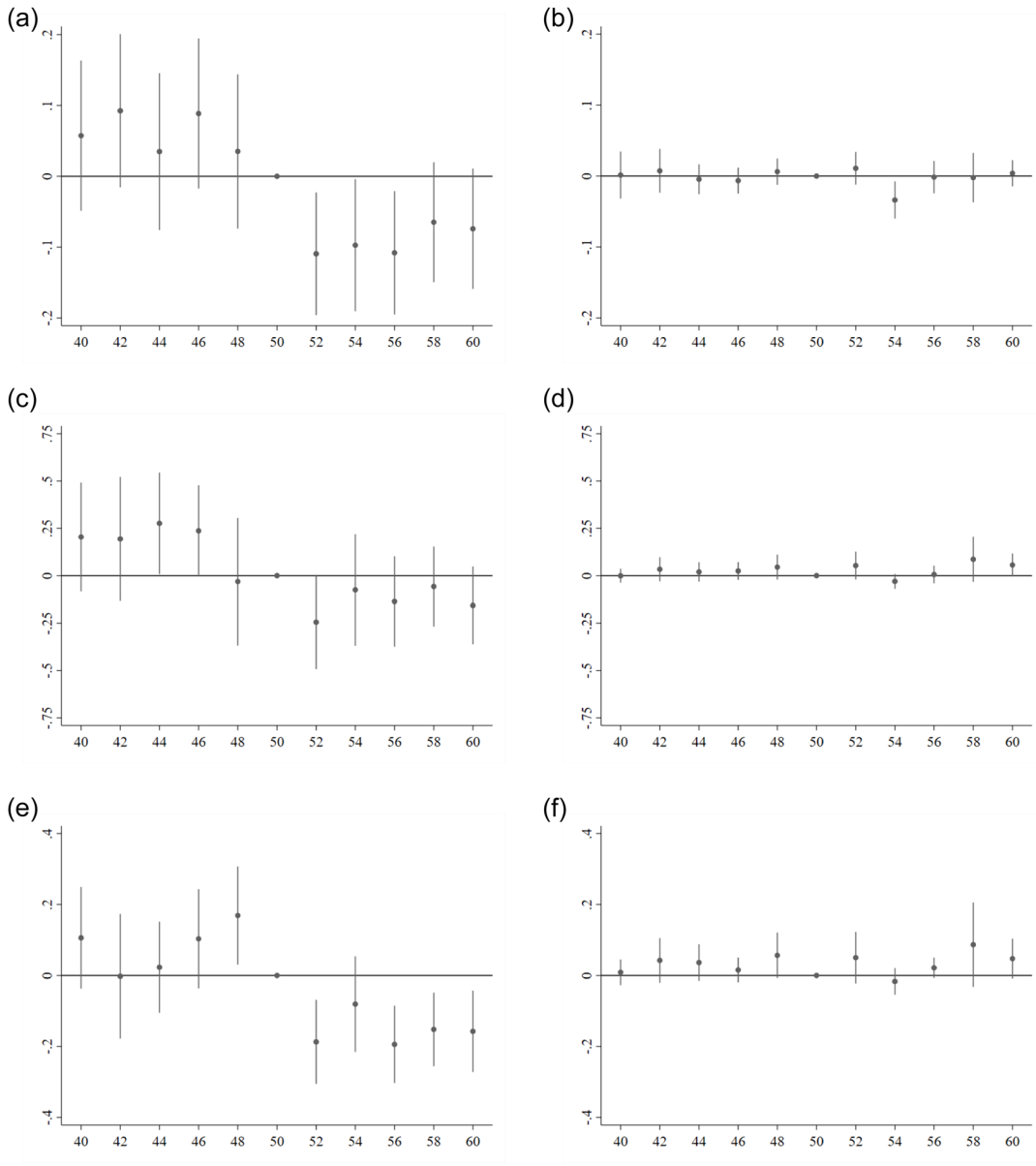

FIGURE 1 Pre- and post-election difference in outcomes, by vote share. Each figure is from a separate regression. Outcomes from the focal locality are on the left and overlappers on the right. Each point shows the difference in outcome 5 years after the focal election relative to the 2 years prior, by the vote share of the focal election (*x*-axis). Vertical lines indicate 95% confidence intervals clustered at the focal locality level. "Has election" is a binary variable; all other outcomes are dollar amounts scaled by the total property value (market value-based assessment value) in the jurisdiction. (a) Has election (own), (b) has election (overlapper), (c) requested amount (own), (d) requested amount (overlapper), (e) win amount (own), (f) win amount (overlapper), (g) GO bond outstanding (own), (h) GO bond outstanding (overlapper), (i) NonGO bond outstanding (own), (j) NonGO bond outstanding (overlapper), (k) debt service property tax (own), (l) debt service property tax (overlapper), (m) operations property tax (own), and (n) operations property tax (overlapper).

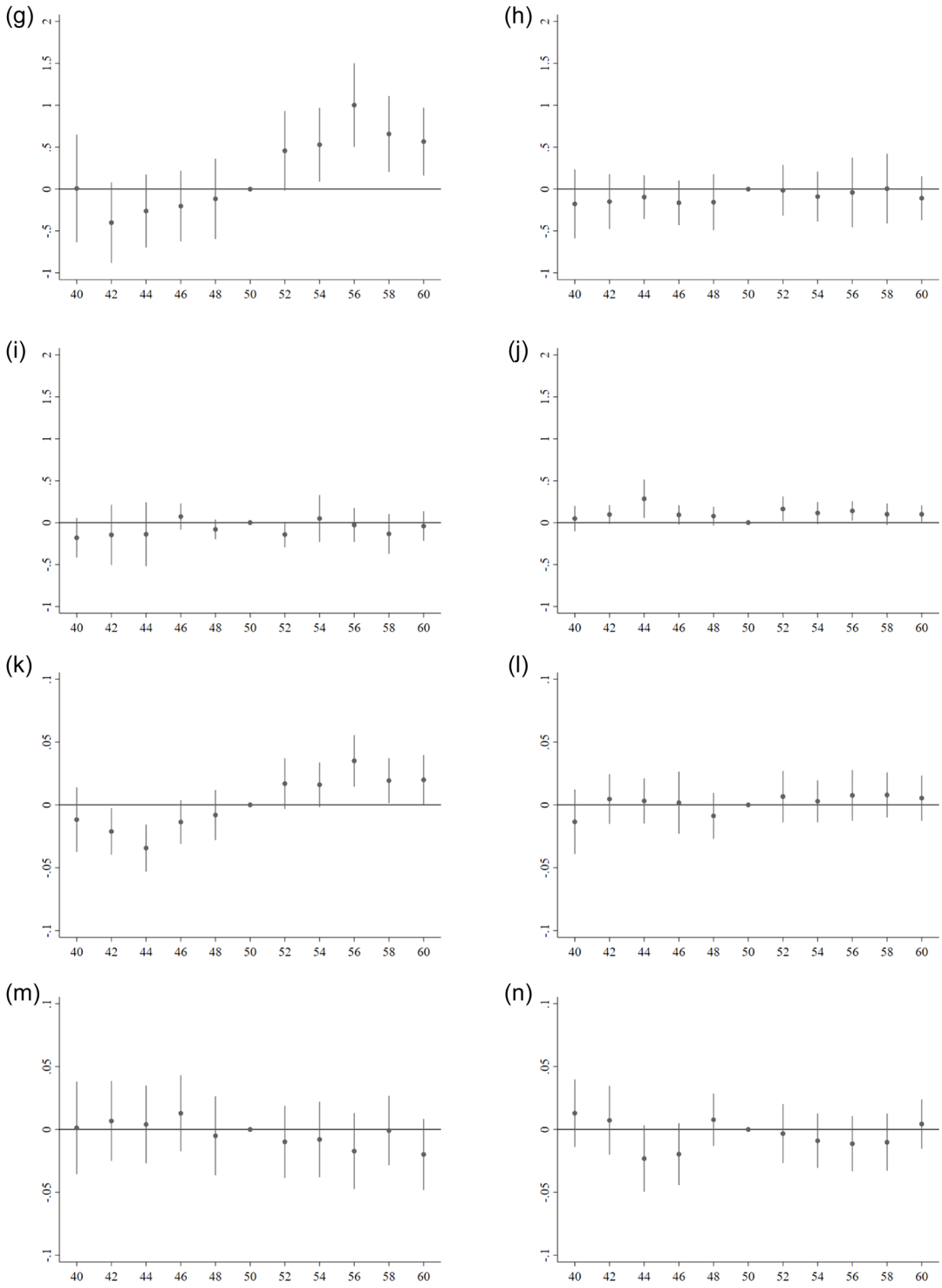

FIGURE 1 (Continued)

results from the econometric analyses that are more precise due to various controls and can separately identify ITT and TOT effects.

