MARKET RESPONSES TO VOTER-APPROVED DEBT

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US local governments often seek voter approval through bond referenda prior to borrowing. Passing bond referenda increases borrowing authority and potential debt levels, which can influence government credit risk. Focusing on Texas local governments, we estimate the causal effects of bond authorization on credit risk with a regression discontinuity design. Credit risk is measured by the average bond yields of existing debt. For school districts, one standard deviation increase in authorized debt per capita causes bond yields to increase by approximately 9.9–22.5 basis points in the subsequent 12 months. We find no effects for cities and counties.

Keywords: bond referenda, borrowing authorization, credit risk, regression discontinuity design

JEL Codes: H74, H76

I. INTRODUCTION

Governments sometimes require voter approval of local bond sales. Each additional approved bond authorization increases the potential outstanding debt for that government. This may change the credit risk perceived by the investors who hold the existing bonds because a larger debt burden might increase the propensity to default and because bondholders would face competition from more creditors should that default occur. Moreover, the yields from trading on a government's bonds today can signal its underlying credit risk and affect the borrowing costs for new bonds sold to the same investor community tomorrow. We examine the effects of passing bond referenda on investors' perceptions of the attractiveness of a government's existing debt expressed as the yields on its bonds in the secondary market.

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We use local bond referenda in Texas as an empirical setting. To estimate the causal impact of bond authorization, we use a regression discontinuity (RD) design, a causal identification strategy that has been widely applied to bond referenda (Cellini, Ferreira, and Rothstein, 2010; Hong and Zimmer, 2016; Martorell, Stange, and McFarlin, 2016; Kogan, Lavertu, and Peskowitz, 2017). The RD design compares the average bond yields of a local government that barely passes a bond referendum with one that barely loses one, assuming some "as if" randomness in the bond passage by a narrow vote margin (Lee and Lemieux, 2010). We find that, as the authorized borrowing amount per capita increases by one standard deviation, the average bond yields increase by approximately 9.9–22.5 basis points for school districts during the 12 months following a bond referendum. This indicates that bond investors interpret bond referendum passage as a cause of higher government credit risk.

This work has research and policy implications. The approval of bond referenda has the potential to affect the value not only of the (few) bonds that trade but also the portfolios of bonds that investors may seek to sell later. Evidence that authorizations move market yields suggests that the information provided from referenda is efficiency improving but could result in capital losses for potential sellers. Information about issuers is often incomplete or infrequently provided. The Government Accounting Standards Board (GASB) is actively considering more frequent financial reporting requirements for governments, which would come at a substantial cost to governments seeking to comply. The assumption that market participants care about interperiod changes in financial condition is made without much evidence one way or the other. This study may provide some evidence of the value of increased information. We discuss this more at the end of the paper.

As we discuss in Section III, there is an incomplete picture of disciplinary market effects with municipal debt issuance in the literature. It seems that those issuing more debt ought to pay more at the margin as the repayment ability of the jurisdiction is further extended and investors seek more compensation for the risk. Governments often operate under self-imposed debt limits to protect taxpayers from high debt burdens and to protect their good standing in the financial markets. It becomes harder to argue for such limits if markets do not act as if additional debt matters. Investor demand for the tax exemption may simply overpower such disciplinary effects. Also, the view of increased debt contributing to a net negative assessment of a jurisdiction is not universally supported. That picture is made considerably more complex based on research showing property value growth following school bond approvals (Cellini, Ferreira, and Rothstein, 2010). There is inconsistent evidence of this kind of market discipline to date, and we offer a new approach to investigating it.

II. BACKGROUND

Local governments issue debt on the capital markets to finance many kinds of infrastructure, including schools, buildings, bridges, and many other things. Title 26 of the Code of the Internal Revenue Service (26 US Code § 103) excludes the interest on state and local government bonds from gross income calculations, thus raising the after-tax yield of a municipal bond investment relative to a similar corporate bond or other taxable investment. This indirect subsidy, allowing governments to borrow at lower rates than taxable issuers, is a primary motivation for governments to issue municipal bonds as opposed to borrow in some other manner.

While there are more than 89,000 local governments in the United States, most are small. Even among those that issue debt in a given year, it may be five or ten years before they borrow again. Annual financial statements typify the totality of disclosure for most. Few issuers maintain any regular investor outreach efforts. This increases the cost of "information search" for municipal market participants. The small subset of outstanding bonds traded prior to maturity is traded "over the counter."[1] Large numbers of mostly small issuers, high transaction costs, infrequent issuance, rare trading (conducted "over the counter"), and limited information disclosure are among the most common arguments supporting the characterization of the municipal bond market as an inefficient market (Harris and Piwowar, 2006; Reck and Wilson, 2006).

Only a minority of bonds outstanding are traded in the secondary market, but the resulting yields provide an important piece of market information for the larger universe of bondholders making buy or hold decisions. The trades are also informative for underwriters contemplating new money issues and provide credit-specific yields compared with generally blended indices. One could view them as a proxy of the market responses when anticipating material changes in the financial condition of a government (such as newly authorized debt).

III. PREVIOUS STUDIES

The financial market provides a financing channel and may also put downward pressure on government borrowing through higher interest rates. The response in the financial market is a critical parameter for local governments to consider in making borrowing decisions (Hildreth, 1993). As interest costs increase in the financial market, local governments tend to borrow less (Holtz-Eakin, 1991; Clingermayer and Wood, 1995; Millar, 2003). Moreover, local governments may borrow less because of the potential defeats of bond proposals on the ballot (Kiewiet and Szakaly, 1996; Ely and Jacob, 2013).

Previous studies on government credit risk have focused on various aspects of borrower characteristics, including fiscal performance and debt level in the primary and secondary bond markets. Many studies find that the fiscal performance of government borrowers influences their credit risk as reflected in bond yields in the primary

[1] Municipal bonds are traded in disparate over-the-counter dealer markets between individual brokers and their customers without any central exchange, clearing or market-making operations, or official trading hours. Broker-dealers execute transactions for customers by individually matching buyers and sellers.

bond market (Ardagna, Caselli, and Lane, 2007; Novy-Marx and Rauh, 2012). As indicated by factors such as deficit levels, when fiscal performance deteriorates, government borrowers may have difficulty servicing debt, leading the bond investors to request higher returns to compensate for the increased default risk (Benson and Marks, 2007).

Another indicator of fiscal performance, outstanding debt, can influence credit risk. The "market discipline" thesis suggests that the financial market requires higher risk premiums expressed as higher interest costs as governments borrow more (Lane, 1993). There has been mixed evidence for the market discipline effect. On the one hand, many studies find that the level of government borrowing is positively correlated with borrowing costs (Liu and Thakor, 1984; Capeci, 1994). On the other hand, some studies do not find the predicted positive impact of borrowing level on borrowing costs (Capeci, 1991; Robbins and Simonsen, 2012). For example, Robbins and Simonsen (2012) find little evidence that the level of debt outstanding influences state government borrowing costs, a fact that they attribute to a strong taste for state government general obligation debt.

However, it remains unclear whether the relationship between borrowing level and borrowing costs is causal. Anticipated interest costs can affect the decision to borrow. Only a few studies have made explicit attempts to address this endogeneity. For instance, Robbins and Simonsen (2012) follow Capeci (1994) and use an instrumental variable approach by treating the homeowner rates and the proportion of senior citizens in the population as instruments for state debt levels.

