Should governments prohibit negotiated sales of municipal bonds?

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Abstract

Legislation in several states bans negotiated sales of municipal bonds. We hand collect the legal provisions on the sale of 281,913 school bonds across 40 states. Sales restrictions lower offering yields by 13 bps. The use of negotiated sales increases yields by 15–17 bps when they are allowed. Yet, over 80% of unconstrained issuers use negotiated sales. The evidence suggests that issuance choices depend mainly on non-yield benefits and that sales restrictions improve the industrial organization of the underwriting sector. A nationwide restriction would have saved free issuers $3.6 billion between 2004 and 2013.

Keywords: Primary market, state laws, sales method, municipal bonds

Classification: H3, H7, G1

1. Introduction

Municipalities issue over $400 billion in new issues each year with municipal bonds financing more than 70% of state and local government infrastructure investment. Some issuers use negotiated sales where they precommit to an underwriter and some use competitive sales where they auction off the securities. A long-standing question is which sales method leads to higher proceeds for the issuer? The municipal bond market offers a unique setting to study sales methods not only because of its size and variation in the use of competitive and negotiated sales, but also because several states restrict the use of negotiated sales for certain bonds. Legal restrictions on sales methods can be costly or beneficial to issuers depending on whether the constraints they impose dominate the inefficiencies they eliminate.

We estimate that restrictions on the use of negotiation lead to 1% higher proceeds for issuers because of two channels—issuers do not consider only yield in their sales choices and state-level restrictions change the nature of competition in the underwriting sector. We obtain our estimates using hand-collected data on the legal restrictions for 281,913 municipal bonds issued by 8,332 independent school districts in 40 states and the history of amendments to the sales provision. The restrictions are predetermined at the time of issuance, allowing us to estimate causal impacts of the legal restriction to using competitive sales on offering yields and causal impacts of the choice of sales method on offering yields when negotiated sales are allowed.

Several decades ago, legislatures put restrictions on negotiated sales to enhance government transparency. Following a number of corruption scandals, the presumption was that auctions were optimal and that restrictions were necessary. Competitive sales can lead to efficient allocations (Bulow & Klemperer 1996). An argument against restrictions is that negotiated sales incentivize underwriters to invest in gathering information from investors to increase sales proceeds (Benveniste & Spindt 1989, Benveniste & Wilhelm 1990, Spatt & Srivastava 1991, Biais & Faugeron-Crouzet 2002, Biais et al. 2002, Sherman 2005). Yet, negotiated sales for municipal bonds may provide scope for corruption (Butler et al. 2009, Brown 2017). Underwriters may make campaign contributions to politicians increasing costs. Auctions can curb the impact of political contributions on municipal bond issuers. Forcing competitive sales can also affect the industrial organization of the underwriting sector by diminishing entry barriers and frictions arising from underwriter specialization and capital immobility (Duffie 2010, Duffie & Strulovici 2012, Garrett et al. 2017, Cestau 2020). Little empirical evidence exists on how sales methods affect issue proceeds for general securities issues, because few new securities other than U.S. municipal bonds are sold via competitive sales worldwide (Jagannathan et al. 2015).

Municipal school bonds provide a good empirical laboratory to study how sales methods affect yields for several reasons. Schools are the second-largest U.S. public infrastructure investment so reducing interest costs is economically important. Schools have similar simple revenue structures, they issue similar plain-vanilla bonds, and the restrictions cannot be modified by the issuer. Underwriter services are important for municipal bond issuers, in part because the municipal market is fragmented and opaque (Butler (2008), Green, Hollifield & Schürhoff (2007), Green (2007) and Cestau, Green & Schürhoff (2013)). Butler, Fauver & Mortal (2009) and Brown (2017) argue that negotiated sales may allow local politicians to extract private benefits from underwriter choice, which may be particularly important for local issuers such as school districts. We can determine if school bond issuers are allowed to use negotiated offerings for different bond types, allowing us to identify the impact of the sales restriction on yields.

The average yield for restricted bonds is 3.02% and the average yield for free bonds is 3.11% in our sample of school bonds. The treatment effect from restricting negotiated sales is between minus 13 and 20 basis points. The restriction is valuable to issuers with the potential savings from restricting the use of negotiated sales largest following the financial crisis after several large underwriters disappeared.

Where issuers are allowed to use negotiated sales, competitively sold bonds have average yields of 2.95% while negotiated bonds have average yields of 3.16%. Yet, about 80% of the issuers choose negotiated sales when they are allowed to do so. We use an instrumental variable approach to estimate the effect of switching between competitive and negotiated sales on offering yields. The statutory bans on negotiated sales allow us to choose and test potential instruments. We choose instrumental variables that predict the choice of sales methods, but that do not predict competitive yields when issues are restricted (this is our zero-first-stage test; see Van Kippersluis & Rietveld (2018)).

The effect of switching from negotiated to competitive sales reduces yields by around 16 basis points. Our IV estimates for the cost of negotiated sales are broadly consistent with our results on the benefits of the restrictions. We interpret this as evidence that issuers do not only consider yields when they choose the sales method, but to a large extent also consider non-yield benefits. As a consequence, issuers make choices that do not maximize bond proceeds. Restricting negotiated sales reduce school bond financing costs. The effect is the largest after the financial crisis when issuers appear to make issuance choices independent of yield considerations and when the industrial organization effect of the sales restriction is the largest.

In counterfactual analysis we measure the effect on bond proceeds if a negotiated sales restriction were imposed on all free bonds, and the effect on negotiated bond proceeds if all negotiated bonds were sold competitively. Bond proceeds would increase by about 1.05% if free bonds were restricted. Gross spreads would increase by 0.20%, leaving a total benefit of 0.85%. Bond proceeds would increase by about 1.24% if all free negotiated deals were issued competitively.

Several authors use municipal bond data to study whether competitive or negotiated sales lead to higher yields for the issuer, finding mixed results. Liu (2017), Guzman &

Moldogaziev (2012), Robbins & Simonsen (2007), and Robbins & Simonsen (2015) find that auctions lower yields,[1] while Fruits, Booth, Pozdena & Smith (2008), Kriz (2003), and Peng & Brucato (2003) find that negotiated sales increase yields. The mixed nature of these findings reflects the difficulty in dealing with the selection effects arising from issuers' endogenous decisions. Using novel data on legal restrictions, our approach allows to use empirical variation from restricted and free issues, which we believe improves the credibility of our estimates.

The remainder of the paper is organized as follows. Section 2 documents the sales laws for fiscally independent school district (ISD) governments and describes the sample of newly issued ISD bonds. Section 3 derives implications for issuers' choice of sales method from the observed yield differences between restricted and free bonds and the revealed preference for negotiated sales. Section 4 estimates the causal effect of the sales restriction on yields. Section 5 employs an instrumental variables approach to estimate the effect of switching from competitive to negotiated sale, or vice versa. Section 6 summarizes the counterfactual analysis. Section 7 concludes.

2. Sales laws and ISD bonds

We use a sample of bonds issued by fiscally independent school district (ISD) governments. School districts are regularly organized as independent governments in 43 states. They account for 91% of all public school systems in the country. ISDs are simple governments; they are stand-alone entities separate from other municipalities including cities and counties; they are more homogeneous than city and county bond issuers; they are structurally similar across states; and they generally collect only ad-valorem property taxes. ISD bonds account for 28% of all municipal bond issues in terms of number of deals and 20% in terms of notional amount. ISDs generally issue simple plain-vanilla

[1] Cestau (2019) finds that auctions lead to lower concentration in the underwriting industry.

bonds. Moody's is starting with ISD bonds to recalibrate their credit rating assessment of municipal bonds as of September 2020. The sales procedures are regulated by public state laws and cannot be modified by the school districts. Unlike ISDs, states and many municipalities can adjust their sales procedures as their needs and preferences change. Finally, the sales laws are predetermined for any given ISD bond issue, simplifying estimation and identification.