RESULTS

Reduced-form estimates

We first present the reduced-form results, that is, whether a focal locality's bond referendum success causes changes in the overlapper's fiscal outcome. The next section presents estimates for fiscal interaction: how does an increase in the focal locality's tax rate as a result of successful referendums change the overlapper's tax rates? Table 3 presents estimates of the intent-to-treat effect of bond passage on overlapping government fiscal decisions over the 5 years following the focal election, based on Equation (2). In the "5-year total" row, we also report results from an alternative specification where one estimate is obtained for the 5 years as a whole.[22] None of the coefficient estimates are statistically significant, suggesting that overlapping jurisdictions are not responsive to the focal locality being approved to increase property tax rates and issue new bonds. Examining the 5 years postmeasure as a whole, which increases the precision of the estimates, we do not observe any statistically significant impact on overlapper outcomes.

Table 4 presents estimates from the treatment-on-the-treated effect of passing focal election on the same sets of overlapping outcomes. For ease of comparison with the ITT estimates, we present here estimates for the 5 years postmeasure but also graph out the long-run effects up to ten years postelection in Appendix Figures D4. The TOT estimates, which control for bond measure dynamics after the focal jurisdiction wins or loses an election, are similar to the ITT estimates shown in Table 3 and consistently statistically insignificant. The confidence intervals are reasonably tight, as compared to the sample means reported in Table 1. In the bottom row, the average effects over the 5 years postelection are also statistically insignificant, again indicating a lack of vertical spillover.[23]

Taken together, we find no change in tax and spending efforts from overlapping governments to focal election passage. The overlapping governments do not change the probability of bringing a bond measure up for election or the proposed amount in bond elections, nor do they experience a discontinuous change in the probability of bonds being approved by voters. The focal election passage does not affect the overlapping governments' GO bond outstanding. It is possible that the voter-approval requirement deters the overlapping governments from issuing some GO bonds but they could tap the fiscal commons by issuing other GO bonds that are not subject to election, such as certificate of obligation. With the effect on the overall GO bonds being zero, it is also possible that the effects on the two types of GO bonds are of different signs and offset each other. We estimate the same ITT and TOT regressions on GO bonds that are subject to voter approval and those that are not. Results in Appendix Table D2 show that regardless of the types of GO bonds, the effect of focal election passage is statistically indistinguishable from zero.[24] Further, the overlappers do not respond by issuing more nonGO bonds, which are not subject to voter approval but are repaid using special taxes or fees and charges.

[22]This alternative specification is estimated with the same stacked data but in this specification, we replace the indicator variables for each of the 5 postmeasure years with one indicator variable representing the whole 5-year period.
[23]These are estimated based on an alternative specification with the same panel data for the TOT analysis. In the specification, we replace the indicator variables for election passage in each of the recent 5 years with one indicator variable representing the whole 5-year period.
[24]Data on the subcategories of GO bonds are available for a shorter period (2007 to 2019) as compared to that on the overall GO bond outstanding (2003 to 2019). The ITT estimate for voter-approved GO bonds is marginally significant in the third year after a successful measure. However, the TOT estimate for that year is not statistically significant.

TABLE 3 ITT effect of bond passage on overlapper fiscal outcomes.

	Has election (1)	Requested amount (2)	Approved amount (3)	GO bond (4)	NonGO bond (5)	MO tax (6)	IS tax (7)	Total revenue (8)	Total spending (9)	Total debt (10)
1 year after	−0.0020	−0.0158	0.0034	−0.0365	0.0114	0.0047	0.0006	0.0694	0.3811	−0.1320
	(0.0138)	(0.0420)	(0.0352)	(0.1077)	(0.0254)	(0.0075)	(0.0056)	(0.3971)	(0.2841)	(0.2108)
2 year after	−0.0121	0.0207	0.0205	−0.0064	0.0091	0.0031	0.0109	0.0538	0.5222	0.0997
	(0.0128)	(0.0285)	(0.0266)	(0.1214)	(0.0339)	(0.0087)	(0.0065)	(0.5868)	(0.3734)	(0.2238)
3 year after	−0.0134	0.0120	0.0012	−0.0025	0.0095	−0.0004	0.0104	−0.1855	0.4742	0.0086
	(0.0132)	(0.0615)	(0.0603)	(0.1494)	(0.0421)	(0.0097)	(0.0074)	(0.7839)	(0.4714)	(0.2666)
4 year after	0.0114	0.0976	0.0941	−0.0369	0.0717	0.0034	0.0103	−0.7602	0.2160	0.1018
	(0.0146)	(0.0673)	(0.0657)	(0.1973)	(0.0872)	(0.0109)	(0.0078)	(1.0091)	(0.3296)	(0.2915)
5 year after	−0.0058	−0.0368	−0.0248	−0.0019	0.0425	0.0037	0.0097	−1.0201	0.3350	0.2386
	(0.0146)	(0.0500)	(0.0397)	(0.1543)	(0.0521)	(0.0121)	(0.0081)	(1.3575)	(0.3396)	(0.3259)
5 year average	−0.0047	0.0144	0.0183	−0.0182	0.0253	0.0030	0.0080	−0.2838	0.3964	0.0413
	(0.0092)	(0.0238)	(0.0228)	(0.1226)	(0.0338)	(0.0083)	(0.0058)	(0.7318)	(0.3414)	(0.2336)
N	13,714	13,011	13,011	10,513	10,653	11,364	11,364	10,224	10,224	10,224

Note: The table shows intent-to-treat (ITT) effects. The MO tax represents property tax levied for operation. The IS tax represents property tax levied for debt services. GO bond stands for general obligation bond, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance, **1% level significance, ***0.1% level significance.

TABLE 4 TOT effect of bond passage on overlapper fiscal outcomes.

	Has election (1)	Requested amount (2)	Approved amount (3)	GO bond (4)	NonGO bond (5)	MO tax (6)	IS tax (7)	Total revenue (8)	Total spending (9)	Total debt (10)
1 year after	0.0020	0.0068	0.0026	0.0915	−0.0540	−0.0075	0.0044	0.3336	0.7194	−0.0046
	(0.0145)	(0.0309)	(0.0302)	(0.1353)	(0.0539)	(0.0097)	(0.0064)	(0.5960)	(0.5238)	(0.4280)
2 year after	−0.0059	0.0033	0.0056	0.0467	−0.0710	−0.0113	0.0099	0.3022	0.9188	0.5034
	(0.0108)	(0.0239)	(0.0154)	(0.1431)	(0.0591)	(0.0107)	(0.0077)	(0.8627)	(0.6779)	(0.3599)
3 year after	−0.0013	0.0366	0.0223	−0.0619	−0.0721	−0.0072	0.0037	−0.1636	0.6918	0.3253
	(0.0132)	(0.0704)	(0.0690)	(0.1931)	(0.0594)	(0.0103)	(0.0090)	(1.0781)	(0.7948)	(0.4006)
4 year after	0.0196	0.1108	0.1022	−0.0815	−0.0293	0.0008	0.0005	−1.0307	0.2086	0.2794
	(0.0157)	(0.0768)	(0.0754)	(0.2384)	(0.1043)	(0.0106)	(0.0089)	(1.2146)	(0.4879)	(0.4277)
5 year after	0.0045	−0.0047	−0.0041	−0.0268	−0.0829	0.0058	0.0006	−1.3415	0.3490	1.1508
	(0.0155)	(0.0454)	(0.0413)	(0.1985)	(0.0681)	(0.0102)	(0.0095)	(1.5663)	(0.4550)	(0.9219)
5 year average	0.0069	0.0288	0.0178	−0.0715	−0.0050	0.0058	−0.0038	0.1959	0.3830	−0.3949
	(0.0055)	(0.0240)	(0.0236)	(0.0767)	(0.0259)	(0.0047)	(0.0038)	(0.3082)	(0.2727)	(0.6014)
N	10,413	10,407	10,407	9155	9272	8729	87,294	7574	7574	7574

Note: The table shows treatment-on-the-treated (TOT) effects. The MO tax rate represents the property tax rate for operations. The IS tax rate represents the property tax rate for debt services. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance, **1% level significance, ***0.1% level significance.

Because focal election passage has little impact on overlapping governments' bond measure and bond issuance decisions, we expect no changes in the property taxes for debt service. That is exactly what we find. Theoretically speaking, the overlapping governments may respond by changing the operations property taxes, which are at the discretion of the local officials. However, we find no statistically significant effects on the operations tax rate. Finally, although estimates in the last three columns of Tables 3 and 4, which are based on the Census Survey of Government Finance data, are relatively imprecise, they provide a look into the effect on the overall government financial profile. There is no spillover to nonproperty tax revenue (such as sales taxes, fees and charges, and intergovernmental transfers) or nonbonded debt (such as short-term notes and bank loans). As a result, the focal election passage does not affect the overall spending level of overlapping governments.