In the secondary bond market, bond yields reflect market assessments of the credit quality of the bonds being traded, which further captures the credit quality of the governments that issue these bonds (Cole, Liu, and Smith, 1994). If material changes occur to the original bond issuers, the credit risk will vary, leading to changes in the secondary market bond yields (Denison, 2000, 2006). Empirical studies have shown that borrower characteristics influence credit risk in the secondary bond market. For instance, the uncertainty of passing a tax reform by the federal government affects secondary market bond yields (Slemrod and Greimel, 1999). The bankruptcy of Orange County, California, in 1994 increased the bond yields of municipal bonds issued by Orange County and created contagion effects for non–Orange County bonds (Denison, 2000; Halstead, Hegde, and Klein, 2004). Similarly, the literature on the secondary bond market has not addressed the endogeneity concern adequately until a few recent studies that use difference-in-difference analyses (Gao, Murphy, and Qi, 2019; Chalmers, Liu, and Wang, 2021) and an instrumental variable regression (Gao, Lee, and Murphy, 2020).

IV. BOND AUTHORIZATION AND CREDIT RISK

Bond authorization in bond referenda indicates an increase in debt levels and capital investments in the future. Both consequences can affect the credit risk of local governments. First, the authorized debt issuance may increase the credit risk.

Unless the new debt issuance replaces exactly existing debt, it should increase the total debt level of a local government. Despite the lack of consistent empirical evidence, there seems to be a theoretical consensus on the positive relationship between debt level and credit risk (Johnson and Kriz, 2005; Robbins and Simonsen, 2012). As Robbins and Simonsen (2012) note, higher debt levels indicate potential constraints on future government budgets because of debt expenditure and the potential for increased tax burdens on citizens to service the debt.

The relationship between debt level and borrowing costs is at the core of the market discipline thesis in finance theory. The intuition is that interest costs will rise when one borrows more. Specifically, the market discipline thesis states that "as borrowing increases, the market initially insists on a higher interest rate spread and eventually excludes the borrower from further lending altogether" (Lane, 1993, p. 58). This implies that the debt level should increase borrowing costs at an accelerated rate. The market discipline thesis has received some empirical support (Liu and Thakor, 1984; Capeci, 1994; Bayoumi, Goldstein, and Woglom, 1995). For example, Goldstein and Woglom (1991) find that the level and growth of the debt of municipalities increase their yield spread.

Second, capital investments resulting from bond authorizations might potentially decrease the credit risk of a local government issuer. A majority of bond proposals fund capital projects vital for public amenities and the local economy. For example, among the 1,935 Texas bond measures between 2005 and 2016, 54.26 percent were proposed to finance the buildings and renovations of school facilities, streets, and parks and recreational facilities. If the marginal benefits from these capital investments exceed the marginal costs of the tax burden to support the debt, the bond authorization may increase government assets, which should decrease the credit risk.

More specifically, remediating undercapitalization can increase property values and local governments' fiscal capacity. Cellini, Ferreira, and Rothstein (2010) find that school facility investments authorized via bond referenda increased property values in California. They interpret the positive effect as a sign of an inefficiently low level of school capital investments. This could certainly be the case for Texas school districts, as Martorell, Stange, and McFarlin (2016, p. 14) point out that "a significant number of schools in the state are in need of repair." Given the heavy reliance on property taxes by school districts, the increases in property values should strengthen their fiscal capacity and reduce the credit risk. Furthermore, capital investments may help grow the local economy (Gramlich, 1994; Romp and De Haan, 2005; Pereira and Andraz, 2013), contributing to a more robust revenue base and higher fiscal capacity. As Munnell (1992, p. 191) summarizes, "everyone agrees that public capital investment can expand the productive capacity of an area, both by increasing resources and by enhancing the productivity of existing resources."

In sum, while the potential rise in debt levels may increase the credit risk, the growth in capital investments may decrease it, leaving the net impact of bond authorization indeterminate ex ante. As Cellini, Ferreira, and Rothstein (2010, p. 222) point out, "bond-funded investments are accompanied by an increased tax burden with an approximately equal present value." Suppose the funds are well-spent on capital investments and thus generate larger benefits than the taxes levied to service the debt. In that case, a bond authorization may reduce the credit risk of a local government issuer. Conversely, if the funds do not lead to sound capital investments or generate smaller benefits than the taxes, bond authorization should increase the credit risk.

Finally, the timing of the effects of bond authorization depends on the schedule of new debt issuance and the efficiency of the market responding to new information. On the one hand, in Texas, the bonds authorized by voters are usually issued later on a need basis. The capital investments associated with the bond authorization take time to plan and carry out. Consequently, bond investors may not observe an immediate surge in new debt issuances or capital improvements. On the other hand, if bond investors are forward-looking, they may not need to wait until the new debt issuances to respond to the new information from the bond passage. The market efficiency theory holds that a market is efficient if prices "at any time 'fully reflect' all available information" (Fama, 1970, p. 383). The more efficient the market is, the shorter it should take to respond to the information of bond referenda passage. In a less efficient market, it could take some time for the information to percolate through to yields. In the empirical tests, we account for the dynamic effects of bond authorization by imposing alternative windows of time.

V. DATA

A. Bond Referenda Data

This article uses bond referenda data from local governments in Texas, including cities, counties, and school districts. As Martorell, Stange, and McFarlin (2016) point out, Texas is a diverse and large state that provides an ideal setting to study local bond referenda. To issue general obligation bonds, local governments in Texas must obtain voter approval in referenda that use a simple majority rule. Voter approval provides authorization for local governments to issue bonds. The passage of a bond referendum does not require that the bonds be issued shortly. The bonds will be issued when the funds are needed.

The bond referenda data are taken from the Texas Bond Review Board. The key feature of the data that allows an RD design is that they include the voting records of bond referenda. In particular, the data contain the number of votes for and against a bond proposal, making it possible to calculate the share of votes for the bond passage. Under a simple majority rule, a bond proposal passes if the vote share is greater than 50 percent. Due to data availability, we focus on the period between 2005 and 2016. In the sample, cities and counties held 778 bond referenda, while school districts held 1,058 bond referenda. A local government entered the sample if it held at least one bond referendum in this period. Some local governments held more than one bond referendum, while others held only one.

We group the local governments in the sample into two categories. One includes the general-purpose governments of cities and counties,[2] and the other includes special-purpose governments of school districts. Table 1 shows the summary statistics of bond referenda by year for the city and county sample as well as the school district sample. As Panel A of Table 1 shows, cities held more bond referenda than counties. Panel B shows that school districts held more bond referenda than cities. During the Great Recession, all three types of local governments held fewer bond referenda than in other periods.

Bond authorization through bond referenda signifies potential changes in future debt levels or capital investments, which may affect the government credit risk as indicated by the average bond yields of the existing debt. Empirically, bond referenda provide a setting for applying RD designs to estimate the causal impact of bond passage. Several studies have applied RD designs to the bond referenda of school districts in California (Cellini, Ferreira, and Rothstein, 2010), Michigan (Hong and Zimmer, 2016), Texas (Martorell, Stange, and McFarlin, 2016), and Ohio (Kogan, Lavertu, and Peskowitz, 2017). In the RD design, we compare the bond yields of the jurisdiction that barely passes a bond referendum with that which barely fails one. The narrowly passed bond referenda may generate considerable news about future bond issuances and make it more likely to cause secondary market responses.