We obtain information for all ISD bonds issued between 1990 and 2014 from Thomson Reuter's SDC Platinum database. We use the government type identifier in the SDC database and the SDC issuer name variable to identify 62,844 new-money and refunding ISD bond deals. We use this school deal sample to construct several instrumental variables. ISD deals generally comprise a series of individual bonds. SDC Platinum provides offering prices for each individual bond, and issuer, deal, and bond characteristics. Price data in SDC is limited prior to July 2003 with only 1.4% of deals having offering prices. While we keep all available data, our results are more representative of the 2003–2014 period. After applying the filters described in Appendix A.1, our ISD school bond sample contains 281,913 individual bonds in 26,683 deals issued by 8,332 ISDs in 40 states.

2.1. Bond types and enabling laws

We identify three types of school bonds. There are two basic bond types in every state, general new-money bonds and general refunding bonds, and in some states, ISDs can also issue alternative new-money bonds. Bond types are described in state laws called *enabling laws* that set out the bond type characteristics including the list of permitted purposes for the bond issue, the procedures to approve a bond issue, and other provisions. The bond type depends on the purpose of the bond issue. For example, an issuer must use new-money bonds to finance new capital expenditures and use refunding bonds to refinance a previous bond issue.

Generally, voters are required to approve new-money issues and extraordinary property taxes to pay the bonds at a bond election. The ballot measure must include the purpose for the bond issue, the issue amount, total debt servicing costs, and the additional property taxes required to pay the debt.[2] Extraordinary property taxes are transferred to a debt fund, and can only be used to pay the bonds. Bond proceeds are transferred to special funds and can only be used for the purposes approved at the bond election. Total proceeds are determined at the bond election, but by state law cannot be higher than 105% of the underlying project cost. Debt service must be roughly evenly distributed over the useful life of the project defined by law. The issuer has little flexibility in selecting the maturity structure. Municipal bond deals use a series of sequential bond maturities to meet the legal timing constraint on debt service.

Some state laws provide for alternative new-money bonds in separate enabling laws. Alternative new-money bonds enable the school to advance certain pre-approved special taxes collected into special funds that are legally committed to certain purposes. Alternative bonds are either payable solely from these special taxes or have the same security as general new-money bonds coupled with an offsetting reduction in said special taxes. The bonds do not create an additional burden on taxpayers and do not require approval at a referendum. Proceeds from the bonds are transferred to the project's fund and can only be used to finance the project. Alternative new-money bonds represent 10% of all new-money bond issues. Appendix A.2 provides more details on alternative new-money bond types.[3]

[2] In most states, these additional property taxes are collected separately from the general levy. State laws sometimes provide for election waivers in separate enabling laws for several essential capital expenditures including those related to environmental cleanup, accessibility, life safety, transportation, and school equipment.

[3] State laws can also create complementary new-money bond types for specific projects not included in the general enabling laws: pension bonds, judgment bonds, and working cash fund bonds. However, these are seldom used.

We hand collect the enabling laws for each deal in the school deal sample to identify the bond type. We obtain the section numbers of the statutes containing the enabling laws from the official statements accompanying every new issue available online at the FINRA designated official source for municipal securities data, EMMA, and we obtain the texts of the statutes from the websites of the state legislatures and two repositories of legal documents, Justia.com and lexisnexis.com.

We hand collect the legal provisions relating to the sale of bonds for each bond type. Sales provisions may be stipulated in the enabling law of the bond type, or in another enabling law by cross-reference, or in other sections providing for the sales of school bonds or municipal bonds generally, or the provisions may not be provided anywhere. In cases where the sales laws impose a restriction on negotiated sales, the laws often provide for exceptions to the law based on taxable status, deal amount (below one or two million), coupon type, and, occasionally, maturity (under one or two years to maturity). We refer to these as TACM exceptions.

We hand collect the history of amendments to each sales provision, the texts of repealed sales provisions, and the history of amendments to the repealed sales provisions when available. We obtain the texts of repealed and amended laws from the repositories of House and Senate bills available online at each state legislature. Bills are generally available from at least 1997. The sales provisions are sometimes amended and sometimes repealed altogether. Table 1 documents the law changes between 1990 and 2014. We observe seven states passing TACM exceptions—five between 2009 and 2011 allowing for the negotiated sale of taxable bonds to facilitate the implementation of the taxable bonds created by the American Recovery and Reinvestment Act of 2009. We observe five states relaxing the general sales laws in our school deal sample.[4] Due to the lack of yield data before 2003, only Alabama and Montana provide meaningful time-series variation in the

[4]Tennessee also changed its general laws on 06-03-2009 but we are unable to assign the enabling laws for the 61 Tennessee deals in the sample so is not reported in Table 1.

Table 1: Time series variation in sales laws. The left column indicates state and date of restriction end. The right column shows state and date of exceptions passed or relaxed based on TACM (**T**axable, notional **A**mount, non-fixed **C**oupon, short **M**aturity).

State	Bond type restriction relaxed	TACM exception passed or relaxed
OR	09-25-1991	–
TX	06-19-1999	–
ID	06-01-2001	–
NM	–	04-05-2005
KS	–	07-01-2008
NM	–	04-07-2009
NV	–	04-07-2009
MT	–	06-01-2009
SC	–	06-03-2009
NY	–	04-25-2010
AL	01-01-2011	–
MT	03-01-2011	–

general sales provisions in our school bond sample.[5]

Table 2 reports the heterogeneity in sales restrictions across states and bond types. The table shows the restriction type (free=0 and restricted=1) for general new-money bonds, alternative new-money bonds, general refunding bonds, and TACM bonds in each state, and the number of observations in each category. There is substantial heterogeneity in the general sales laws across new-money bonds in different states. A total of 19 states have restricted new-money bonds[6] and 24 states have free new-money bonds. Many states have heterogeneous sales laws across bond types. A total of 16 states have restricted general new-money bonds and free refunding bonds at the same time; ten states offer TACM exceptions for restricted bond type; sales laws for general and alternative new-money bonds are the same in all but 3 states and partially 2. A total of 18 states have cross-sectional variation in sales laws.

[5]We only observe a handful of deals with yield data before the law change in Texas, and no observations for Idaho and Oregon.

[6]22 in the school deal sample. Besides Oregon and Idaho that changed the law before July 2003, Vermont restricts all negotiated sales. However, we only observe Vermont data before July 2003.

Table 2: Sales laws across states, bond types, and time. The table reports the sales law restrictions (free=0 and restricted=1) by bond type for general new-money bonds, alternative new-money bonds, and general refunding bonds (columns 1–3), the number of bonds issued under these sales law restrictions excluding bonds that qualify for a TACM exception (columns 4–6), the number of bonds that qualify for a TACM exception (column 7), and the number of restricted and free bonds in the final sample (columns 8–9). – indicates not applicable. * indicates TACM exception passed or relaxed during sample period. State names followed by year ranges indicate law changes during sample period.