While the empirical results suggest no change in overlapping jurisdictions' tax efforts, two scenarios are possible. First, as a successful focal election induces additional borrowing and property taxes of the focal jurisdiction, the tax base declines for all the overlapping localities due to the mobility of property owners.[25] As a result, the finding of a constant tax effort of the overlappers suggests a decline in the overall tax and spending among these localities, which we do not see using the Census Survey of Government Finance data. Alternatively, the successful focal election may have a negligible effect on the tax base and thus the overall budget of the overlapping jurisdictions. Testing the impact on the tax base helps shed light on the possible mechanisms behind vertical spillover (or the lack of). Table 5 reports the ITT and TOT regression results using the logged property market value within the focal jurisdiction as the outcome variable. All the coefficient estimates are statistically insignificant and very small in magnitude. That is, after the passage of a focal election, we observe no statistically significant mobility in the tax base after exogenous changes in taxation and public good provision induced by successful bond measures.[26]

Fiscal interaction estimates

So far, the results have shown that a focal locality's bond measure passage does not lead to a statistically significant change in the fiscal outcomes of overlapping jurisdictions. In this section, we go beyond the reduced form estimates and examine how the overlapper's tax and debt efforts respond to, instead of the binary event of measure passage, changes in the focal jurisdiction's tax and debt. Specifically, we modifies the ITT specification in equation 2 to the following two-stage least square (2SLS) regressions:

$$focal_{j,t+\tau} = \theta_\tau w_{j,t} + f(v_{j,t}, \lambda_\tau) + \alpha_\tau + \delta_t + \gamma_{jt} + e_{j,t+\tau}, \tag{4}$$

$$y_{j,t+\tau} = \mu^{ITT} focal_{j,t+\tau} + f(v_{j,t}, \lambda_\tau) + \alpha_\tau + \delta_t + \gamma_{jt} + \epsilon_{j,t+\tau}. \tag{5}$$

For policy relevance, we focus on the overall property tax effective rate, which combines the operations and debt service property taxes, and the GO bond outstanding. The earlier results show that a successful measure passage increases the focal government's debt-service property tax and GO bond, providing support for a strong first stage.[27] The first stage in Equation (4)

[25]The mobility could occur through a depressed residential real estate market as houses become less desirable, or reduced capital investment as the after-tax investment return decreases with the increased taxes.

[26]Individual property transaction data or local government-level migration data may provide more precision to test this but are unfortunately not available to us.

[27]In contrast, the overall tax and spending outcomes using the Census of Government Finance data are much less precisely estimated in the first stage, with *F*-statistics just shy of 10.

TABLE 5 Effect of focal measure passage on property value within focal jurisdiction.

	ITT	TOT
	(1)	(2)
1 year after	0.0139	0.0098
	(0.0153)	(0.0249)
2 year after	0.0003	0.0010
	(0.0204)	(0.0297)
3 year after	0.0088	0.0046
	(0.0236)	(0.0312)
4 year after	0.0159	0.0082
	(0.0263)	(0.0319)
5 year after	0.0161	0.0176
	(0.0288)	(0.0319)
5 year average	0.0106	−0.0113
	(0.0202)	(0.0141)
N	15,118	13,486

Notes: The table shows effects of focal measure passage on the logged market value of properties located within the focal jurisdiction. The column ITT reports the intent-to-treat effect, and the column TOT reports the treatment-on-the-treated effect. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance, **1% level significance, ***0.1% level significance.

instruments for focal government tax or bond efforts ($focal_{j,t+\tau}$) using focal measure passage ($w_{j,t}$). Then in Equation (5) we regress the overlapper outcome ($y_{j,t+\tau}$) on the instrumented focal government outcome. The coefficient estimate for μ^{ITT} provides the measure of vertical fiscal interaction. The 2SLS regression for the TOT effect is similarly estimated.

Table 6 presents results from the instrumental variables approach. Columns 1 and 2 focus on the ITT effects while the last two columns are TOT estimates. Columns 1 and 3 report the first-stage results. While the first-stage specification allows for time-varying effects of measure passage on focal government outcomes, we report the average effects in the table to preserve space. In line with the evidence presented earlier, successful measure passage is associated with a statistically significant increase in property taxes (by 0.37 mills in effective tax rate) and GO bonds (by $0.75 per $100 in property market value in the jurisdiction) during the 5 years postelection, as shown in column 1. All estimates in columns 2 and 4 are statistically insignificant, relatively small in magnitude, and do not have consistent signs. These estimates suggest that changes in focal jurisdiction property tax and GO bond outstanding do not spill over to overlapping jurisdictions. In Appendix C, we contrast these RD estimates with estimates from other specifications commonly used in the fiscal interaction literature such as fixed effects or instrumental variable regressions, to demonstrate how failing to account for endogeneity may cause one to find an effect when the true effect is null.

Robustness checks

We conduct several checks to test the robustness of the results to different specifications or measurements. Appendix Tables D4 and D5 include water districts in calculating the average

TABLE 6 Impact of focal government fiscal behavior on overlapper outcomes.

	ITT first stage (1)	ITT (2)	TOT first stage (3)	TOT (4)
Property tax	0.0369***	0.1209	0.0365***	−0.0384
	(0.007)	(0.1464)	(0.0042)	(0.1185)
GO bond	0.7534***	−0.0292	0.7094***	−0.0706
	(0.1538)	(0.0879)	(0.0785)	(0.1024)

Note: Each point estimate comes from a separate regression. The first two columns focus on the intent-to-treat (ITT) effects. The specification for column 1 comes from Equation (4), which is the first stage that instruments for focal government fiscal behavior using bond measure passage. The specification for column 2 comes from Equation (5), which regress the overlapper fiscal outcome on the instrumented focal government fiscal outcome. The last two columns focus on the treatment-on-the-treated (TOT) effects using a similar two-stage-least-square modification of Equation (3). Property tax is the overall effective property tax rate, combining both operation and debt service property taxes. GO bond measures general obligation bond outstanding, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance, **1% level significance, ***0.1% level significance.

overlapper outcomes. Recall that many water districts are established for newly developed or in-development areas and have relatively low tax bases but high debt amounts. As a result, for water districts, the outcomes that are scaled by property value have large means and are heavily skewed to the right. Compared to the baseline results excluding water districts in Tables 3 and 4, results including water districts in measuring overlapping outcomes show larger standard errors. The point estimates are also larger in magnitude, especially for outcomes related to bond measures and bond outstanding, as the means of these variables are very large among water districts. Despite these differences, the finding that focal election passage does not lead to a statistically significant change in overlapper outcomes largely holds.[28]

Appendix Table D6 shows the robustness of the reduced form results in a local RD framework.[29] Specifically, we estimate the ITT effects with the nonparametric local linear model following Calonico et al. (2019) and Calonico et al. (2017). We select a mean squared error-optimal bandwidth around the 50% vote share cutoff and compare overlapper outcomes between winning and losing focal elections within this bandwidth.[30] One constraint to the local RD approach is that it does not allow for an event study-style estimate. For each of years 1 to 5 after a focal election, we estimate the impact on overlapper outcomes separately.[31] As Table D6 shows, the estimates largely remain statistically insignificant and consistent with our main findings. Another constraint to the local RD approach is that it does not allow for the estimate of TOT effects, which requires controlling for issuance history. We cannot include the issuance history as covariates: the issuance history does meet the "predetermined covariates" criteria of being smooth at cutoff, as we have shown that earlier election results affect later election probability and results.

Appendix Table D8 examines the robustness of the reduced form results to alternative thresholds for identifying overlapping governments. We expect a priori that if a vertical spillover exists, it is likely more pronounced among the complete-overlap group as these overlapping

[28]The only exception is that the ITT estimate for debt-service property tax is marginally significant at the 5% level. However, the TOT estimate is statistically insignificant and very close to zero.

[29]Additionally, Appendix Table D7 shows that for the baseline global RD, findings are robust with linear or quadratic functions of vote share.

[30]We use the more parsimonious linear function of vote share in the estimation, but the results, available upon request, are robust with a quadratic function of vote share.

[31]As a result, different bandwidths are selected for estimating the effect of focal elections that occurred n=1 to 5 years ago. That is, the focal elections driving the estimates may differ.

jurisdictions are located completely within the focal locality and all of their taxpayers are subject to the tax changes of the focal locality. The impacts of the focal election in the 5 years after the election are consistently statistically insignificant, regardless of the overlapping threshold. The only exception is that the impact on overlapping localities' debt-service tax rate is significant at the 5% level. However, the magnitude of the estimate (−0.0034) is very similar to that from the baseline specification (−0.0038) and is only significant at the 5% level.

In addition, we examine the heterogeneity of effects to explore how the estimates may vary by contexts and institutions, including the size of bond measures, rural versus urban areas, and types of local governments. We first conduct an analysis focusing only on "significant" focal elections with potentially large effects, as reported in Appendix Table D9. Because it is not clear a priori which type of bond measure is likely to be the most salient among voters and policymakers in the focal and overlapping jurisdictions, we test two definitions of significant measures. First, we focus on the top tercile of measures based on the measure amount scaled by property value within the jurisdiction (measures that are larger than $2.3 per $100 in property value), as these measures are likely to represent a more significant burden on taxpayers. Second, we include only the top tercile of measures based on the proposed dollar amount of the bond (measures that are larger than $33 million), as large bond measures and the associated capital projects are likely to receive more publicity and attention. We re-estimate the measure of vertical interaction with the subsample based on each of the definitions, using the same model specification as in Table 6. Columns 1 and 3 in Table D9 show that focal bond measure passage is associated with a statistically significant increase in GO bonds and property taxes in the focal locality. Columns 2 and 4 show that the estimates are consistently statistically insignificant and similar in magnitude to those based on the full sample. These findings further suggest that the passage of focal measures, including large measures, does not spill over to overlapping jurisdictions.