Nonetheless, there may be potential threats to the validity of the RD design using local bond referenda data. Local governments may attempt to manipulate bond passage by holding multiple bond referenda for the same bond proposal or splitting the same bond proposal into multiple parts (Hong and Zimmer, 2016). Following Cellini, Ferreira, and Rothstein (2010) and Hong and Zimmer (2016), the sample is adjusted in two steps.[3] First, the first bond measure is kept among all the bond measures of the same issuer in a year. Eliminating the repeated bond measures within a year should reduce the threat to RD validity resulting from multiple attempts at bond referenda in a short time. Second, the bond measure with a maximum vote share is kept among multiple bond measures by the same issuer on the same day. This should help reduce the threat to RD validity that is caused by splitting the same bond proposal to maximize votes. The adjusted sample of 304 bond measures for cities and counties and 903 bond measures for school districts contain only one bond measure per issuer in a year.

B. Bond Yield Data

The municipal bond transaction data for local governments in Texas between 2005 and 2016 are taken from the Municipal Securities Rulemaking Board (MSRB),

[2] As a robustness check, we have reestimated the model for cities and counties separately and find stronger effects for cities than counties. The results are presented in Table OB2.

[3] See the Appendix A for details.

Table 1

Summary Statistics for Bond Measures

Year	No. of Bond Measures	Percent by City	Percent Passed	Total Votes	Vote Share	Bond Amount (million dollars)
Panel A. City and County (N = 778)						
2005	73	0.82	0.83	8,771	0.66	13.62
					(0.17)	(23.38)
2006	124	0.84	0.88	50,331	0.65	39.48
					(0.14)	(70.17)
2007	117	0.86	0.93	12,991	0.73	25.10
					(0.17)	(52.60)
2008	30	0.73	0.80	38,774	0.60	22.06
					(0.11)	(27.01)
2009	8	0.87	0.75	736	0.66	18.21
					(0.33)	(23.84)
2010	50	0.94	0.58	5,450	0.51	6.16
					(0.15)	(12.95)
2011	35	0.62	0.62	4,194	0.57	21.75
					(0.33)	(40.57)
2012	67	0.88	0.89	73,815	0.64	43.90
					(0.12)	(76.25)
2013	81	0.85	0.79	11,281	0.57	24.10
					(0.14)	(44.29)
2014	61	0.98	0.88	12,001	0.67	27.93
					(0.13)	(82.78)
2015	87	0.80	0.93	20,518	0.67	37.64
					(0.13)	(92.98)
2016	45	0.80	0.86	28,237	0.63	54.50
					(0.12)	(126.94)
Panel B. School District (N = 1,058)						
2005	50		0.68	2,377	0.55	44.79
					(0.16)	(71.09)
2006	72		0.79	4,088	0.59	52.04
					(0.11)	(71.65)
2007	77		0.75	2,234	0.59	56.78
					(0.14)	(119.69)
2008	59		0.66	4,884	0.58	(47.88)
					(0.14)	(101.50)
2009	50		0.64	2,326	0.57	30.39
					(0.15)	(41.95)
2010	57		0.45	3,617	0.53	29.95
					(0.17)	(61.75)

Table 1 (Continued) Summary Statistics for Bond Measures

Year	No. of Bond Measures	Percent by City	Percent Passed	Total Votes	Vote Share	Bond Amount (million dollars)
2011	69		0.58	1,869	0.55	38.42
					(0.13)	(74.96)
2012	77		0.75	5,161	0.60	47.90
					(0.17)	(216.62)
2013	159		0.71	2,971	0.57	46.21
					(0.14)	(79.60)
2014	136		0.78	4,336	0.59	75.36
					(0.13)	(167.60)
2015	138		0.82	2,723	0.62	79.43
					(0.14)	(179.91)
2016	114		0.75	4,611	0.58	66.02
					(0.15)	(113.73)

Note: In Panel A, because the proportion of bond measures by cities and counties adds up to 100 percent, this table only presents the percent of bond measures by cities. For vote share and bond amount, standard deviations are in parentheses.

accessed through Wharton Research Data Service (WRDS). The bond transaction data record the bond yield of each transaction. They also include detailed information about the transactions, such as the timing of trades and the trading amounts. The observations with bond yields below the 1st and above the 99th percentiles are replaced with the values at these points to avoid the influence of outliers.[4]

The dependent variable is the average bond yields at the jurisdiction level. In the bond transaction data, one local government may have multiple bonds, which can be traded multiple times. As in prior studies (Gao, Lee, and Murphy, 2019, 2020; Partridge and Medda, 2020), we aggregate the bond yields from the transaction level to the jurisdiction level by taking the average for each period. We further weigh the average bond yields by time to maturity of the transacted bonds because the bonds with longer time to maturity may be subject to greater shocks from bond referenda passage.

Finally, we match the adjusted bond referenda sample with the bond transaction data using the names of the government issuers. In the matched sample, the number of bond measures is 228 for cities and counties and 632 for school districts. The data allow us to observe bond yields before and after bond referenda at the jurisdiction level. Table 2 shows the summary statistics of average bond yields during the window of 2 months before and 12 months after bond referenda. The average bond

[4] The main results are robust with alternative treatments of outliers, as shown in Table OD5.

Table 2
Summary Statistics for Bond Yields

	Passed	Failed	All	Passed	Failed	All
Variables	City and County			School District		
Bond yield	2.981	2.766	2.958	2.727	2.719	2.725
	(1.111)	(1.151)	(1.117)	(1.150)	(1.152)	(1.151)
Maturity (months)	162.688	171.557	170.610	206.233	201.686	202.561
	(47.662)	(56.266)	(55.470)	(70.743)	(72.160)	(71.906)
Par amount (thousands)	180.311	173.112	173.881	154.198	201.682	192.541
	(766.042)	(267.416)	(355.397)	(252.671)	(283.565)	(278.494)
Dollar price	104.177	103.479	103.554	99.643	102.053	101.589
	(3.961)	(4.694)	(4.626)	(13.244)	(11.289)	(11.728)
Coupon rate	4.183	4.189	4.188	3.779	3.806	3.801
	(0.705)	(0.859)	(0.844)	(1.290)	(1.217)	(1.231)
No. of observations	2,184	261	2,445	4,589	1,094	5,683
No. of bond measures	200	28	228	506	126	632

Note: The sample is limited to the window of 2 months before and 12 months after bond referenda. Passed means that a vote share is greater than 50 percent. Failed means that a vote share is less than or equal to 50 percent. Standard deviations are in parentheses.

yield is about 2.96 in the city and county sample and about 2.73 in the school district sample.

VI. EMPIRICAL STRATEGY

A. Regression Discontinuity

We estimate the effects of passing bond referenda on the average bond yields of existing debt at the jurisdiction level. However, the bond passage is likely endogenous to bond yields. First, local governments may consider the expected borrowing costs when making borrowing decisions, issuing more bonds when interest rates are lower. This may create a negative yet spurious correlation between bond passage and bond yields. Second, there can be many observable or unobservable factors (willingness to pay, management capacity and professionalism, changes in capital improvement plans, demographic factors) that could influence both bond passage propensities and bond yields simultaneously. While scholars can control as many observables as possible, measurement errors and unobservable factors can still bias the estimates.