	Sales law restriction			Number of bonds					
	by bond type			by bond type ex TACM			TACM	by restriction	
	New-money		General	New-money		Gen.			
State	General	Alternative	refunding	Gen.	Alt.	refund.		Restricted	Free
AL 90–11	1	–	1	234	–	261	–	495	0
AL 11–14	0	–	0	534	–	627	–	0	1,161
MT* 90–11	1	–	0	462	–	201	34	462	235
MT 11–14	0	–	0	110	–	116	–	0	226
TX 90–99	1	0	0	18	0	49	–	18	49
TX 99–14	0	0	0	22,216	513	26,972	–	0	49,701
IA	1	0	0	2,542	2,776	1,807	–	2,542	4,583
WI	1	0	0	1,170	1,116	7,773	0	1,170	8,889
LA	1	0/1	0	2,771	324	1,319	–	2,967	1,447
MS	1	0/1	0	429	253	999	–	429	1,252
MN	1	1	0	2,350	1,871	3,916	1,634	4,221	5,550
ND	1	1	0	501	244	350	0	745	350
NV*	1	1	1	614	20	354	88	988	88
IN	1	–	0	901	–	126	–	901	126
KS*	1	–	0	1,627	–	3,515	84	1,627	3,599
NJ	1	–	0	3,972	–	5,374	1	3,972	5,375
NM*	1	–	0	2,241	–	584	21	2,241	605
NY*	1	–	0	9,871	–	7,758	2,561	9,871	10,319
OK	1	–	0	5,174	–	3	–	5,174	3
WV	1	–	0	231	–	29	–	231	29
SC*	1	–	1	1,516	–	938	12	2,454	12
AR	1	–	1	3,459	–	8,284	–	11,743	0
AZ	0	0	0	3,467	82	961	–	0	4,510
CA	0	0	0	22,330	3,133	11,218	–	0	36,681
FL	0	0	0	0	206	131	–	0	337
IL	0	0	0	8,778	1,379	7,502	–	0	17,659
MI	0	0	0	4,383	271	5,061	–	0	9,715
NE	0	0	0	1,434	1,195	2,715	–	0	5,344
OH	0	0	0	2,756	295	4,320	–	0	7,371
SD	0	0	0	387	1,198	1,432	–	0	3,017
WA	0	0	0	2,554	81	2,994	–	0	5,629
CO	0	–	0	1,929	–	2,832	–	0	4,761
CT	0	–	0	314	–	307	–	0	621
GA	0	–	0	1,301	–	473	–	0	1,774
ID 01–14	0	–	0	1,146	–	771	–	0	1,917
MA	0	–	0	1,080	–	238	–	0	1,318
ME	0	–	0	94	–	21	–	0	115
MO	0	–	0	4,060	–	5,918	–	0	9,978
NH	0	–	0	153	–	110	–	0	263
PA	0	–	0	9,857	–	9,849	–	0	19,706
OR 91–14	0	–	0	1,475	–	1,741	–	0	3,216
UT	0	–	0	1,317	–	645	–	0	1,962
WY	0	–	0	147	–	22	–	0	169

2.2. Statutory security and other controls

Statutory security defines the sources of revenue that the issuer must legally use to secure bond payments. The legislator that writes the sales provisions for a bond type may factor in its statutory security when they decide on sales provisions. We therefore need to use statutory security as a control in our empirical models.

The issuer cannot use other revenues to provide for the payment of the bonds than those established by the statutes. We hand collect the statutory security data from the enabling laws and the official statements, and we classify it following the approach in Cestau, Hollifield, Li & Schürhoff (2018). We recognize two categories of low-quality general obligations, two categories of higher-quality revenue bonds, and two highest-quality categories of general obligations. Appendix A.3 expands on the statutory security classification. Table A.1 in the Appendix, reports the statutory security of general and alternative new-money bonds in our school deal sample. Statutory security is homogeneous across states in our sample. A total of 84.5% of school deals are secured by an unlimited and specific pledge of ad-valorem property taxes plus the full faith and credit of the issuer.

In addition to statutory security, we hand collect the legal requirements for public sales and rate them based on their flexibility on a scale from 0 to 3. We also hand collect data on the state enhancement programs specially developed for school bonds from the official statements and the laws that create them, and rate the enhancement programs on quality between 0 and 1. Appendix A.4 provides detailed data on public sales procedures and state enhancement programs. Appendix A.5 provides detailed information on the data collection process for the enabling laws, sales laws, amendments thereof, and statutory security. Over 150 bond lawyers helped us to construct these data.

The legislator writing the sales provisions may also factor in other non-statutory characteristics of the bonds. We collect from SDC Platinum additional issuer, deal, and bond level variables our specifications include as controls. At the issuer level, we control for issuer size using quantiles of the total notional amount issued by the municipality between 1990 and 2014. At the deal level, the controls include the natural logarithm of deal size, date of issue, a taxable indicator, a bank-qualified indicator, a series/term indicator, a sinkable indicator for term bonds, a multiple deals in one issue indicator, the difference between deal size and total issue size for multiple deals issues, and a callable deal indicator. At the bond level, the controls include coupon type, a non-callable-bond indicator, the bond's par amount, years-to-maturity fixed effects, an indicator for non-rated bonds, and credit rating dummy variables. We also collect the gross spread of the deal which is composed of management fee, underwriting fee, takedown, and expenses.

2.3. Summary statistics and univariate analysis

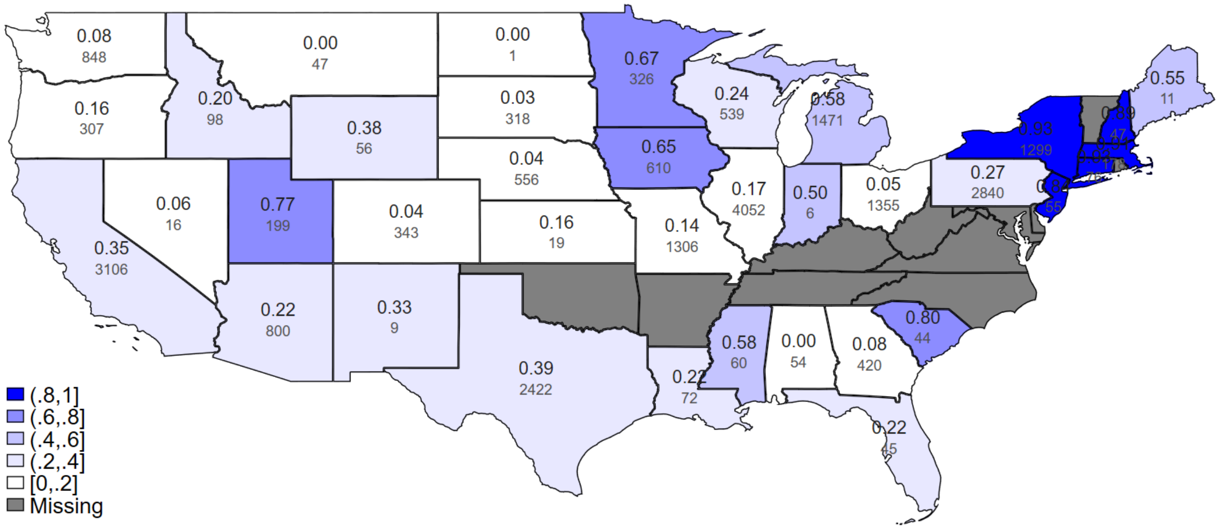
Figure 1 shows the share of competitive sales by state and refunding type between 1990 and 2014 for the sub-sample of free school deals. Competitive sales account for less than 20% of the sales in most states and more than 80% in only a few states.[7] Table 3 shows summary statistics in the sample of school bonds with yield data. Free bonds have average yields of 3.11% compared to 3.02% for restricted bonds giving a 9 bps spread.[8] The last set of columns in Table 3 shows that the offering yields in negotiated sales are on average 21 bps higher than in competitive sales for the sub-sample of free bonds, and gross spreads are 1 bps higher. However, free issuers prefer negotiated sales; only 20% of all free bonds are sold competitively.[9] Issuers' revealed preference for negotiated sales suggests that a ban on negotiated sales would be costly.

[7]A high proportion of the competitive sales of new-money bonds in New York, Pennsylvania, and Minnesota are private competitive sales. In a private competitive sale, the underwriter must be invited to bid. The SDC database does not distinguish between public competitive and private competitive sales.

[8]Even larger are the yield differences in new-money bonds, as the yields on free bonds are 47 bps higher than on restricted bonds.