Another potential source of effect heterogeneity is that focal elections may have a spillover effect in some geographic areas but not others. Compared to rural areas, high-density urban areas have greater government fragmentation, more channels for information dissemination, and political preferences that are more tolerant of big government. As a result, focal elections held by localities within these areas may be more likely to attract voter attention or to impact a large number of overlapping jurisdictions. Appendix Table D10 explores this issue by focusing on the core-based statistical areas as defined by the Census Bureau. In Panel A, we limit the analysis to focal jurisdictions that are at least partially located within a combined statistical area, which has at least one urban cluster of 10,000 or more population. Panel B limits the analysis to metropolitan statistical areas, a subset of the combined statistical area with at least one urbanized area of 50,000 or more population. Panel C limits the analysis to only central counties within a combined statistical area, which are the counties containing high-density urban areas instead of outlying counties of commuters.[32] Again, across the panels, we see strong first-stage results and statistically insignificant fiscal interaction estimates. There is no evidence for vertical spillovers in high-density areas.

Lastly, it may be possible that fiscal interactions differ with regard to the specific types of focal and overlapping localities, resulting in a null average effect. For example, if taxpayers value school infrastructure but not capital investments made by general-purpose governments, the tax base response and thus overlapper response to a rate increase by different types of focal jurisdictions will differ. In Appendix Table D11, we conduct analyses by specific types of government pairs. Panel A and B provide the fiscal interaction estimates between a focal school district and the overlapping cities or counties respectively. Due to the relatively small number of

[32]The technical definition is a county where at least 50% of the population resides within urban areas of 10,000 or more population, or contains at least 5000 people residing within a single urban area of 10,000 or more population.

general-purpose government referendums, in Panel C, we examined both cities and counties as the focal jurisdiction and the responses from the overlapping school districts. Overall, we see no statistically significant fiscal interaction and no consistently signed estimates for property tax and bonding effort responses, confirming the baseline results. One limitation of this subgroup analysis is that, due to the reduced sample size, the estimates are often imprecise, especially in Panels B where the number of overlappers is small and in Panel C where the number of focal jurisdictions is small.

DISCUSSION AND CONCLUSION

The theoretical literature on vertical fiscal spillovers points to ambiguity in the relationship and the existing empirical literature has yet to provide conclusive findings. This paper uses an RD design and finds that a successful bond election, which increases property taxes of the focal locality, does not impact overlapping governments' bond election efforts and results. Moreover, the election has no effects on bonds not subject to voter referendums or the overall revenue, spending, and debt outstanding of the overlapping governments. The null effects of a focal election are relatively precisely estimated and we are able to rule out effects of small sizes. It is reassuring that the findings hold for both bond authorization and bond issuance (as reflected by the level of bond outstanding). The findings also hold for both the direct responses and downstream responses with regard to nonGO debt and the overall budget. Instrumenting for the focal jurisdiction's debt and tax levels using focal election outcomes, we estimate the vertical spillover effect and fail to reject that it is statistically different from zero. The findings are robust across alternative measurements of overlapping governments, various model specifications, and subsample analyses focusing on large measures, urban areas, or specific types of government pairs. Together, the findings show no evidence of vertical fiscal spillover in the context of Texas local governments.

The theoretical literature suggests that the vertical spillover could be positive due to a fiscal commons effect where governments ignore the negative externality of their tax decisions on the common tax base (Flowers, 1988; Keen & Kotsogiannis, 2002). Alternatively, the positive effects may be caused by the price inelasticity of the demand for local public goods, where localities increase tax efforts to finance the same level of public goods when the tax increase of an overlapping jurisdiction lowers the tax base (Keen, 1998). On the other hand, a negative spillover could occur if Tiebout sorting takes place when more tax base moves to areas of the overlapping jurisdiction that are not congruent with the focal locality. The empirical findings that successful bond measures and the resulting property tax increases do not affect the tax base cast doubt on the applicability of the different theories with regard to tax base mobility in our context.

The null effect finding may suggest that all spillover effects are zero or that some are nonzero but offsetting (Turnbull & Djoundourian, 1993). For example, perhaps the public goods provided by some overlapping governments are substitutes while others are complements to that provided by the focal jurisdiction. While we cannot test this directly, the null findings from examining each type of government pair suggest against such a proposition. Nevertheless, we argue that in fiscal interaction research, beyond rate setting, the use of tax dollars also matters. The exogenous changes in local debt and taxes examined in this paper are for specific capital projects, which may conform to the benefit view of property tax. Assuming mobile households and immobile housing, this view postulates that local property taxes are fees paid to receive public services and thus tax increases do not drive out taxpayers because they come with offsetting public benefits (Nechyba, 2001).[33] For instance, Fischel (2001) argue that homeowners

[33]If the average taxpayer perceives the cost of tax increases to roughly match the benefit provided by the capital project, we would observe no capitalization and thus no change in the property tax base.

are motivated to have a voice in local finances because their home values are affected by taxes and services, which offsets the "race to the bottom" in tax-base competition.

Another possible interpretation is that the results are unique to the context of bond elections. That is, while local governments tend to tap the fiscal commons, the voter approval process effectively constrains the localities' ability to do so. However, a successful focal election does not affect overlappers' bond outcomes not subject to voter approval or the parts of their budget that are at the discretion of the local officials. Thus, the lack of vertical spillover may not be unique to the context of direct democracy.

The discussions thus far shed light on a few limitations of this study. The results do not imply that the decisions of overlapping jurisdictions never affect each other. Tax increases that are not specifically used to provide a public service (capital projects in our context) may induce behavioral responses from residents and the real estate market. For instance, Goodman and Carroll (2024) notes that current operation expenditures rather than infrastructure spending drive the substitution effects between special district and county expenditures. Very large or very persistent increases in taxes could also lead to a spillover, even though our results are robust to the inclusion of only the largest bond measures. The external validity of the finding may be constrained by the uniqueness of the state of Texas. The prevailing conservative ideology of the state may imply that public officials are wary of tax increases regardless of the voter approval requirement or decisions of the overlapping jurisdiction. However, our subsample analysis focusing on the more politically liberal urban counties generates the same null effect findings.

Further, Cellini et al. (2010) find school bond referendums to increase housing values in California, while we do not see an impact even when examining only school bond referendums. Institutional differences may be at play: California localities face very stringent property tax limits, and as a result, residents prefer additional bonds and taxes; such stringent constraints are not present in Texas. As this study contributes to the literature on fiscal decentralization and government fragmentation as well as the policy discussion on special-purpose governments, we call for more future work on broadly examining the vertical impact of a wide range of government policy decisions.

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CONFLICT OF INTEREST STATEMENT

The authors declare no conflict of interest.

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Appendix A: OVERLAPPER DEFINITIONS

Figure D2 demonstrates the process using the example of Orange County and nearby cities. Beaumont borders but does not overlap with the county; therefore, there is no overlapping relationship between the pair. A small portion (largely uninhabited) of Port Arthur is located within the county but the rest is in the nearby Jefferson County. They would be overlapper to each other only under the any-overlap threshold. That is, the any-overlap threshold likely identifies too many jurisdictions as overlappers and could lead to the attenuation in estimated effects. Seven cities, including the city of Orange, are located completely within the county. When Orange County successfully passes a bond referendum, all properties within the city of Orange (and the other six cities within the county) are subject to the resulting property taxes for county bond repayment. Therefore, Orange City is an overlapping locality of the county under the baseline and complete overlap thresholds. In contrast, the city of Orange represents only a small area within the county and its successful bond measures are less likely to have an impact on the county and county residents overall; the county would only be defined as an overlapper of the city with the any-overlap threshold.

Appendix B: PREREFERENDUM BALANCE

The RD design assumes that bond measure passage is as good as randomly assigned conditional on a smooth function of the vote share. We have presented evidence earlier that local governments cannot perfectly manipulate election outcomes. In this section, we test if, conditional on the vote share of a focal election, there are any differences in the means and trends of the

outcome variable prior to the focal election between overlappers (as well as focal localities themselves in a different set of regressions) with passed and failed focal elections. Because now the outcome variables are measured in the period before the election is held, we expect a RD regression similar to Equation (2) to yield no statistically significant effects.

Table D1 presents the pre-measure balance between the treatment and control groups. Each entry in the table represents an estimate of the relationship between focal election passage and a pre-measure outcome variable from a separate regression. The outcomes in the first two columns represent the pre-measure fiscal variables in the focal locality, while the outcomes in the last two columns are the average pre-measure fiscal variables across overlapping localities. The samples in columns 1 and 3 include only the year prior to a focal election. Columns 2 and 4 use data covering the 2 years prior to a focal election to estimate the effect on the changes in the outcomes.

Table D1 shows that, with three exceptions, most of the premeasure outcomes do not statistically significantly correlate with a later bond passage. The first exception is that bond passage seems to be positively correlated with the change in nonGO bond outstanding from $t-2$ to $t-1$ for focal localities. This suggests that the focal localities with an increasing revenue bond liability are more likely to see later GO bond measure passage. Moreover, the bond passage is positively correlated with a prior increase in operations property tax efforts and total spending among overlappers. Nonetheless, these differences could arise by chance, as we estimate a large number of regressions here. The three estimates are only statistically significant at the 5% significance level, and only the trend, but not the level, of pre-measure outcomes, exhibits a statistically significant relationship with later measure passage. Overall, the lack of consistently significant estimates indicates that the pre-measure characteristics are balanced between treatment and control groups, and lends support to the validity of the RD design.