We employ an RD design in this study to address these endogeneity concerns. In an RD design, there is a running variable, which carries a cutoff. Treatment is assigned on one side of the cutoff but not on the other side. The key assumption is that the assignment of treatments is "as good as random" near the cutoff. This is based on the related assumption that the subjects cannot *precisely* manipulate the running variable to change their treatment status (Lee and Lemieux, 2010). Consequently, the observations just below the cutoff provide good comparisons to those just above the cutoff. Scholars conclude causal inference by using the randomness introduced by the inability of subjects to manipulate. The RD analysis estimates the local average treatment effects, which may not be generalized to observations far away from the cutoff.

In the case of local bond referenda in Texas, the running variable is vote share, calculated as the percentage of votes supporting a bond proposal in total votes. The cutoff is 50 percent because a vote share greater than 50 percent means that a bond proposal has passed. The bond referenda with vote shares above 50 percent constitute the treatment group, while those with vote shares below 50 percent constitute the control group. Under the RD design, the assumptions are that there is no precise manipulation of the vote share and the treatment assignment is approximately random near the 50 percent cutoff. In the following analyses, we provide tests that confirm the plausibility of these assumptions.

Nevertheless, one complication exists because of the panel structure of the bond referenda. Local governments can hold no bond referenda, one bond referendum, or multiple bond referenda in a year or across years. Bond referenda that fail one year may be passed the following year, and vice versa. This can result in multiple treatment assignments of the same local governments over time. To account for this dynamic nature of the treatment assignments, Cellini, Ferreira, and Rothstein (2010) modify the traditional RD method, followed by Martorell, Stange, and McFarlin (2016) and Hong and Zimmer (2016). Built upon these studies, we estimate the following model:

$$y_{jt\tau} = \sum_{\tau=-2}^{12} (\theta_\tau d_{jt} g_\tau + \delta_\tau f(v_{jt}) g_\tau) + \alpha_\tau + \beta_{t\tau} + \gamma_{jt} + e_{jt\tau}.$$

In this model, $y_{jt\tau}$ is the outcome variable, that is, the average bond yield observed τ periods after a bond measure at time t by a local government j. On the right-hand side, d_{jt} is a dummy variable indicating whether the local government j passes a bond measure at time t, which equals one if the vote share is greater than 50 percent; g_τ is a dummy variable indicating the number of periods after a bond measure; and $d_{jt}g_\tau$ is an interaction term between d_{jt} and g_τ, which captures the period-specific effects of passing a bond measure as θ_τ. Moreover, $f(v_{jt})$ is a polynomial function of the vote share, specified as a cubic in the main data analyses, as in prior studies (Cellini, Ferreira, and Rothstein, 2010; Hong and Zimmer, 2016). In addition, α_τ is the period-gap fixed effect, $\beta_{t\tau}$ is the calendar-period fixed effect, and γ_{jt} is the bond measure fixed effect. Finally, $e_{jt\tau}$ is the error term.

As in the prior studies (Cellini, Ferreira, and Rothstein, 2010; Hong and Zimmer, 2016; Martorell, Stange, and McFarlin, 2016), the model is estimated by pooling data for multiple periods of τ to improve the precision of the estimation. For each jurisdiction j with a bond measure at time t, we stack all observations in the window of τ periods around t. We then combine the stacked data set for each bond measure into one data set covering the entire study period from 2005 to 2016. The standard errors are clustered at the jurisdiction level to account for the dependence in error terms created by multiple bond measures of the same jurisdiction (Cameron and Miller, 2015).

Following Cellini, Ferreira, and Rothstein (2010) and Hong and Zimmer (2016), θ_τ is constrained to 0 when $\tau < 0$ but is allowed to vary freely when $\tau > 0$. The assumption is that there are no anticipation effects on bond yields of passing a bond referendum near the 50 percent threshold. This seems plausible because bond investors may not precisely predict the probability of bond passage for the bond referenda that pass or fail by a narrow margin. The assumption is consistent with the empirical tests that show a lack of effects of bond passage on prereferenda trends in the average bond yields.[5]

The model is estimated on the stacked data set with the period τ ranging from -2 to 12. While the secondary bond market is generally considered relatively inefficient, there seems no conclusive evidence on the degree of market efficiency (Fischer, 1983; Marquette and Wilson, 1992; Denison, 2006). Previous studies have examined secondary bond market responses to policy changes by week (Fischer, 1983) and by month (Gao, Lee, and Murphy, 2019, 2020; Partridge and Medda, 2020). Anecdotal evidence suggests that it usually takes months from bond referenda to the issuance of the authorized debt (Hudman, Terry, and Hall, 2017; Texas Association of School Boards, 2020). We present the main results using the periods measured in months. We also conduct sensitivity analyses using periods of τ measured in days or weeks.[6]

We adopt an RD implementation approach that uses all data on both sides of the cutoff and absorbs variation from nonclose bond measures with flexible controls of the vote share, as in the previous studies (Cellini, Ferreira, and Rothstein, 2010; Hong and Zimmer, 2016; Martorell, Stange, and McFarlin, 2016; Kogan, Lavertu, and Peskowitz, 2017). Assuming the conditional expectation of the unobserved determinants of the outcome variable given the vote share is continuous, it can be approximated by a polynomial function of the vote share. Under this assumption, the error terms of the outcome variable are asymptotically uncorrelated with the treatment variable and one can estimate the treatment effects consistently (Imbens and Lemieux, 2008; Cellini, Ferreira, and Rothstein, 2010). We use a cubic function of the vote share as the main specification and present sensitivity tests with alternative polynomial functions in the appendix. In addition, we conduct robustness checks with an alternative approach to implementing the RD design that limits the sample to various small bandwidths of the vote share to use the information only from close bond measures.[7] The alternative approach offers lower statistical power and shows

[5] The results are presented in Table OA2.
[6] The results by day and by week are reported in Table OB3.
[7] The results are presented in Table OB1.

weaker effects. But the results are broadly consistent with the main findings, especially for the school district sample.

B. Validity of the RD Design

To test the validity of the RD design, we first conduct a McCrary's density test on the running variable (McCrary, 2008). McCrary's density test examines the distribution of the running variable, which would be smooth around the cutoff if the treatment is randomly assigned. Figure 1 reports the results of McCrary's density test. Figure 1 shows no signs of the vote share jumping near the 50 percent threshold. As shown in both Panels A and B, the discontinuities around the 50 percent threshold are not statistically significant ($p < 0.05$). This suggests that there is no manipulation of the running variable for the sample of cities and counties and the sample of school districts.

The balance of the background variables is then tested. If the RD assumption is valid, the observed characteristics of subjects in the treatment and the control groups should be balanced in the vicinity of the cutoff. Given the data availability, the test incorporates fiscal and demographic characteristics of local governments and some key features of bond referenda. Table OA1 (Appendices A–D are available online) shows the results of the balance tests. When focusing on the full sample, a few variables show statistically significant differences ($p < 0.1$) between the two sides of the cutoff. This suggests that an RD design is warranted to address the potential endogeneity concerns. Nonetheless, when restricting the sample to the vicinity of the 50 percent cutoff, the differences in the background variables become statistically insignificant. This indicates that the background variables are balanced and the RD analysis is valid.