[9]We estimate that the number of public competitive sales is around 15%.

Panel A: Share of competitive sales in free new-money bonds



Panel B: Share of competitive sales in free refunding bonds

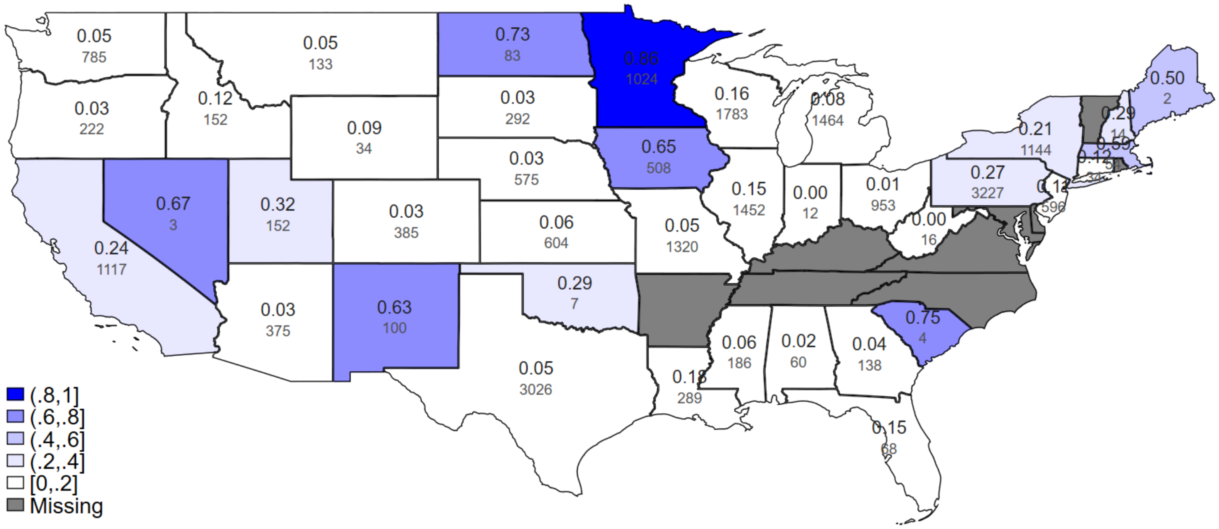


Figure 1: Share of competitive sales in free bonds. The figure shows the share of competitive sales by state and refunding type between 1990 and 2014. Panel A reports shares for free new-money deals, panel B for free refunding deals. Dark numbers indicate proportions, and light numbers sample size. The intensity of the state shade indicates the proportion of competitive sales: 0-20%, 20%-40%, 40%-60%, 60%-80%, 80%-100%.

All control variables, including statutory security and except for only coupon type and deal size, are reasonably balanced between restricted and free bonds, and between competitive and negotiated free bonds. We observe more zero-coupon bonds in free samples, and more specifically, in negotiated samples. The average deal size is similar for competitive and negotiated sales of free bonds, but the average restricted deal is around

Table 3: Summary statistics. The table documents descriptive statistics on the mean and standard deviation (SD) in the final sample and the sub-sample of free bonds. * The empirical fraction of competitive sales in restricted bonds is less than one due to some legal exceptions that we cannot capture systematically.

	Free bonds		Restricted bonds		Free bonds			
					Negotiated		Competitive	
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Yield (%)	**3.11**	1.34	**3.02**	1.19	**3.16**	1.34	**2.95**	1.32
Competitive Sale (0/1)	0.20	0.39	0.97*	0.17	0	–	1	–
Refunding bond (0/1)	0.54	0.50	0.19	0.39	0.57	0.49	0.42	0.49
Alternative new-money (0/1)	0.06	0.23	0.04	0.21	0.05	0.21	0.10	0.29
Statutory security (1–6)	5.74	0.66	5.48	0.88	5.76	0.62	5.64	0.77
Public sale proc. (0-3)	0.43	0.66	1.75	0.58	0.45	0.67	0.35	0.63
State enhancement (0–1)	0.15	0.21	0.08	0.06	0.15	0.21	0.14	0.22
Taxable (0/1)	0.04	0.20	0.02	0.12	0.05	0.21	0.04	0.18
Multiple deal (0/1)	0.08	0.27	0.07	0.26	0.08	0.28	0.06	0.25
Zero coupon (0/1)	0.06	0.24	0.00	0.02	0.07	0.26	0.02	0.13
Bank-qualified (0/1)	0.51	0.50	0.61	0.49	0.50	0.50	0.53	0.50
Callable (0/1)	0.83	0.37	0.83	0.38	0.82	0.38	0.86	0.34
Term deal (0/1)	0.08	0.27	0.10	0.30	0.08	0.28	0.06	0.23
Sinkable (0/1)	0.06	0.24	0.08	0.27	0.06	0.24	0.05	0.22
Issuer size bin (1–5)	3.05	1.41	2.60	1.36	3.01	1.41	3.23	1.41
Bond size ($M)	1.44	9.97	1.03	4.70	1.46	10.00	1.33	9.82
Deal size (ln $M)	2.30	1.28	1.86	1.32	2.32	1.29	2.26	1.23
Issue–deal size ($M)	1.80	27.97	1.46	14.94	1.99	30.89	1.01	7.31
Rated (0/1)	0.94	0.23	0.87	0.34	0.94	0.24	0.96	0.20
Rating (1-10)	2.13	1.52	2.64	1.70	2.11	1.50	2.21	1.60
Maturity (years)	9.26	6.31	9.52	6.21	9.28	6.35	9.20	6.15
Issuance year	2009	3.44	2008	3.48	2009	3.41	2009	3.54
Gross spread (% of deal size)	0.84	0.45	1.11	0.59	0.85	0.44	0.84	0.52
Missing gross spread	39%		80%		35%		54%	

8-10 million lower than free deals. Restricted and free issuers have similar size, however. Overall, restricted bonds have slightly worse credit ratings, enhancement programs, and statutory securities, yet they have lower yields.

The observed yield differences between restricted and free bonds and the revealed preference for negotiated sales despite being less advantageous to the issuer motivate our empirical approach described in the next section. Since we have substantial missing data for gross spreads, we focus exclusively on bond pricing. We compare price differences with gross spreads in Section 6.

3. Sales Restrictions and Yield: A Decomposition

The goal of this section is to show how the restriction on the use of negotiated sales as well as free issuers endogenous choice of sales methods determine the yields observed empirically. The restriction on the use of a negotiated sales is predetermined at the time of issue choice. Define the restriction indicator function

$$\mathbf{R}_i = \begin{cases} 1 & \text{if bond issue } i \text{ is restricted from using a negotiated sale,} \\ 0, & \text{otherwise,} \end{cases} \tag{1}$$

and the issue choice indicator function

$$d_i = \begin{cases} 1 & \text{if the issuer uses a negotiated sale for bond issue } i, \\ 0, & \text{otherwise.} \end{cases} \tag{2}$$

Define heterogeneous potential yields for each i as

$$y_i = \begin{cases} y_i^R & \text{if } \mathbf{R}_i = 1 \ (d_i = 0), \\ y_i^C, & \text{if } \mathbf{R}_i = 0 \text{ and } d_i = 0, \\ y_i^N, & \text{if } \mathbf{R}_i = 0 \text{ and } d_i = 1. \end{cases} \tag{3}$$

The heterogeneous differences in potential yields are

$$\begin{aligned} \Delta_i &\equiv y_i^R - y_i^C, \\ \delta_i &\equiv y_i^N - y_i^C. \end{aligned} \tag{4}$$

Of particular interest are the heterogeneous differences in potential yields between restricted bonds and free bonds for each i, λ_i^*, with

$$\lambda_i^* = \Delta_i - d_i \delta_i. \tag{5}$$

The first term on the right hand side of equation (5) reflects how the restriction affects competitive sales. We refer to it as the *industrial organization* effect, or simply IO effect.[10] The second term in equation (5) depends on the issuer's optimal selection between negotiated and competitive sales. We refer to it as the *selection* effect.