Appendix C: NONRD SPECIFICATIONS

To put the RD estimates into perspective and to ensure that the lack of statistically significant spillover effects is not due to the specific data and institution examined in this study, we estimate the effect of a focal government's tax/bond effort on overlappers' tax/bond efforts, using identification strategies commonly used in the literature. As discussed earlier, the empirical literature on vertical spillover has mainly used a fixed effects model or instrumental variables regression to address endogeneity concerns.[34] Table D3 presents results from three different specifications. Column 1 estimates the standard two-way fixed effects controlling for focal locality-specific, time-invariant factors as well as year-fixed effects. The estimates will be biased if any locality-specific, time-varying factors affect the focal and overlapping fiscal outcomes at the same time. Column 2 additionally instruments for focal locality tax/bond with the same variable but lagged by 5 years. This follows the approach of Berry (2008), who argues that the historical value is likely more exogenous to current-year fiscal decisions. However, Berry (2008) uses a long, 30-year lag, which was not feasible given the sample period of our study. The instrumental variable does not eliminate bias if overlapping governments co-evolve on the same path of fiscal decisions over time, which they likely do as they operate in the same economic and political environment. Lastly, column 3 controls for focal locality-specific linear year trend in addition to the locality fix effects, to control for over-time differences specific to each locality, similar to Burge and Rogers (2011). The parametric time trend, however, is quite restrictive.

[34]The literature that examines vertical spillover between hierarchical governments, such as federal and state governments, often instruments for federal taxes using federal deficit. This type of instrumental variable is not feasible in the context of overlapping local governments, because local governments generally run a balanced budget and the deficit financing decisions of overlapping governments are unlikely to be exogenous to each other.

Table D3 shows that the nonRD specifications consistently point to a statistically significant, negative property tax spillover among overlapping governments (while the correlation is also negative for GO bond outstanding, it is statistically insignificant across all columns). When a focal locality increases the effective tax rate by 1 mill, the average overlapping government decreases its effective tax rate by 0.11 to 0.22 mill. This is similar to the negative relation between municipal and county property tax rates identified in Wu and Hendrick (2009) but smaller than the negative relation between city and county sales tax rates found in Agrawal (2016).[35] Importantly, the nonRD estimates are larger in magnitude than the ITT and TOT estimates for property tax in Table 6. While it is beyond the scope of this paper to pinpoint the reason behind the difference between RD and nonRD estimates, it is possible that the latter suffers from omitted variable bias. One omitted variable could be the underlying economic trends. For example, if an area experiences increasing poverty, cities may respond by improving tax efforts to provide needed public services but at the same time, the overlapping school district may decrease tax efforts as it is now eligible for more state transfers. The RD approach mitigates concerns over omitted variables by exploiting quasi-natural changes in tax efforts induced by successful referendums.

Appendix D: APPENDIX D

Figure D1

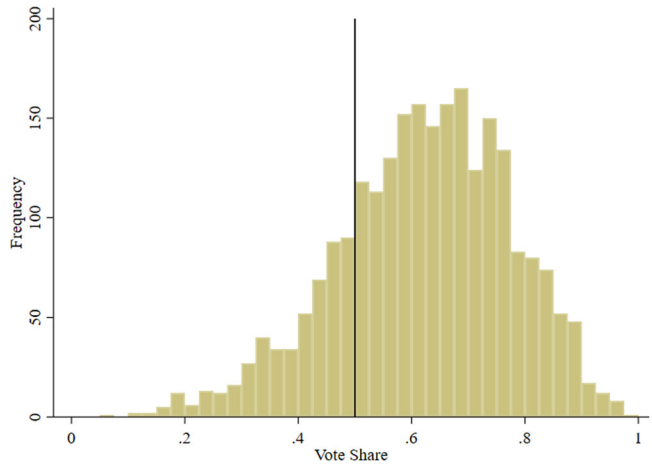
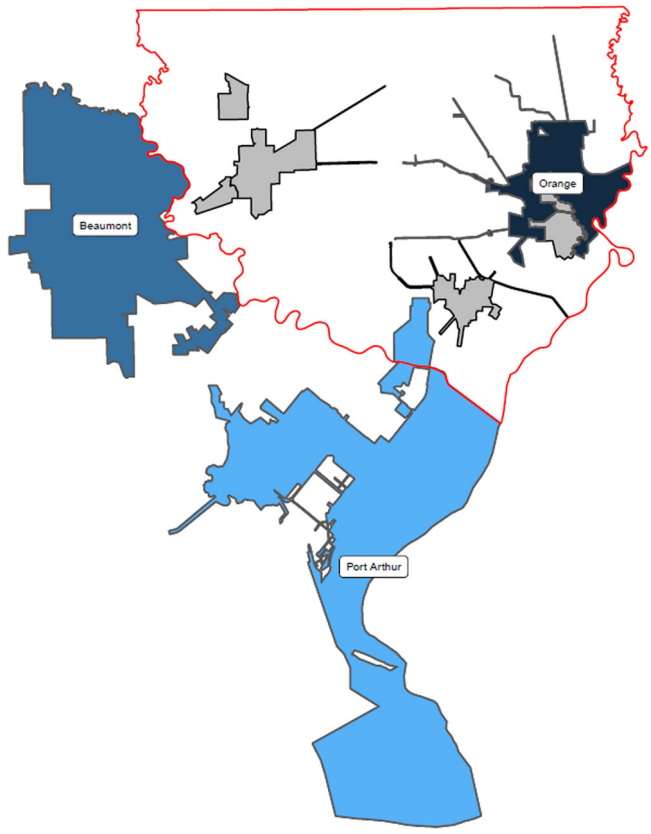


FIGURE D1 Histogram of vote share. The *Y* axis presents the number of focal elections within each bin. The vertical line indicates the threshold for voter approval.

[35]However, Wu and Hendrick (2009) find a positive relationship between the property tax rates of municipalities and overlapping school districts. Agrawal (2016) instruments for sales tax rates with the jurisdiction's area and perimeters, which cannot be applied here as the instrument is not as appropriate in a nonsales tax setting.

Figure D2



Locality pair	Focal locality	Nearby locality	Whether overlap		
			Baseline	Complete	Any
(1)	Orange county	Beaumont	No	No	No
	Beaumont	Orange county	No	No	No
(2)	Orange county	Orange	Yes	Yes	Yes
	Orange	Orange county	No	No	Yes
(3)	Orange county	Port Arthur	No	No	Yes
	Port Arthur	Orange county	No	No	Yes

FIGURE D2 Example of overlapping jurisdictions. The graph shows the boundaries of Orange county, outlined in red, and nine nearby cities (shaded). Three cities are highlighted to demonstrate how their overlapping relationship with the county changes with the different overlap thresholds and choices of a focal jurisdiction. The baseline/complete/any overlap scenario identifies overlappers where at least 50/100/>0% of its areas are located within the focal jurisdiction.

Figure D3

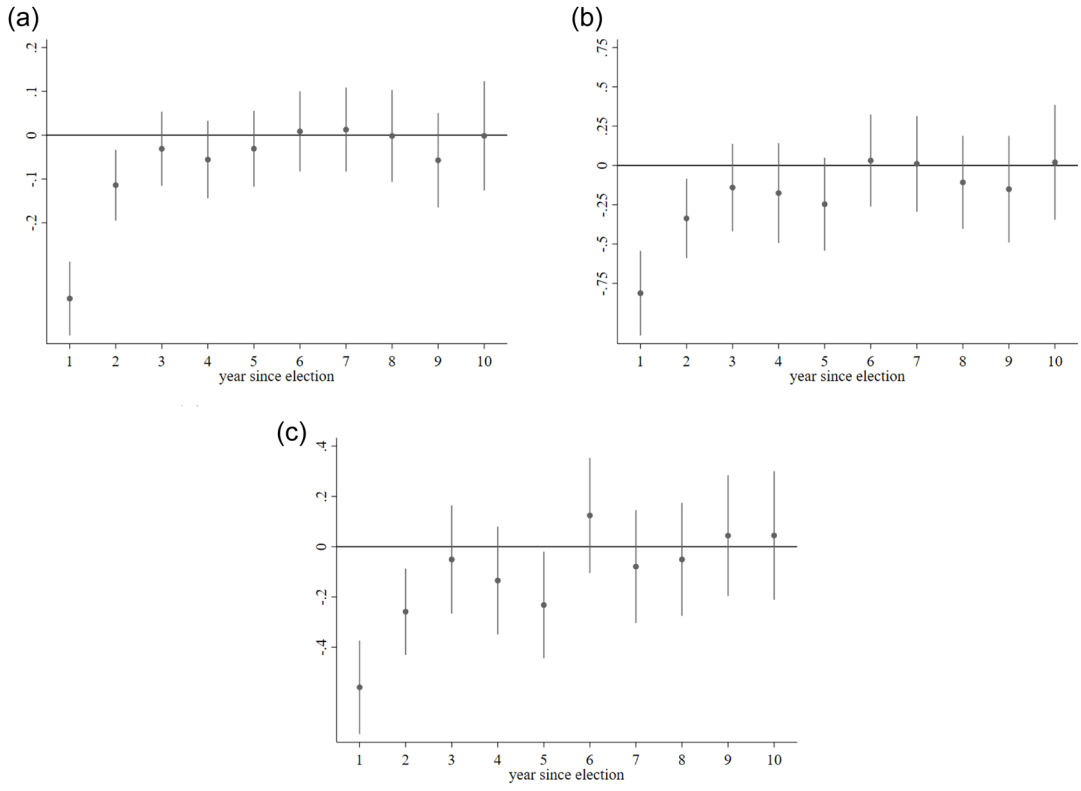


FIGURE D3 Impact of successful measure on focal locality's future bond elections. The graph shows the coefficients and the 95% confidence interval for the effects of bond measure passage on the focal local government's bond measure-related outcomes in later years. Vertical lines indicate 95% confidence intervals which are clustered at the focal locality level. (a) Has election, (b) requested amount, and (c) win amount.