Before conducting a formal RD analysis, we present graphic evidence to show the differences in means of average bond yields between local governments that pass and local governments that fail bond referenda. Figure 2 shows the average bond yields by vote share for bond referenda in the window of 2 months before and 12 months after bond referenda. With 50 percent as the cutoff, the bond measures on the left constitute controls, while those on the right are treated. As Figure 2 shows, the average bond yields for the treatment group, or the local governments that pass bond referenda successfully, are higher than the control group. The differences appear small but are evident without controlling for any covariates. This is particularly the case for the school district sample.

VII. RESULTS

A. Main Results

Table 3 shows the impact of passing a bond measure on bond yields in the subsequent 12 months. While Columns 1 and 2 show the effects of bond passage, Columns 3 and 4 show the effects of the authorized bond amount in bond referenda. Each row shows the month-specific effects. Columns 1 and 3 present the results

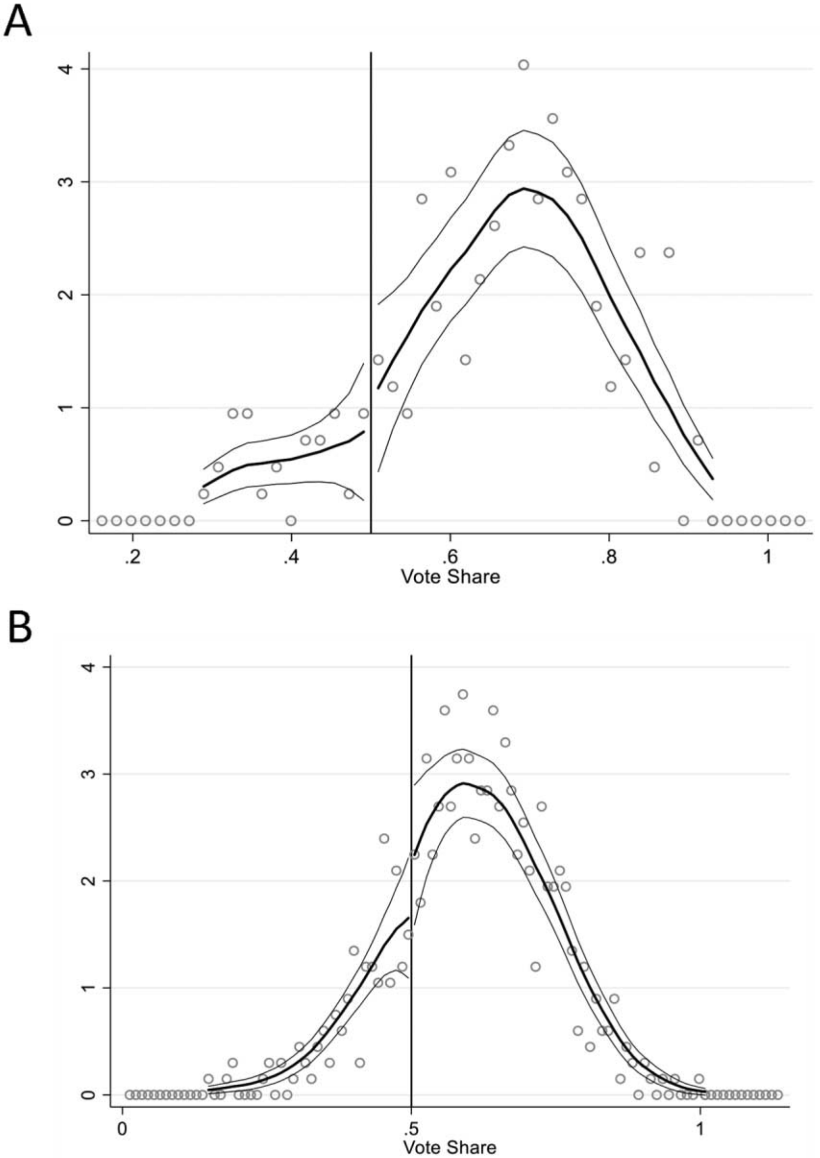


Figure 1. Distribution of vote share for matched bond measures. Panel *A* depicts the distribution of vote share for cities and counties. Panel *B* depicts the distribution of vote share for school districts. The number of bond measures is 228 for cities and counties and 632 for school districts. The *x*-axis represents vote share for passing a bond measure; the *y*-axis represents density.

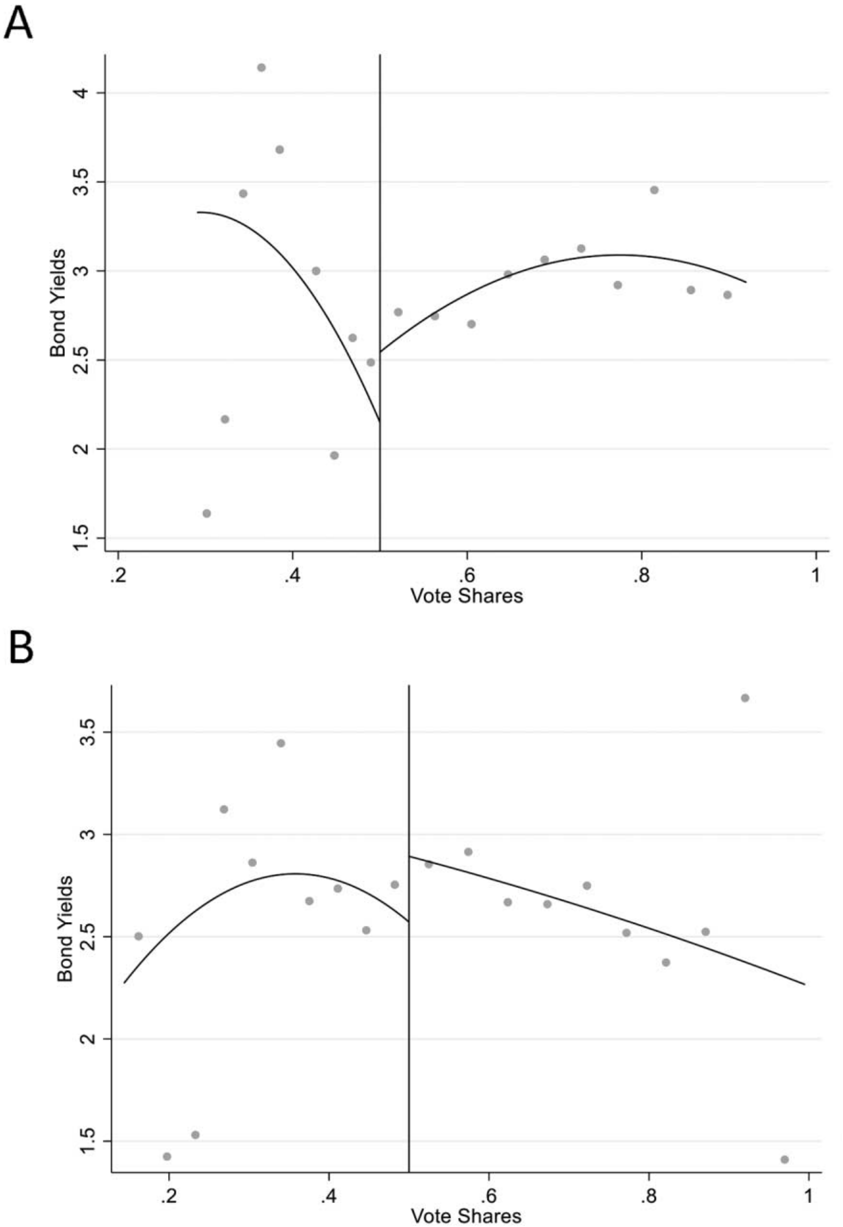


Figure 2. Average bond yields by vote share. Panel *A* depicts the average bond yields for cities and counties. Panel *B* depicts the average bond yields for school districts. This graph plots bond yields by vote share without covariates, limiting the window to 2 months before and 12 months after a bond measure. The points show the sample average of bond yields within bins of 5 percent points of vote share; the line shows a polynomial fit of order 2.