Suppose that the issuer chooses the sales method on the basis of δ_i and the non-yield benefit B_i for a negotiated offer, expressed as a yield difference, where we have normalized the non-yield benefits from using a competitive offering to zero. The benefit B_i captures any non-yield benefits received by the issuer including gross spread reductions, or potential kickbacks received by the issuer. The issuer solves

$$d_i = \underset{d \in \{0,1\}}{\operatorname{argmin}} \; y_i^C + d_i \left(\delta_i - B_i \right), \tag{6}$$

with the optimal sales decision

$$d_i \left(\delta_i, B_i \right) = \begin{cases} 1, & \text{if } \delta_i < B_i, \\ 0, & \text{else,} \end{cases} \tag{7}$$

where we use $d_i(\delta_i, B_i)$ to emphasize the decision depends on the difference in potential yields and the non-yield benefits.

We use equation (5) to calculate the expected value of λ_i^* conditional on $\mathbf{X}_i$, where $\mathbf{X}_i = \{\alpha_b, \alpha_s, \alpha_t, X_i\}$ denotes bond type fixed effects α_b, state fixed effects α_s, time fixed effects α_t, and the set of control variables X_i defined in Section 2. Assuming that $\mathbf{R}_i$ is

[10] Cestau (2020) shows that underwriters specialize in one sales method. When negotiated sales are restricted, the natural way for an underwriter to enter is to specialize. Entry in competitive sales increases auction competition and ceteris paribus decreases offering yields. Wu (2020) shows that the number of bids in a competitive sale is negatively correlated to issuer costs.

conditionally independent of the potential yields conditional on $\mathbf{X}_i = x_i$,

$$\begin{aligned} E\left[\lambda_i^* \mid x_i\right] &= E\left[\Delta_i \mid x_i\right] - E\left[d_i \delta_i \mid x_i\right] \\ &= E\left[\Delta_i \mid x_i\right] - E\left[\delta_i \mid d_i = 1, x_i\right] \Pr\left[d_i = 1 \mid x_i\right]. \end{aligned} \tag{8}$$

The first term is the average IO effect, and the second term is the average selection effect.

Several special cases help interpret δ_i and λ_i^* and our empirical estimates. As a first case, suppose that issuers strictly choose the sales method with the lowest potential yields so $d_i(\delta_i, B_i) = d_i(\delta_i)$. Then, the issuer chooses $d_i = 1$ when a negotiated sale offers the lowest yield, that is, when $\delta_i < 0$. Therefore

$$d_i(\delta_i) \times \delta_i < 0, \text{ and } E\left[d_i(\delta_i)\delta_i \mid x_i\right] < 0. \tag{9}$$

Substituting into equation (8) and using inequality (9),

$$E\left[\lambda_i^* \mid x_i\right] > E\left[\Delta_i \mid x_i\right]. \tag{10}$$

Hypothesis 1 *If the average IO effect is zero, $E\left[\Delta_i \mid x_i\right] = 0$, and free issuers choose the sales method to minimize yields, then*

$$E\left[\lambda_i^* \mid x_i\right] > 0. \tag{11}$$

When the restriction does not affect the average competitive yield and issuers choose the sales method to minimize yields, the restriction increases yields—it is costly for issuers because they cannot choose the lowest-yield method. If $E\left[\lambda_i^* \mid x_i\right] < 0$, either free issuers choose more expensive negotiated yields because they factor in non-yield benefits B_i, or the average IO effect is negative enough to offset the inability to choose the lowest-yield method, or a combination of both.

As as second special case and opposite to the first one, suppose that issuers strictly choose the sales method with the highest non-yield benefits so $d_i(\delta_i, B_i) = d_i(B_i)$, or that $\delta_i = \delta$ is a constant. In either case d_i is conditionally independent of δ_i. Therefore

$$E\left[\delta_i | d_i, x_i\right] = E\left[\delta_i | x_i\right]. \tag{12}$$

Substituting into equation (8),

$$E\left[\lambda_i^* | x_i\right] = E\left[\Delta_i | x_i\right] - E\left[\delta_i | x_i\right] \times \Pr\left[d_i = 1 | x_i\right]. \tag{13}$$

Hypothesis 2 *If the average IO effect is zero, $E\left[\Delta_i | x_i\right] = 0$, and issuers choose the sales method to strictly maximize non-yield benefits, then*

$$E\left[\lambda_i^* | x_i\right] = -E\left[\delta_i | x_i\right] \times Pr\left[d_i = 1 | x_i\right]. \tag{14}$$

The effect of the restriction is equal to the expected difference in yields, unconditional on choice, times the probability of choosing a negotiated sale.

We do not expect that the special cases described above hold exactly in our sample. We expect free issuers to factor in both B_i and δ_i, in which case $E\left[\delta_i | d_i = 1\right] < E\left[\delta_i\right]$ due to selection. Thus, we can define a lower bound for the IO effect by

$$\Delta\text{-bound} = E[\lambda_i^* | x_i] + E\left[\delta_i | x_i\right] \times \Pr\left[d_i = 1 | x_i\right]. \tag{15}$$

We argue that we can credibly estimate weighted averages of λ_i^* by OLS in Section 4. We cannot estimate average values of δ_i by OLS if issuers factor in yield differences δ_i to select the sale type or if yield levels are correlated with B_i. Therefore, in Section 5 we employ an instrumental variables approach.

4. The Cost and Benefit of Sales Restrictions

The realized yield is

$$y_i = \left(y_i^C + d_i\delta_i\right) + \mathbf{R}_i\lambda_i^*. \tag{16}$$

Using state, time, and bond type fixed effects, the set of control variables X_i of Section 2, and recognizing that the issue type restriction is predetermined at the time of the bond issues, we argue below that the restriction indicator variable $\mathbf{R}_i$ is as good as randomly assigned conditional on our control variables in our sample, so that the regression

$$y_i = \alpha + \alpha_b + \alpha_s + \alpha_t + \beta X_i + \mathbf{R}_i\lambda^* + \epsilon_i, \tag{17}$$

with $\alpha_b, \alpha_s, \alpha_t$ bond type, state and time fixed effects, provides an estimate of

$$\lambda^* = \sum_{x_i} \omega(x_i) E\left[\lambda_i^* \,|x_i\right], \tag{18}$$

where the weights $\omega(x_i)$ are defined in Angrist & Pischke (2008), and x_i defined above.

4.1. Identifying and interpreting λ^*

Most of the identification of λ^* in regression equation (17) in our sample comes from cross-sectional variation within state rather than time-series variation in sales laws. A total of 18 states have within-state variation in sales laws across bond types. Given the control variables X_i, in these states the difference in expected yields between restricted and free bond types is $\alpha_b + \lambda^*$, and it is α_b in states with homogeneous sales laws across bond types. The difference of the differences is λ^*. Two states, Alabama and Montana, provide most of the time-series variation in the general sales laws. Given controls X_i, the difference between the expected yields of new-money bonds before and after the law change is $\alpha_t + \lambda^*$ since the state fixed effects cancel out. The difference between the expected yields of new-money bonds before and after the law change in all other states is α_t. The difference of the differences is λ^*.

The restriction needs to be as good as randomly assigned conditional on the control variables to interpret regression estimates of λ^* in equation (17) causally. The conditional independence is a reasonable assumption for several reasons:

First, sales restrictions are predetermined at the time of the bond issue and the sales laws do not influence the bond type being issued. By law, the bond type depends on the purpose for the issue. Issuers must use refunding bonds to refinance outstanding bonds; they must issue new-money bonds to finance new capital expenditures. If issuers can use an alternative new-money bond type to finance a certain project, they will use it because the project must have available revenues to pay the alternative bonds and, as a consequence, the issuer does not need to seek approval of additional taxes at a new-money bond election.