Figure D4

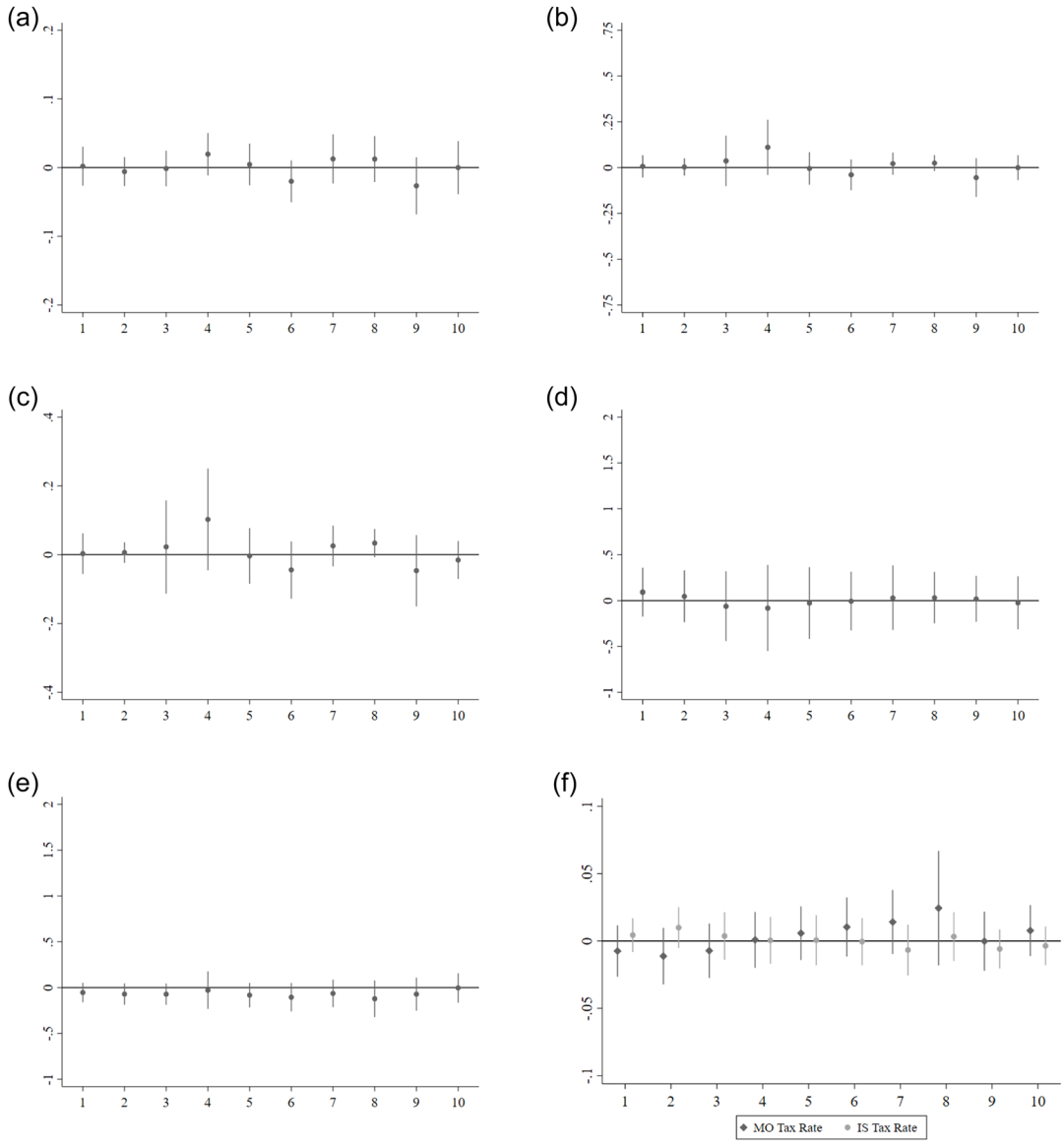


FIGURE D4 Treatment on the treated effect of bond passage on overlapper fiscal outcomes. The graph shows the coefficients and the 95% confidence interval for the treatment-on-the-treated effects of bond measure passage on the average overlapping local government's fiscal outcomes. The *x* axis represents the years since focal measure passage. The MO tax rate represents the property tax effective rate for operations. The IS tax rate represents the property tax effective rate for debt services. Standard errors are clustered at the focal locality level and reported in parenthesis. (a) Has election, (b) requested amount, (c) win amount, (d) GO bond outstanding, (e) NonGO bond outstanding, and (f) property tax.

Table D1

TABLE D1 Premeasure balance of treatment and control groups.

	Own		Overlapping	
	(1)	(2)	(3)	(4)
	Outcome $t-1$	ΔOutcome $t-2$ to $t-1$	Outcome $t-1$	ΔOutcome $t-2$ to $t-1$
Has election	−0.0173	−0.0005	0.0067	−0.0031
	(0.0303)	(0.0416)	(0.0128)	(0.0147)
Requested amount	−0.0269	−0.0664	−0.0165	−0.0411
	(0.1007)	(0.1475)	(0.0240)	(0.0352)
Approved amount	−0.0179	−0.0498	−0.0266	−0.0572
	(0.0234)	(0.0673)	(0.0209)	(0.0327)
GO bond	0.0366	0.0077	0.1619	−0.0236
	(0.2029)	(0.0390)	(0.1873)	(0.0505)
NonGO bond	−0.3433	0.1700*	−0.0811	−0.0025
	(0.2481)	(0.0780)	(0.0634)	(0.0199)
Operations property tax	0.0284	−0.0099	−0.0172	0.0106*
	(0.0281)	(0.0054)	(0.0231)	(0.0053)
Debt service property tax	0.0111	0.0021	−0.0172	−0.0072
	(0.0105)	(0.0040)	(0.0113)	(0.0053)
Total revenue	−0.1183	0.0302	0.2466	0.1315
	(0.1447)	(0.0213)	(0.2525)	(0.1050)
Total spending	−0.0962	0.0745	0.3990	0.2107*
	(0.1508)	(0.0506)	(0.2467)	(0.1002)
Total debt	−0.1472	0.2294	−0.7251	−0.0640
	(0.2383)	(0.2404)	(0.4747)	(0.1200)

Note: Each entry comes from a separate regression. Columns 1 and 3 include observations from the year before each focal election. Columns 2 and 4 include observations from the 2 years before each focal election. The specification follows Equation (2) with year indicators, a cubic function of the vote share, and a measure passage indicator. Standard errors are clustered at the focal jurisdiction level in parentheses.

*5% level significance, **1% level significance, ***0.1% level significance.

Table D2

TABLE D2 Effect of focal measure passage on overlapper GO bonds.

	ITT		TOT	
	Voter approval required (1)	Not required (2)	Voter approval required (3)	Not required (4)
1 year after	−0.0811	0.0114	−0.0527	−0.0540
	(0.0652)	(0.0254)	(0.0722)	(0.0539)
2 year after	−0.1077	0.0091	−0.0745	−0.0710
	(0.0743)	(0.0339)	(0.0819)	(0.0591)
3 year after	−0.1738*	0.0095	−0.1595	−0.0721
	(0.0846)	(0.0421)	(0.0913)	(0.0594)
4 year after	−0.1516	0.0717	−0.0959	−0.0293
	(0.0873)	(0.0872)	(0.0873)	(0.1043)
5 year after	−0.1377	0.0425	−0.0720	−0.0829
	(0.0843)	(0.0521)	(0.0762)	(0.0681)
5 year average	−0.1245	0.0657	−0.0637	0.0057
	(0.0685)	(0.1455)	(0.0346)	(0.0717)

Note: The table shows effects of focal measure passage on the overlapping governments' two subcategories of general obligation (GO) bonds, one subject to voter approval and one that is not. Certificate of obligation is a type of GO bonds backed by property taxes but do not require a referendum. The second type of GO bonds not requiring voter approval is the school maintenance and operations (M&O) bonds, which are issued for operation purposes but not construction or renovation; voters approve the maximum tax rate for M&O debt, and once approved, M&O debt may be issued without an election. Standard errors are clustered at the focal locality level and reported in parenthesis.

*5% level significance, **1% level significance, ***0.1% level significance.

Table D3

TABLE D3 Vertical spillover estimates from nonRD specifications.