Table 3

Effects of Passing Bond Measures on Bond Yields for 12 Months

Relative Months after Referenda	Bond Passage: City and County (1)	Bond Passage: School District (2)	Authorized Bond Amount: City and County (3)	Authorized Bond Amount: School District (4)
1	0.362*	0.008	0.076**	0.002
	(0.187)	(0.090)	(0.038)	(0.014)
2	−0.040	0.226*	−0.009	0.036*
	(0.181)	(0.118)	(0.038)	(0.019)
3	0.072	0.339***	0.015	0.054***
	(0.189)	(0.117)	(0.039)	(0.018)
4	0.011	0.524***	0.003	0.082***
	(0.265)	(0.117)	(0.056)	(0.018)
5	−0.005	0.462***	−0.001	0.072***
	(0.198)	(0.134)	(0.042)	(0.021)
6	0.203	0.298**	0.043	0.048**
	(0.227)	(0.119)	(0.047)	(0.019)
7	0.315**	0.419***	0.063**	0.067***
	(0.141)	(0.116)	(0.029)	(0.018)
8	−0.266	0.441***	−0.052	0.070***
	(0.219)	(0.135)	(0.045)	(0.021)
9	0.064	0.300**	0.015	0.048**
	(0.209)	(0.118)	(0.044)	(0.019)
10	0.188	0.399***	0.040	0.064***
	(0.216)	(0.118)	(0.046)	(0.019)
11	0.126	0.441***	0.027	0.070***
	(0.196)	(0.115)	(0.041)	(0.018)
12	0.253	0.220*	0.054	0.036*
	(0.207)	(0.121)	(0.043)	(0.019)
Constant	3.167***	2.521***	3.286***	2.434***
	(0.321)	(0.137)	(0.249)	(0.149)
Observations	2,445	5,683	2,445	5,683
R^2	0.245	0.238	0.243	0.235
No. of bond measures	228	632	228	632

Note: Fixed effects of relative months, calendar months, bond measures, and a cubic function of vote share are included but not shown. Robust standard errors, clustered at the jurisdiction level, are in parentheses.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

for the city and county sample; Columns 2 and 4 report the results for the school district sample. As shown in Columns 1 and 2, passing a bond measure has positive and statistically significant effects on bond yields. For the city and county sample, the bond passage shows positive and statistically significant effects one month ($p < 0.1$) and seven months ($p < 0.05$) after bond referenda. For the school district sample, the bond passage shows positive and statistically significant effects except for the first month ($p < 0.1$) after bond referenda.[8]

Given that we estimate the month-specific effects of bond passage for 12 months, we take two steps to assess the overall statistical significance of these results. First, we conduct a joint F-test for all the 12 coefficients. Second, we apply the Bonferroni correction for the p values to account for the influences of multiple hypotheses testing.[9] For the city and county sample, the F-test statistic is 1.74 and reaches statistical significance at the $p < 0.1$ level. The results with the Bonferroni correction show no statistically significant effects of bond passage. Taken together, we conclude that bond passage shows minimal effects on the average bond yields for cities and counties. By contrast, for the school district sample, the F-test statistic is 2.94 and statistically significant at the $p < 0.01$ level. Moreover, the pattern of the results remains unchanged with the Bonferroni correction. This suggests that passing a bond measure increases government credit risk perceived by bond investors for school districts.

Based on the results in Columns 1 and 2 of Table 3, Figure 3 shows the effects of passing bond referenda on bond yields in the subsequent 12 months. Panel A of Figure 3 shows the impact of bond passage for the city and county sample, which is consistent with the interpretation of a lack of market responses. Panel B of Figure 3 shows that the effects of bond passage on bond yields range from 0.220 to 0.524 in the 12 months after a bond referendum for the school district sample. This suggests that passing a bond measure increases bond yields by 22–52.4 basis points during the subsequent 12 months.

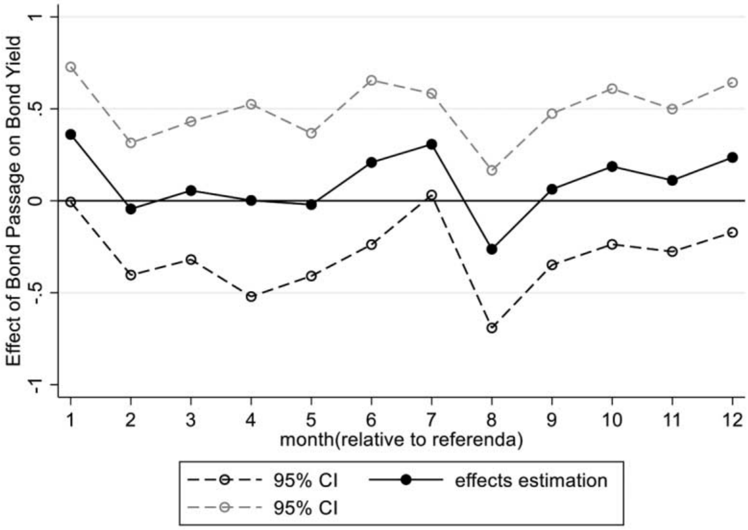
In addition, the impact of passing bond referenda may vary by the authorized bond amounts on the ballot. The credit risk can go up as the authorized bond amounts increase. The key independent variable is changed to authorized bond amounts per capita from the dummy variable that indicates bond passage to examine the impact of bond passage with varying authorized bond amounts. The rest of the model is kept intact. Following Cellini, Ferreira, and Rothstein (2010), the model is then estimated with an instrumental variable regression, using bond passage as an instrument for the authorized bond amount.

Columns 3 and 4 of Table 3 show the effects of authorized bond amounts on bond yields 12 months after bond referenda. The pattern of the results is consistent with that in Columns 1 and 2. Column 3 shows that, for the city and county sample, the authorized bond amount has positive and statistically significant effects ($p < 0.05$) one

[8] At the $p < 0.05$ level, the bond measure shows positive and statistically significant effects between three and 11 months after bond referenda for the school district sample.

[9] The results for the F-test and the Bonferroni correction are reported in Table OD1.