Second, we use a comprehensive set of control variables. Since the sales laws vary by state and by bond type, unobserved state characteristics and bond type characteristics may correlate with the sales restriction. Sales laws are mostly fixed in our bond sample so it is sufficient to control for unobserved fixed characteristics. We use state level fixed effects to control for unobserved differences across states. We use bond type fixed effects to control for unobserved differences across bond types. We also control for the relevant statutory characteristics of the bond types. Our statutory controls include the statutory security, state enhancement programs, and the procedures required for public sales. We also control for an extensive list of non-statutory characteristics.

Third, the sales laws applying to ISDs are not customized for school issuers. About half of the sales laws applying to school bonds apply to all municipal governments issuing bonds in the state. Even when the sales laws for school bonds are provided separately, they are often identical to the sales laws that apply to all municipal governments.[11] The changes in sales laws that took place between 2009 and 2011 occurred because of the Federal American Recovery and Reinvestment Act of 2009, which was a nation-wide common shock and not specific to school issuers.

4.2. Estimates of the impact of sales restrictions on offering yields

In Table 4, we report our estimates of λ^* for different specifications of (17). In the first specification, we regress the observed yields on the restriction variable $\mathbf{R}_i$ and the full set of control variables. In the second specification, we add state fixed effects.[12]

In the third specification, we add county fixed effects. Table IA.1 in the Internet Appendix provides the full specification with all coefficient estimates.

λ^* is consistently estimated at -13 bps once we include state or county fixed effects, and at -21 bps without fixed effects. Surprisingly, the sales restriction benefits the average issuer by reducing yields. Hypothesis 1 is rejected in the data, implying that either free issuers choose more expensive negotiated yields because they factor in non-yield benefits B_i, or that the average IO effect is negative enough to offset the inability to choose the lowest-yield method due to the restriction, or a combination of both.

The effect of the restriction is statistically significant at the 1% level in all specifications and also economically significant. In Section 6, we estimate that it increases bond proceeds by more than 1%; several times higher than the difference between restricted and free gross spreads. The effect of the restriction is equivalent to the yield difference

[11] Although we have not surveyed all the sales laws that apply to non-school governments, we have only observed different sales laws for school bonds and other municipal bonds in California only until 2009.

[12] State fixed effects control for systematic differences across states, but they eliminate almost all variation coming from Arkansas, Nevada, South Carolina, Oklahoma, and Indiana since they are colinear with the restriction variable in these states. The statutory security dummies also reduce the variation coming from Louisiana.

Table 4: Impact of sales restrictions on offering yields. The table reports λ^* estimates in equation (17). Controls include indicators for refunding, alternative new-money, fixed effects for statutory security, public sales procedures, state enhancement program, interaction state enhancement with non-rated status, indicators for taxable and multiple-deal in one issue, zero-coupon, bank-qualified, callable deal, non-callable bonds in callable deals, term bond, and sinkable term bond, issuer size quintile, bond size ($M), natural logarithm of deal size ($M), difference between deal and total issue size ($M), fixed effects for rating categories, and fixed effects for years-to-maturity, year-month, and state or county. The number of observations is 281,913. Standard errors are adjusted for heteroskedasticity and clustering at the deal level. ***1%, **5%, *10%.

Variable	(1)	(2)	(3)
λ^*	**-0.21*** **	**-0.13*** **	**-0.13*** **
	(0.00)	(0.00)	(0.00)
Controls	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
State FE		Yes	
County FE			Yes
R-sq	0.87	0.88	0.89

between non-rated bonds and AA- rated bonds. It is larger than the average state fixed effect and the effects of all other controls except for taxable status and bond term. The coefficients on the control variables in equation (17) have signs that one would expect.

To gauge the variability in the impact of sales restrictions on yields, Table 5 reports the effect of the restriction on negotiated sales interacted with time for each year between 2004 and 2014. We replicate the specifications of Table 4 where we substitute the restriction indicator with interaction terms between the restriction variable and year.

The effect of the restriction is always negative and exhibits sizeable variation. In particular, the yield reduction from the restriction increases strongly after the financial crisis. In the most restrictive specification with controls, year-month fixed effects, and county fixed effects, λ_t^* drops from about -9 bps before the financial crisis to -25 bps in 2009 and a peak of -33 bps in 2011, before it reverts back to -7 bps by 2014. These findings suggest that the restriction was most valuable in the years following the financial crisis.

Table 5: Time variation in impact of sales restrictions on offering yields. The table reports λ_t^* estimates in equation (17) for year t. Controls include the same set as in Table 4. The number of observations is 281,913. Standard errors are adjusted for heteroskedasticity and clustering at the deal level. ***1%, **5%, *10%.

Variable	(1)	(2)	(3)
λ^*	-0.18***	-0.10***	-0.09***
	(0.02)	(0.02)	(0.02)
$\lambda^* \times$ year=2004	-0.01	0.01	-0.01
	(0.02)	(0.02)	(0.02)
$\lambda^* \times$ year=2005	-0.04*	-0.04	-0.05**
	(0.02)	(0.02)	(0.02)
$\lambda^* \times$ year=2006	0.01	0.02	0.01
	(0.03)	(0.03)	(0.03)
$\lambda^* \times$ year=2007	0.03	0.04	0.04
	(0.03)	(0.03)	(0.03)
$\lambda^* \times$ year=2008	-0.05**	-0.04*	-0.06**
	(0.02)	(0.02)	(0.02)
$\lambda^* \times$ year=2009	-0.11***	-0.11***	-0.16***
	(0.03)	(0.03)	(0.03)
$\lambda^* \times$ year=2010	-0.12***	-0.11***	-0.15***
	(0.03)	(0.03)	(0.03)
$\lambda^* \times$ year=2011	-0.21***	-0.20***	-0.24***
	(0.04)	(0.03)	(0.03)
$\lambda^* \times$ year=2012	-0.04	-0.04	-0.05**
	(0.03)	(0.03)	(0.03)
$\lambda^* \times$ year=2013	-0.03	-0.03	-0.05*
	(0.03)	(0.03)	(0.03)
$\lambda^* \times$ year=2014	0.04*	0.04	0.02
	(0.02)	(0.02)	(0.02)
Controls	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
State FE		Yes	
County FE			Yes
R-sq	0.87	0.88	0.89

4.3. Robustness of the negative λ^* estimates

Table 6 shows the results of eight robustness tests. In each test we eliminate a potential source of bias in the sample and show that λ^* estimates do not change significantly. Panel A shows the results of four robustness tests based on TACM exceptions. In the first and third tests, we drop taxable bonds and zero-coupon bonds respectively, because they are not well balanced between the samples of restricted bonds and free bonds. In the second test, we drop TACM bonds with notional amounts under $2 million. Although maximum bond proceeds are set in the bond election, the issuer can split the authorized proceeds into several smaller issues so some schools could self-select into this TACM exception. In the fourth test, we combine all of the above. We estimate equation (17) with and without state fixed effects. The coefficients on the restriction variable are all negative, statistically significant at the 1% level, and similar in magnitude to those in Table 4. We report all other controls in the Internet Appendix. They all have the expected signs, and their magnitudes are consistent with those in Table 4.