	Fixed effects (1)	Instrument using lagged value (2)	Focal locality-specific trend (3)
Property tax	−0.1118***	−0.2188**	−0.1147***
	(0.0213)	(0.0735)	(0.0219)
GO bond	−0.0208	−0.1032	−0.0209
	(0.019)	(0.1877)	(0.0196)

Note: Each point estimate comes from a separate regression that does not take a regression discontinuity (RD) approach. The sample is a panel data set of focal locality-years. The dependent variable is the average overlapping government's fiscal outcomes, and the key independent variable is the focal government's property tax or general obligation (GO) bond outstanding, both scaled by the market value of properties within the jurisdiction. All specifications control for focal government fixed effects and year fixed effects. The specification in the first column only controls for focal locality fixed effects. The specification in column 2 also instruments for focal locality tax or bond using the lagged value of the tax or bond variable by 5 years. The specification in column 3 controls for linear time trends specific to each focal locality. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance, **1% level significance

***0.1% level significance.

Table D4

TABLE D4 ITT effect on overlapper fiscal outcomes, including water districts.

	Requested amount (1)	Approved amount (2)	GO bond (3)	NonGO bond (4)	MO tax (5)	IS tax (6)	Total revenue (7)	Total spending (8)	Total debt (9)
1 year after	0.8684	0.8893	0.0163	0.1013	−0.0062	0.0129	0.1379	0.3591	0.1156
	(0.9339)	(0.9337)	(0.1105)	(0.0912)	(0.0113)	(0.0118)	(0.3786)	(0.2776)	(0.3663)
2 year after	0.6877	0.6870	0.0354	0.0846	−0.0045	0.0215*	0.0839	0.4963	−0.0469
	(0.5618)	(0.5617)	(0.1283)	(0.0870)	(0.0106)	(0.0098)	(0.5526)	(0.3578)	(0.2726)
3 year after	0.9287	0.9211	0.0620	0.1950	−0.0063	0.0168	−0.0929	0.4759	−0.1743
	(0.8075)	(0.8074)	(0.1554)	(0.2266)	(0.0108)	(0.0090)	(0.7397)	(0.4509)	(0.3307)
4 year after	1.1400	1.1387	−0.1413	0.1346	−0.0064	0.0243*	−0.6536	0.1826	−0.2514
	(0.6827)	(0.6826)	(0.2761)	(0.1367)	(0.0123)	(0.0106)	(0.9539)	(0.3120)	(0.3712)
5 year after	0.6959	0.6965	0.1028	0.1087	−0.0158	0.0339*	−0.9096	0.3257	−0.1283
	(0.7108)	(0.7106)	(0.1823)	(0.1185)	(0.0163)	(0.0164)	(1.2868)	(0.3150)	(0.4317)
5 year average	0.8610	0.8643	0.0154	0.0518	−0.0074	0.0209*	−0.2070	0.3781	−0.0756
	(0.6669)	(0.6669)	(0.1278)	(0.0531)	(0.0099)	(0.0099)	(0.6883)	(0.3245)	(0.2918)

Note: The table shows intent-to-treat (ITT) effects for outcomes scaled by property market value within a jurisdiction. Compared to the effects reported in the main text, samples here include water districts as well. The MO tax represents the property tax effective rate for operations. The IS tax represents the property tax effective rate for debt services. GO bond stands for general obligation bond, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis.

*5% level significance, **1% level significance, ***0.1% level significance.

Table D5

TABLE D5 TOT effect on overlapper fiscal outcomes, including water districts.

	Requested amount (1)	Approved amount (2)	GO bond (3)	NonGO bond (4)	MO tax (5)	IS tax (6)	Total revenue (7)	Total spending (8)	Total debt (9)
1 year after	1.4850	1.4805	0.0306	0.0802	−0.0164	0.0091	0.4753	0.7210	0.5262
	(1.0537)	(1.0538)	(0.1718)	(0.1132)	(0.0105)	(0.0080)	(0.5803)	(0.5061)	(0.6246)
2 year after	1.0434	1.0417	−0.0202	0.0429	−0.0195	0.0135	0.3942	0.9107	0.6323
	(0.7459)	(0.7457)	(0.2038)	(0.1079)	(0.0126)	(0.0095)	(0.8207)	(0.6488)	(0.4065)
3 year after	0.6309	0.6159	−0.0536	0.1541	−0.0200	0.0059	0.0574	0.7665	0.4551
	(0.8096)	(0.8102)	(0.2254)	(0.2459)	(0.0118)	(0.0096)	(1.0390)	(0.7682)	(0.4318)
4 year after	1.4945	1.4863	−0.2716	0.0684	−0.0070	0.0052	−0.6978	0.2753	0.4099
	(1.1047)	(1.1051)	(0.4081)	(0.1531)	(0.0123)	(0.0104)	(1.1768)	(0.4775)	(0.4725)
5 year after	2.0839	2.0834	−0.0110	0.0280	−0.0109	0.0056	−0.9658	0.4777	1.4838
	(2.0817)	(2.0813)	(0.2332)	(0.1313)	(0.0124)	(0.0120)	(1.5204)	(0.4349)	(0.9796)
5 year average	−0.1047	−0.1163	−0.1068	0.0647	0.0024	−0.0020	0.1966	0.3804	−0.1664
	(0.2297)	(0.2298)	(0.1201)	(0.0715)	(0.0061)	(0.0044)	(0.2998)	(0.2613)	(0.5679)

Note: The table shows treatment-on-the-treated (TOT) effects for outcomes scaled by property market value within a jurisdiction. Compared to the effects reported in the main text, samples here include water districts as well. The MO tax rate represents the property tax rate for operations. The IS tax rate represents the property tax rate for debt services. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance, **1% level significance, ***0.1% level significance.

Table D6

TABLE D6 ITT effect of bond passage on overlapper fiscal outcomes, nonparametric local linear estimates.

	Has election (1)	Requested amount (2)	Approved amount (3)	GO bond (4)	NonGO bond (5)	MO tax (6)	IS tax (7)	Total revenue (8)	Total spending (9)	Total debt (10)
1 year after	0.0127	0.0229	0.0114	0.0446	−0.0906	0.0252	0.0059	0.5389	1.0746	−0.0112
	(0.0136)	(0.0232)	(0.0149)	(0.2367)	(0.1325)	(0.0308)	(0.0157)	(0.7319)	(0.6447)	(0.5541)
2 year after	0.0081	0.0170	0.0111	0.2556	−0.1041	0.0375	0.0087	0.0659	1.0926	0.4744
	(0.0127)	(0.0146)	(0.0092)	(0.2365)	(0.1236)	(0.0335)	(0.0170)	(0.9743)	(0.7925)	(0.5072)
3 year after	−0.0034	−0.0058	−0.0151	0.2350	0.0057	0.0298	0.0036	−0.2927	1.0997	0.2174
	(0.0082)	(0.0179)	(0.0159)	(0.2493)	(0.1285)	(0.0348)	(0.0184)	(1.2653)	(0.8185)	(0.4516)
4 year after	0.0169	0.0886	0.0903	0.0579	0.2418	0.0550	0.0101	−1.6181	0.6880	0.2902
	(0.0264)	(0.1027)	(0.0950)	(0.3635)	(0.2403)	(0.0404)	(0.0206)	(1.5956)	(0.3778)	(0.4262)
5 year after	0.0344	0.0781*	0.0760	0.3132	−0.0048	0.0509	0.0158	−2.0561	0.9509	1.2174*
	(0.0181)	(0.0342)	(0.0440)	(0.2807)	(0.1387)	(0.0413)	(0.0210)	(1.9377)	(0.5178)	(0.5336)
5 year average	0.0137	0.0283	0.0192	−0.0022	−0.0028	0.0232	0.0074	−0.1172	1.1979	0.2243
	(0.0092)	(0.0338)	(0.0319)	(0.2493)	(0.1222)	(0.0348)	(0.0160)	(1.1307)	(0.7675)	(0.5140)

Note: The table shows intent-to-treat (ITT) effects estimated with the nonparametric local linear model (i.e., robust in Stata) based on Calonico et al. (2019) and Calonico et al. (2017). The MO tax represents property tax levied for operation. The IS tax represents property tax levied for debt services. GO bond stands for general obligation bond, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis.

*5% level significance, **1% level significance, ***0.1% level significance.

Table D7

TABLE D7 Five-year effect on overlapper fiscal outcomes, alternative polynomials.