A



B

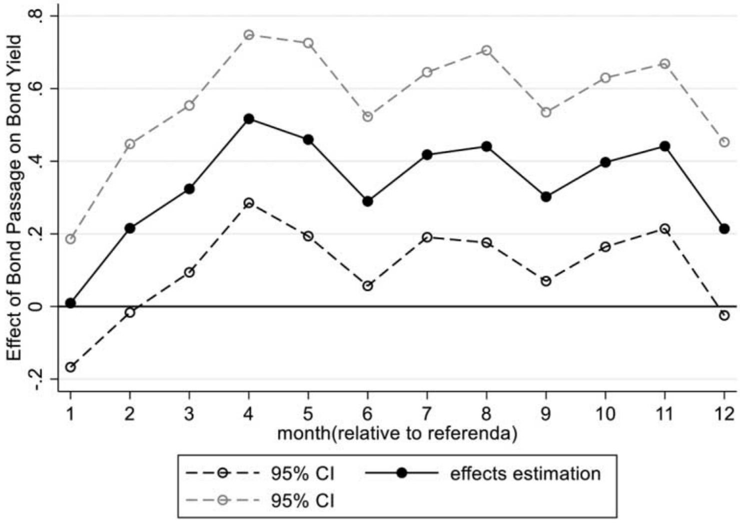


Figure 3. Effects of bond passage on bond yields for 12 months after bond referenda. Panel *A* depicts the bond passage effects for cities and counties. Panel *B* depicts the bond passage effects for school districts.

month and seven months after a bond referendum. Again, the results of the joint F-test and the Bonferroni correction indicate that these effects are weak. Specifically, while the F-test statistic is statistically significant at the $p < 0.05$ level, the results with the Bonferroni correction show no statistically significant effects of the authorized bond amount.

Column 4 shows that the authorized bond amount has positive and statistically significant effects ($p < 0.1$) except for the first month after bond referenda for the school district sample. The sizes of the effects range from 0.036 to 0.082. Because the variable of the authorized bond amount per capita is measured in its natural log form, this indicates that one standard deviation increase (about 274 percent) in authorized bond amount per capita increases average bond yields by 9.9–22.5 basis points. These effects are smaller but comparable to that of bond referenda passage.

B. Robustness Checks

To test whether our findings are consistent as opposed to idiosyncratic based on our modeling and specification choices, we conduct several robustness checks for the main results.[10] First, we use alternative polynomial functions of vote share in the estimation. While we use a cubic function of vote share in the main model, we show that the main results are robust with the control of linear, quadratic, or two-part cubic functions of vote share, as reported in Table A1. Second, we use alternative weights in aggregating bond yields from the bond transaction level to the jurisdiction level. In the main model, we weigh the average bond yields by time to maturity of the transacted bonds. We test the sensitivity of the main findings with weighting by trade frequency and par value traded (Gao, Lee, and Murphy, 2019, 2020) as well as using no weights. As shown in Table A2, the results remain consistent.

VIII. DISCUSSION AND CONCLUSION

Using an RD design, we estimate the causal impact of passing bond referenda on market-perceived local government credit risk as indicated by the average bond yields of existing debt. Overall, the results show that bond referenda passage increases the market assessment of government credit risk for Texas school districts. Specifically, one standard deviation increase in authorized borrowing amount per capita causes bond yields to increase by approximately 9.9–22.5 basis points in the first 12 months after bond referenda for school districts. Because the sample average of bond yields is about 2.73 percent, the sizes of the effects represent increases by about 3.6–8.2 percent. The sizes of the effects are larger than those reported in prior studies on secondary bond market responses, which find an effect of 5.5–6.4 basis points for state policies for fiscally distressed municipalities (Gao, Lee, and Murphy, 2019)

[10] To save space, we report additional robustness checks in the Appendices A–D.

and an effect of about 6.4 basis points after a newspaper closure (Gao, Lee, and Murphy, 2020).

We find that the effects of bond referenda passage persist over a range from 2 to 12 months for school districts. The market responses are not immediate, which seems not surprising given that the secondary bond market is relatively thin. For cities and counties, the lack of effects may result from imprecise estimation on a small sample size or a low level of market efficiency. Future research may improve statistical power to further test the market responses for cities and counties. Taken together, the secondary bond market shows a degree of efficiency in that it responds to the information of bond referenda passage, which is consistent with existing studies on the market responses to policy changes and external shocks (Fischer, 1983; Marquette and Wilson, 1992; Denison, 2006; Gao, Murphy, and Qi, 2019; Chalmers, Liu, and Wang, 2021).

Anecdotal evidence suggests that the gap of time from bond referenda to bond issuance has a minimum of one month for school districts (Texas Association of School Boards, 2020) in Texas, but it can take much longer. The effects of bond passage by month seem to coincide with the new bond issuances for school districts, the impact of which may be captured by the secondary bond market responses. In the case of market responses ahead of the new debt issuances, it might reflect market efficiency in that bond investors adjust to an expected change in debt levels before it materializes. Future research may examine the impact of the new bond issuances authorized in bond referenda on bond yields of the existing debt to provide direct evidence.

The positive effects of bond authorization on bond yields for school districts are consistent with the market discipline thesis (Lane, 1993). Because bond authorization indicates a higher level of future debt, the secondary market could temper a government's taste for new debt by offering less for their existing bonds. Traders and investors may observe a bond notice or bond referenda result and draw the conclusion that the debt of that local government is increasing, which, all else equal, it will. It is often the case that governments retire or refund old debt before issuing new debt, with a negligible effect on debt levels. So this study reports on the market response to a signal of new impending debt issuances or capital investments, not necessarily the (net) effect of more debt.

The findings have important policy implications. First, the positive effects of bond authorization on bond yields for school districts suggest that passing (failing) a bond measure creates capital losses (gains) for the bond investors who hold existing bonds from that local government. Second, the delay in the secondary bond market responses suggests that thinly trading markets may be slow to absorb information. One policy response is to increase information disclosure regarding the bond measures. For instance, local governments may find it helpful to reach out to those market participants in an effort to explain how the new debt relates to existing debt or how they are managing the burden of additional obligations. Third, the positive effects of bond passage for school districts indicate that local governments may need to consider effects on credit quality when passing bond referenda.

In August 2019, the GASB began a Pre-Agenda Research Project on Interim Financial Reporting to evaluate the potential for new guidance requiring monthly, quarterly, or semiannual financial reporting (only annual reporting is presently required with financial statements typically released six or more months after the conclusion of a fiscal year). Implementing a change like this would come at a substantial cost to local governments. The costs include those for auditing fees and the substantial internal resources needed to generate interim financial statements on a reporting basis that they would not otherwise use. Governments benefit little from interim statements under the GASB standards because they are designed to be decision-useful for external audiences while managers and elected officials rely on other forms of financial reporting to manage the enterprise. For example, government officials typically use cash basis and budgetary basis accounting for budget planning, execution, and evaluation. The benefits of interim reporting for investors, particularly those who actively trade municipal bonds, rest on the ability to remediate an information asymmetry that exists because governments possess potentially relevant information about their interperiod changes in financial condition that go unpublished until the close of the fiscal year.

The absence of any evidence regarding how market participants change their assessments of governments based on interim information makes it hard to substantiate benefit claims from such regulations. Municipal bond defaults are rare and involve mostly credits well known to be distressed (Fortune, 1991; Holian and Joffe, 2013; Standard and Poors, 2020; Fitch Ratings, 2021a), but changes in financial condition that have the potential to affect repayment ability are more frequent (Fitch Ratings, 2021b). Bond rating transitions signal investors of new information about a government, but they can take a long time to be discovered and published. We evaluate a common occurrence, the announcement of a new borrowing authorization, and find that markets do in fact respond, at least to this particular kind of interim information. This seems to bolster the potential argument for the benefit claims of semiannual financial reporting for investors.