Table 6, Panel B, shows the results for four additional tests. In the first and fourth tests, we drop observations systematically different from most other school bonds. In the first test, we drop school districts that have a shared liability over bond issues with two or more municipalities.[13] In the fourth test, we drop uncommon statutory securities—we keep only the top-notch categories of general obligation bonds. In the second test, we drop bond issues from before July 2003 because yield data are sparse. In the third test, we drop callable bonds because bond maturity is one of the main yield determinants and the expected maturity of callable bonds is not precisely known. We estimate equation (17) with and without state fixed effects. The effect of the restriction is always negative and statistically significant at the 1% level. The λ^* estimates are similar to those in Table 4, except in the sample of non-callable bonds, where λ^* is estimated at -27 bps.

5. The Impact of Negotiated Sales on Yields

Using the sub-sample of free bonds, we employ an instrumental variables (IVs) approach to estimate the expected effect of switching from a competitive sale to a negotiated sale unconditional on d_i, averages of δ_i, the yield difference defined in expression (4). A valid instrumental variable should monotonically affect the decision to use a negotiated or a

[13]The filter eliminates the few observations from Massachusetts, Maine, and Connecticut.

Table 6: Robustness of our estimates of the impact of sales restrictions on offering yields across subsamples. The table documents the impact of sales restrictions on offering yields by reporting estimates for the coefficients in equation (17). Controls include the same set as in Table 4. The sample contains no COPs. Panel A checks robustness to TACM filters. Specifications (1)-(2) drop taxable bonds. Specifications (3)-(4) drop deal sizes under $2M. Specifications (5)-(6) drop zero-coupon bonds. Specifications (7)-(8) combine all filters. Panel B checks robustness to other filters. The number of observations is 281,913. Standard errors are adjusted for heteroskedasticity and clustering at the deal level. ***1%, **5%, *10%.

Panel A: Robustness tests based on TACM exceptions

	Tax-exempt		Deal size $\geq$ \$2M		Fixed-rate		Tax-exempt & deal size $\geq$ \$2M & fixed-rate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
λ^*	-0.20***	-0.11***	-0.21***	-0.14***	-0.18***	-0.11***	-0.16***	-0.10***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE		Yes		Yes		Yes		Yes
N	269,787	269,787	246,666	246,666	265,459	265,459	223,779	223,779
R-sq	0.881	0.885	0.877	0.88	0.884	0.887	0.894	0.897

Panel B: Robustness tests based on other sample splits

	No shared liability school districts		Year $\geq$ 2004		Non-callable		Top-notch GO	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
λ^*	-0.21***	-0.13***	-0.22***	-0.13***	-0.27***	-0.22***	-0.18***	-0.13***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)	(0.00)	(0.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE		Yes		Yes		Yes		Yes
N	279,859	279,859	270,012	270,012	50,474	50,474	255,789	255,789
R-sq	0.875	0.878	0.873	0.877	0.878	0.882	0.876	0.879

competitive sale but should not affect the potential yields. Formally,

Assumption 1.

$$y_i^R \Big| \mathbf{X}_i \sim y_i^R \Big| \mathbf{X}_i, z_t \rightarrow \left(y_i^C, \delta_i\right) \Big| \mathbf{X}_i \sim \left(y_i^C, \delta_i\right) \Big| \mathbf{X}_i, z_t. \tag{19}$$

Section 4 results suggest that issuers may be factoring in non-yield benefits B_i when choosing the sale type, i.e., $d_i = d_i(\delta_i, B_i)$, which makes IV estimation possible. Suppose variables Z_i predict non-yield benefits such that $B_i = B_i(Z_i, \mathbf{X}_i)$. Our empirical approach is to choose potential instrumental variables Z_i that satisfy Assumption 1 and apply two-stage least-squares to estimate local average treatment effects for δ via

$$y_i | \left(\mathbf{R}_i = 0\right) = a + a_b + a_s + a_t + \beta^R X_i + \delta \widehat{d}_i + e_i, \tag{20}$$

with a a constant, a_b, a_s, a_t fixed effects, X_i the controls defined in Section 2, and choice $\widehat{d}_i$ predicted using Z_i and the same controls X_i.

5.1. Potential instruments for sales method d_i

We construct a number of variables that we expect to satisfy Assumption 1. Our list includes issuer preference over sales methods, bond counsel incentives and preferences, and local political conditions. We also explore issuer relationship to underwriter and underwriter specialization, financial advisors' sale type preference, bond statutory security, underwriter fees, exits of major underwriters, and several alternative measures of local political conditions.

Issuer preference: Issuers may prefer a particular sales methods, regardless of potential yield. For example, they might keep using the same method due to learning by doing or inertia. For each deal, we calculate the historical average of the choice indicator d_i for the issuer's bond sales between 1990 and the given deal in the sub-sample of free deals. The variable captures issuers' revealed preference over sales method. Because it is constructed from past data, the instrument should be uncorrelated with current yield determinants. We include fixed effects to control for any correlation with time-invariant determinants of both yields and choices.

Bond counsel incentives: Bond counsels have played an important role in municipal bond issuance since the end of the 19^{th} century (Cestau et al. 2018). They are responsible for issuing a legal opinion on the legality of the bonds issue and on whether the bonds qualify for federal tax exemption. The experience of the bond counsel with each sales method is unlikely to be linked to current yields given the nature of their task, but experience could affect choices because the selection of the bond counsel precedes the choice of the sales method and bond counsels have long-standing relationships with underwriters and issuers. To construct the bond counsel instrument, we calculate the proportion of negotiated sales of the current bond counsel in the previous calendar year. Since we use past data, the instrument should be uncorrelated with current yield determinants.

Local political conditions: Officials and constituents that sympathize with different political parties may have different attitudes towards competitive and negotiated sales, which could affect school choices independent of yield. For school districts, we cannot directly measure political preferences because there are no partisan elections at the school district level. To proxy for local political preferences, we calculate the average proportion of uncontested Democratic seats in the last state lower house election in the lower house districts that overlap the school district.[14] Around 32% of all state lower house elections are uncontested.

The next section reports statistical tests that all three instruments pass our instrument selection criteria. In Appendix B.1 we document the sources of data and details on the construction of the political condition instruments. Appendix B.2 expands on the other candidate IVs that do not pass our exclusion restriction or relevance condition tests.

[14] We also explore uncontested Republican seats but that does not satisfy the selection criteria.

5.2. Instrument selection

The statutory bans on negotiated sales allow us to test the direct effect of potential instrumental variables on restricted yields. Van Kippersluis & Rietveld (2018) call this the zero-first-stage test. The ability to informally test the exclusion restriction is a key benefit associated with observing the sales laws. Our instruments meet both the zero-first-stage test and the relevance condition.

Issuers of restricted bonds cannot use negotiated sales—a variable cannot affect yields through its effect on choices for restricted issues. Any direct correlation with restricted yields rules out a variable as instrument. To check the condition, we regress yields in the sample of restricted bonds on the instrument or set of instruments and include the same controls as in our baseline specification. We estimate the direct effect $\boldsymbol{\xi}$ of instrumental variables Z_i on yields of restricted bonds via

$$y_i | \left(\mathbf{R}_i = 1\right) = a + a_b + a_s + a_t + \beta^R X_i + \boldsymbol{\xi} Z_i + e_i, \tag{21}$$

with a a constant, a_b, a_s, a_t fixed effects, and X_i the controls defined in Section 2. We use t-tests and F-tests to check whether the instruments are correlated with restricted yields. We rule out instruments when the p-value of the null, $H_0 : \boldsymbol{\xi} = 0$, is 10% or less.

We assume that if an instrument is not correlated with yields in the sample of restricted bonds, it is not directly correlated with potential yields in the sample of free bonds—our exclusion restriction: $\boldsymbol{\xi}| \left(\mathbf{R}_i = 1\right) = 0 \implies \boldsymbol{\xi}| \left(\mathbf{R}_i = 0\right) = 0$. For the assumption to hold, we need free yields to be affected by the same bond fundamentals as restricted yields although not necessarily by the same amount. The effects need to be qualitatively similar but not quantitatively. We argue that this condition is met in our data. First, we only have ISDs in both samples, and ISDs have simple and similar revenue structure throughout the country. Second, they mainly issue plain-vanilla bonds

Table 7: Instrument variables used in estimating the effect of negotiated sales on yields. The table reports the results for testing the exclusion restrictions and weak instrument tests for the IV used in estimating δ.