	Has election (1)	Requested amount (2)	Approved amount (3)	GO bond (4)	NonGO bond (5)	MO tax (6)	IS tax (7)	Total revenue (8)	Total spending (9)	Total debt (10)
Panel A: Linear										
ITT	0.0090	0.0365	0.0319	−0.0107	0.0210	0.0027	0.0048	−0.2050	0.2424	−0.1019
	(0.0079)	(0.0221)	(0.0201)	(0.0959)	(0.0228)	(0.0072)	(0.0045)	(0.4779)	(0.2447)	(0.2792)
TOT	0.0084	0.0358	0.0280	−0.0927	0.0142	0.0057	−0.0050	0.1858	0.3412	0.0312
	(0.0049)	(0.0253)	(0.0251)	(0.0694)	(0.0242)	(0.0046)	(0.0036)	(0.2731)	(0.2556)	(0.3966)
Panel B: Quadratic										
ITT	−0.0002	0.0166	0.0117	−0.0125	0.0151	−0.0000	0.0067	−0.3612	0.2595	−0.0574
	(0.0071)	(0.0202)	(0.0182)	(0.1046)	(0.0274)	(0.0068)	(0.0052)	(0.6689)	(0.2883)	(0.2594)
TOT	0.0080	0.0305	0.0188	−0.0503	0.0002	0.0078	−0.0042	0.1743	0.3656	−0.3273
	(0.0053)	(0.0236)	(0.0232)	(0.0799)	(0.0243)	(0.0046)	(0.0037)	(0.2909)	(0.2478)	(0.5761)

Note: The table shows effects during the 5 years after a focal election from intent-to-treat (ITT) and treatment-on-the-treated (TOT) specifications. Panel A shows results with a linear function of vote share; Panel B shows results with a quadratic function of vote share. The MO tax represents property tax for operation. The IS tax represents property tax for debt services. GO bond stands for general obligation bond, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance, **1% level significance, ***0.1% level significance.

Table D8

TABLE D8 Five-year effect on overlapper fiscal outcomes, alternative overlapping threshold.

	Has election (1)	Requested amount (2)	Approved amount (3)	GO bond (4)	NonGO bond (5)	MO tax (6)	IS tax (7)	Total revenue (8)	Total spending (9)	Total debt (10)
Panel A: complete overlap										
ITT	0.0006	0.0081	−0.0035	−0.0683	0.0390	0.0042	0.0081	−0.4192	0.4082	−0.0014
	(0.0062)	(0.0177)	(0.0161)	(0.1447)	(0.0397)	(0.0093)	(0.0075)	(0.9521)	(0.4808)	(0.3022)
TOT	−0.0017	0.0411	0.0370	−0.1051	−0.0093	0.0102	−0.0071	0.1648	0.4480	−0.3924
	(0.0058)	(0.0576)	(0.0575)	(0.0980)	(0.0374)	(0.0062)	(0.0056)	(0.4794)	(0.4201)	(0.7535)
Panel B: any overlap										
ITT	−0.0073	−0.0198	−0.0177	0.0341	0.0012	0.0079	−0.0005	−0.0694	0.1602	0.0008
	(0.0084)	(0.0228)	(0.0213)	(0.0564)	(0.0169)	(0.0059)	(0.0029)	(0.2785)	(0.1534)	(0.1447)
TOT	−0.0037	−0.0045	−0.0068	−0.0036	0.0025	0.0014	−0.0034*	0.0311	0.0797	−0.0752
	(0.0047)	(0.0196)	(0.0194)	(0.0336)	(0.0129)	(0.0029)	(0.0017)	(0.1362)	(0.1254)	(0.1545)

Notes: The table shows effects during the 5 years after a focal election from intent-to-treat (ITT) and treatment-on-the-treated (TOT) specifications. In the "complete overlap" panel, an overlapping jurisdiction completely locates within the focal locality. In the "any overlap" panel, an overlapping jurisdiction shares at least some area with the focal locality. Each point estimate comes from a separate regression. The MO tax represents property tax for operation. The IS tax represents property tax for debt services. GO bond stands for general obligation bond, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis.

*5% level significance, **1% level significance, ***0.1% level significance.

Table D9

TABLE D9 Impact of focal government fiscal behavior on overlapper outcomes, significant measures only.

	(1) ITT first stage	(2) ITT	(3) TOT first stage	(4) TOT
Panel A: large measure by scaled amount				
Property tax	0.0530***	−0.0184	0.0541***	−0.0687
	(0.0125)	(0.1479)	(0.0084)	(0.1403)
GO bond	1.4569***	0.1056	1.2395***	−0.0840
	(0.3347)	(0.1009)	(0.1536)	(0.1225)
Panel B: large measure by dollar amount				
Property tax	0.0359**	−0.0877	0.0274***	−0.0753
	(0.0115)	(0.2369)	(0.0060)	(0.1913)
GO bond	1.1670***	0.0361	0.4953***	−0.0881
	(0.2577)	(0.0707)	(0.1238)	(0.1409)

Note: This table reports the vertical fiscal interaction estimates using bond measures that are significant in size or potential impact. In the "Large measure by scaled amount" panel, the sample includes the top one-third focal elections by the property value-scaled proposed bond amount. In the "Large measure by dollar amount" panel, the sample includes the top one-third focal elections by the proposed bond dollar amount. Each point estimate comes from a separate regression. The first two columns focus on the intent-to-treat (ITT) effects. The specification for column 1 comes from Equation (4), which is the first stage that instruments for focal government fiscal behavior using bond measure passage. The specification for column 2 comes from Equation (5), which regresses the overlapper fiscal outcome on the instrumented focal government fiscal outcome. The last two columns focus on the treatment-on-the-treated (TOT) effects using a similar two-stage-least-square modification of Equation (3). GO bond measures general obligation bond outstanding, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance,

**1% level significance,

***0.1% level significance.

Table D10

TABLE D10 Impact of focal government fiscal behavior on overlapper outcomes in high density areas.

	(1) ITT first stage	(2) ITT	(3) TOT first stage	(4) TOT
Panel A: combined statistical areas				
Property tax	0.0360***	0.0572	0.0334***	0.0333
	(0.0075)	(0.1576)	(0.0043)	(0.1263)
GO bond	0.8887***	−0.0229	0.6974***	0.0096
	(0.1888)	(0.0880)	(0.0938)	(0.0955)
Panel B: metropolitan statistical areas				
Property tax	0.0377***	0.1966	0.0322***	0.0307
	(0.0080)	(0.1716)	(0.0046)	(0.1423)
GO bond	1.0165***	−0.0627	0.7036***	0.0033
	(0.2090)	(0.0942)	(0.0989)	(0.0971)
Panel C: central counties				
Property tax	0.0420***	0.2673	0.0325***	−0.0136
	(0.0104)	(0.2027)	(0.0053)	(0.1742)
GO bond	1.3024***	0.0101	0.7415***	−0.0189
	(0.2615)	(0.1001)	(0.1182)	(0.1159)

Note: This table reports the vertical fiscal interaction estimates using the sample of jurisdictions located in core-based statistical areas as defined by the Census Bureau. In the "combined statistical areas" panel, the sample includes focal jurisdictions which at least partially locate within a metropolitan or micropolitan statistical area. In the "metropolitan statistical areas" panel, the sample includes focal jurisdictions which at least partially locate within a metropolitan statistical area. In the "central counties" panel, the sample includes focal jurisdictions which at least partially locate within a central county, that is, counties within a metropolitan/micropolitan statistical area with high-density urban areas instead of outlying counties of commuters. Each point estimate comes from a separate regression. The first two columns focus on the intent-to-treat (ITT) effects. The specification for column 1 comes from Equation (4), which is the first stage that instruments for focal government fiscal behavior using bond meausure passage. The specification for column 2 comes from Equation (5), which regresses the overlapper fiscal outcome on the instrumented focal government fiscal outcome. The last two columns focus on the treatment-on-the-treated (TOT) effects using a similar two-stage-least-square modification of Equation (3). GO bond measures general obligation bond outstanding, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis. *5% level significance, **1% level significance,
***0.1% level significance.

Table D11.

TABLE D11 Impact of focal government fiscal behavior on overlapper outcomes, by type of government pairs.

	(1) ITT first stage	(2) ITT	(3) TOT first stage	(4) TOT
Panel A: focal school district on overlapping city				
Property tax	0.0388***	−0.0445	0.0382***	−0.1092
	(0.0076)	(0.1549)	(0.0050)	(0.1296)
GO bond	0.7961***	0.0291	0.8122***	−0.1361
	(0.1763)	(0.1147)	(0.0888)	(0.1324)
Panel B: focal school district on overlapping county				
Property tax	0.0559**	−0.1183	0.0406**	−0.3836
	(0.0190)	(0.2965)	(0.0148)	(0.4569)
GO bond	0.5831*	0.0653	0.5336***	−0.0850
	(0.2565)	(0.0557)	(0.1499)	(0.0739)
Panel C: focal county or city on overlapping school district				
Property tax	0.0065	−1.5247	0.0183***	0.5058
	(0.0106)	(1.0221)	(0.0044)	(0.4701)
GO bond	0.1118	−1.6027	0.1682	−0.3216
	(0.1630)	(1.8301)	(0.1367)	(0.7788)

Note: This table reports the vertical fiscal interaction estimates for specific types of government pairs. Each point estimate comes from a separate regression. The first two columns focus on the intent-to-treat (ITT) effects. The specification for column 1 comes from Equation (4), which is the first stage that instruments for focal government fiscal behavior using bond measure passage. The specification for column 2 comes from Equation (5), which regresses the overlapper fiscal outcome on the instrumented focal government fiscal outcome. The last two columns focus on the treatment-on-the-treated (TOT) effects using a similar two-stage-least-square modification of Equation (3). GO bond measures general obligation bond outstanding, which is supported by the full faith and credit of the borrowing government. Standard errors are clustered at the focal locality level and reported in parenthesis.

*5% level significance

**1% level significance

***0.1% level significance.