Making the link to the actual costs imposed on local government issuers once the authorized borrowing occurs lies beyond the scope of this paper but is a logical next step for the work. Ideally, we would like to see the effect on the borrowing costs of an issuer who has an approved authorization. This work brings many challenges. While we can observe yields on bonds that are trading openly in markets, local governments experience and assess borrowing costs differently. The cost of borrowing on an overall debt issue must account for multiple maturities, discounts, premiums, and other features like issuance costs that make maturity by maturity yield comparisons imprecise. The true interest cost (TIC) used when evaluating bids for bond sales is a better measure of the cost of borrowing as governments experience it but is often not even reported due and thus must be calculated individually from the closing documents from each sale, which often omit relevant information. The information provided here should motivate future work to explore this connection.

More broadly, this article opens several avenues for future research. The jurisdictions without bond transactions are omitted from the study because their bond yields are unobserved in the secondary bond market data from MSRB. The present findings should be qualified to those with both bond referenda and bond transactions to the extent that the local governments that issue the traded bonds differ in unobserved ways. Moreover, while the RD design has the unique strength of estimating local average causal effects, one needs to use caution in interpreting the empirical results by not extrapolating them to bond referenda that pass or fail by large margins. Because the bond passage is the focal point of this article, the local causal estimates have critical scholarly and policy relevance. Nonetheless, additional studies are needed to test the external validity of the findings.

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DISCLOSURES

The authors have no financial arrangements that might give rise to conflicts of interest with respect to the research reported in this paper.

APPENDIX: SELECTED RESULTS OF ROBUSTNESS CHECKS

Table A1

Effects of Bond Passage on Bond Yields for 12 Months with Alternative Forms of Polynomial

Relative Months after Referenda	Linear		Quadratic		Two-part Cubic	
	(1) City and County	(2) School District	(3) City and County	(4) School District	(5) City and County	(6) School District
1	0.429*	0.028	0.392*	0.013	0.095	−0.038
	(0.229)	(0.109)	(0.204)	(0.099)	(0.361)	(0.188)
2	−0.006	0.248*	−0.025	0.235*	−0.201	0.323
	(0.220)	(0.135)	(0.197)	(0.126)	(0.420)	(0.305)
3	0.048	0.340**	0.063	0.344***	−0.216	0.378
	(0.199)	(0.137)	(0.194)	(0.126)	(0.657)	(0.249)
4	0.016	0.573***	0.013	0.550***	−0.405	0.395*
	(0.279)	(0.134)	(0.270)	(0.124)	(0.322)	(0.215)
5	0.065	0.462***	0.031	0.467***	0.247	0.831***
	(0.233)	(0.152)	(0.212)	(0.142)	(0.386)	(0.306)
6	0.221	0.296**	0.216	0.303**	0.428	0.590**
	(0.279)	(0.142)	(0.249)	(0.129)	(0.323)	(0.239)
7	0.405**	0.460***	0.356**	0.441***	0.117	0.477**
	(0.163)	(0.136)	(0.151)	(0.125)	(0.391)	(0.205)
8	−0.283	0.462***	−0.269	0.459***	0.085	0.817***
	(0.253)	(0.161)	(0.233)	(0.147)	(0.326)	(0.299)
9	0.071	0.274*	0.070	0.292**	0.003	0.786***
	(0.238)	(0.140)	(0.222)	(0.129)	(0.425)	(0.237)
10	0.197	0.463***	0.195	0.429***	0.783**	0.342
	(0.249)	(0.138)	(0.230)	(0.128)	(0.384)	(0.274)
11	0.119	0.480***	0.125	0.458***	0.288	0.506**
	(0.229)	(0.133)	(0.210)	(0.124)	(0.380)	(0.241)
12	0.360	0.280**	0.299	0.247*	−0.272	0.086
	(0.225)	(0.136)	(0.215)	(0.128)	(0.437)	(0.324)
Constant	3.191***	2.522***	3.174***	2.523***	3.224***	2.526***
	(0.366)	(0.137)	(0.323)	(0.137)	(0.441)	(0.137)
Observations	2,445	5,683	2,445	5,683	2,445	5,683
R^2	0.245	0.238	0.245	0.238	0.249	0.241
No. of bond measures	228	632	228	632	228	632

Note: Columns 1 and 2 report results using a linear function of vote share; Columns 3 and 4 report results with a quadratic function of vote share; Columns 5 and 6 report results with a separate cubic function of vote share on each side of the cutoff of 50 percent. Fixed effects of relative months, calendar months, and bond measures are included but not shown. Robust standard errors, clustered at the jurisdiction level, are in parentheses.

* $p < 0.10$.

** $p < 0.05$.

*** $p < 0.01$.

Table A2

Effects of Bond Passage on Bond Yields for 12 Months: Weighted Average Yields

Relative Months after Referenda	Unweighted: City and County (1)	Unweighted: School District (2)	by Trade Frequency: City and County (3)	by Trade Frequency: School Districts (4)	by Par Value Traded: City and County (5)	by Par Value Traded: School Districts (6)
1	0.361*	0.009	0.267	−0.004	0.352*	−0.002
	(0.186)	(0.090)	(0.219)	(0.109)	(0.188)	(0.089)
2	−0.044	0.215*	−0.042	0.267**	−0.043	0.227*
	(0.182)	(0.118)	(0.220)	(0.134)	(0.185)	(0.119)
3	0.056	0.324***	0.108	0.452***	0.060	0.318***
	(0.190)	(0.117)	(0.222)	(0.130)	(0.193)	(0.118)
4	0.002	0.517***	−0.046	0.550***	0.015	0.508***
	(0.265)	(0.118)	(0.291)	(0.123)	(0.265)	(0.116)
5	−0.021	0.460***	−0.040	0.498***	−0.013	0.447***
	(0.197)	(0.135)	(0.244)	(0.156)	(0.198)	(0.136)
6	0.209	0.290**	0.164	0.318**	0.243	0.284**
	(0.226)	(0.119)	(0.259)	(0.138)	(0.228)	(0.120)
7	0.307**	0.418***	0.369**	0.402***	0.341**	0.406***
	(0.140)	(0.116)	(0.181)	(0.128)	(0.138)	(0.116)
8	−0.263	0.441***	−0.331	0.473***	−0.225	0.426***
	(0.217)	(0.135)	(0.263)	(0.144)	(0.215)	(0.135)
9	0.063	0.302**	0.066	0.335**	0.099	0.302**
	(0.208)	(0.118)	(0.249)	(0.135)	(0.209)	(0.120)
10	0.186	0.397***	0.136	0.382***	0.202	0.388***
	(0.214)	(0.118)	(0.250)	(0.132)	(0.213)	(0.119)
11	0.111	0.441***	0.213	0.415***	0.142	0.430***
	(0.196)	(0.116)	(0.244)	(0.128)	(0.201)	(0.116)
12	0.236	0.214*	0.352	0.174	0.279	0.189
	(0.206)	(0.121)	(0.242)	(0.132)	(0.209)	(0.121)
Constant	3.263***	2.520***	3.192***	2.497***	3.209***	2.492***
	(0.253)	(0.140)	(0.291)	(0.134)	(0.282)	(0.140)
Observations	2,445	5,683	2,457	5,671	2,457	5,671
R^2	0.247	0.235	0.213	0.220	0.248	0.235
No. of bond measures	228	632	230	630	230	630

Note: Fixed effects of relative months, calendar months, bond measures, and a cubic function of vote share are included but not shown. Robust standard errors, clustered at the jurisdiction level, are in parentheses.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

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