	(1)	(2)
Issuer preference		
Zero-first-stage F-test	0.31 (p=0.58)	2.59 (p=0.11)
Underidentification LM-test	16,409 (p=0.00)	9,180 (p=0.00)
Weak identification F-test	21,223 (p=0.00)	11,610 (p=0.00)
Bond counsel preference		
Zero-first-stage F-test	1.06 (p=0.30)	2.62 (p=0.11)
Underidentification LM-test	4,412 (p=0.00)	2,493 (p=0.00)
Weak identification F-test	4,613 (p=0.00)	2,545 (p=0.00)
Political preference		
Zero-first-stage F-test	0.02 (p=0.88)	0.19 (p=0.66)
Underidentification LM-test	281 (p=0.00)	125 (p=0.00)
Weak identification F-test	287 (p=0.00)	124 (p=0.00)
Issuer, counsel & political preferences		
Zero-first-stage F-test	0.52 (p=0.67)	1.69 (p=0.17)
Underidentification LM-test	17,840 (p=0.00)	10,266 (p=0.00)
Weak identification F-test	8,350 (p=0.00)	4,611 (p=0.00)
Overidentification J-test	66.45 (p=0.00)	102.71 (p=0.00)
Controls & year-month FE & state FE	Yes	
Controls & year-month FE & county FE		Yes

with almost identical statutory securities.

We perform an underidentification test using the Kleibergen-Paap LM statistic and a weak identification test based on the Kleibergen-Paap F-statistic and Stock–Yogo critical values. For joint instruments, we perform the Sargan–Hansen J-test for overidentification. In all these tests, we regress an indicator for negotiated sales on the instrument or set of instruments and the same controls as in our baseline specification.

Table 7 summarizes the IV selection process. All IV stages use the same controls as in our baseline specification. The left specification uses state fixed effects and the right uses county fixed effects. We test the exclusion restriction condition in the sub-sample of restricted bonds and the first-stage relevance condition in the sub-sample of free bonds. For all three instruments and the combination of all three, the zero-first-stage test is insignificant, showing that the instruments do not affect restricted yields. Both the underidentification test and the weak identification test are rejected for all three instruments, showing that the instruments affect issuance choices. The over-identification test is rejected for the combination of the three instruments. By contrast, all the other instruments listed in the Appendix fail the zero-first-stage test or the underidentification test and are therefore not used.

5.3. Estimation results

Table 8 reports OLS and IV estimates for the causal effect δ unconditional on choice. All IV stages use the same controls as in our baseline specification. We report IV estimates for each individual instrument and for the combination of the three instruments. The left specification uses state fixed effects and the right uses county fixed effects. OLS estimates show a 13 bps and 12 bps effect on negotiated yields respectively. The IV estimates for the combination of the instrumental variables, our preferred specifications, are 15 bps and 17 bps respectively. In present value terms, they imply a reduction in value of around 1.2% and 1.4% for the average bond in the sample. The estimates produced by each individual instrument are relatively volatile, between 13bps and 60bps, indicating that they affect different issuers with different δ_i differently. All δ estimates in Table 8 are positive, statistically significant at the 1% level, and economically meaningful.

Hypothesis 2 provides information on whether issuers choose more negotiated sales when they are relatively cheaper. That is we can see if $E\left[\delta_i \middle| d_i = 1\right] < E\left[\delta_i\right] < E\left[\delta_i \middle| d_i = 0\right]$. The null is that the three are equal if the average IO effect is zero, which means that the λ^* estimate from Table 4 should be equal to the product of the δ estimate in our preferred specification in Table 8 times the proportion of negotiated sales in the

Table 8: IV estimates of the impact of negotiated sales on yields. The table documents estimates for δ. The first row reports OLS estimates in specifications with control variables and state fixed effects and, alternatively, county fixed effects. Controls include the same set as in Table 4. The remaining rows report IV estimates in the same specifications when we instrument the sale type choice with issuer preference, underwriter preference, bond counsel preference, and local political preference. The last specifications pools all instruments. Estimates are reported only when the relevance condition and exclusion restriction are satisfied. The number of observations is 228,209. ***1%, **5%, *10%.

δ	(1)	(2)
OLS	**0.13*** **	**0.12*** **
	(0.00)	(0.00)
IV *Issuer preference*	0.13***	0.13***
	(0.01)	(0.01)
IV *Bond counsel preference*	0.27***	0.41***
	(0.02)	(0.03)
IV *Political preference*	0.53***	0.60***
	(0.10)	(0.16)
IV *Issuer, counsel & political preferences*	**0.15*** **	**0.17*** **
	(0.01)	(0.01)
Controls & year-month FE & state FE	Yes	
Controls & year-month FE & county FE		Yes

sub-sample of free bonds (80%). The equality holds almost exact for the specifications with county fixed effects and is slightly less tight for the specification with state fixed effects. We are inclined to interpret the estimates as support for Hypothesis 2. Either $E\left[\delta_i \middle| d_i\right] = E\left[\delta_i\right]$ or issuers respond optimally to yield differences to some extent and this is offset by the IO effect.

A priori, there exists no empirical evidence in favor or against an IO effect of sales restrictions. Cestau (2019) shows that the underwriting industry is similarly concentrated for the sub-samples of free competitive sales and restricted competitive sales, and that market concentration has not changed between 1990 and 2014 in either case. Therefore, it seems that on average issuers do not choose more negotiated sales when they are relatively cheaper, that the variation in sales methods derives from variation in B_i, and that OLS estimates are lower than IV estimates because yield levels, both competitive and negotiated, are negatively correlated with non-yield benefits B_i. Nevertheless, the OLS estimates are close to the IV estimates, a finding we interpret as OLS estimates having a small selection bias. In the next section, we explore if relation (14) holds conditionally in different time periods.

Robustness of positive δ estimates: We check for robustness of our IV estimates with respect to how the issuer preference variable is constructed since it is the most influential instrument of the three. We explore three construction alternatives: using all school deals in our sample instead of the sub-sample of free deals; taking the last bond sale instead of the average choice indicator d_i since 1990; using the raw data of school deals before the data filters of Appendix A.1. Figure 2 shows that the alternative estimates of δ, combined with bond counsel and political preferences, are aligned to those in Table 8. The median δ estimate is 16 bps, the lowest is 13 bps, and the highest is 19 bps.

5.4. The IO effect of sales restrictions

Parallel to the analysis of time-series in λ_t^* in Section 4, we estimate $E[\delta_t]$ for each year between 2003 and 2014. To obtain the IV estimates, we use the specification with issuer, counsel, and political preference instruments and all controls, year-month fixed effects, and state fixed effects in Table 8 and we interact the choice of sales method with time of issuance. Figure 3 and Table 9 document the results.

Table 9 reports year-by-year estimates for $E[\lambda_t^*]$, $E[d_t]$, $E[\delta_t]$ in Hypothesis 2, and the Δ_t-bound estimates from equation 15. Panel A of Figure 3 compares the IV estimates of $E[\delta_t]$ to the choice probabilities $E[d_t]$. The dotted lines represent the unconditional time-series averages. As Panel A reveals, the fraction of negotiated sales is stable over time. It is around 80% in all years, with a maximum of 84% in 2005 and a minimum of 74% in 2013 and 2014. The cost of negotiation, $E[\delta_t]$, is always positive and relatively stable, with a low of 11 bps in 2003 and a high of 23 bps in 2011. The right plot in Panel

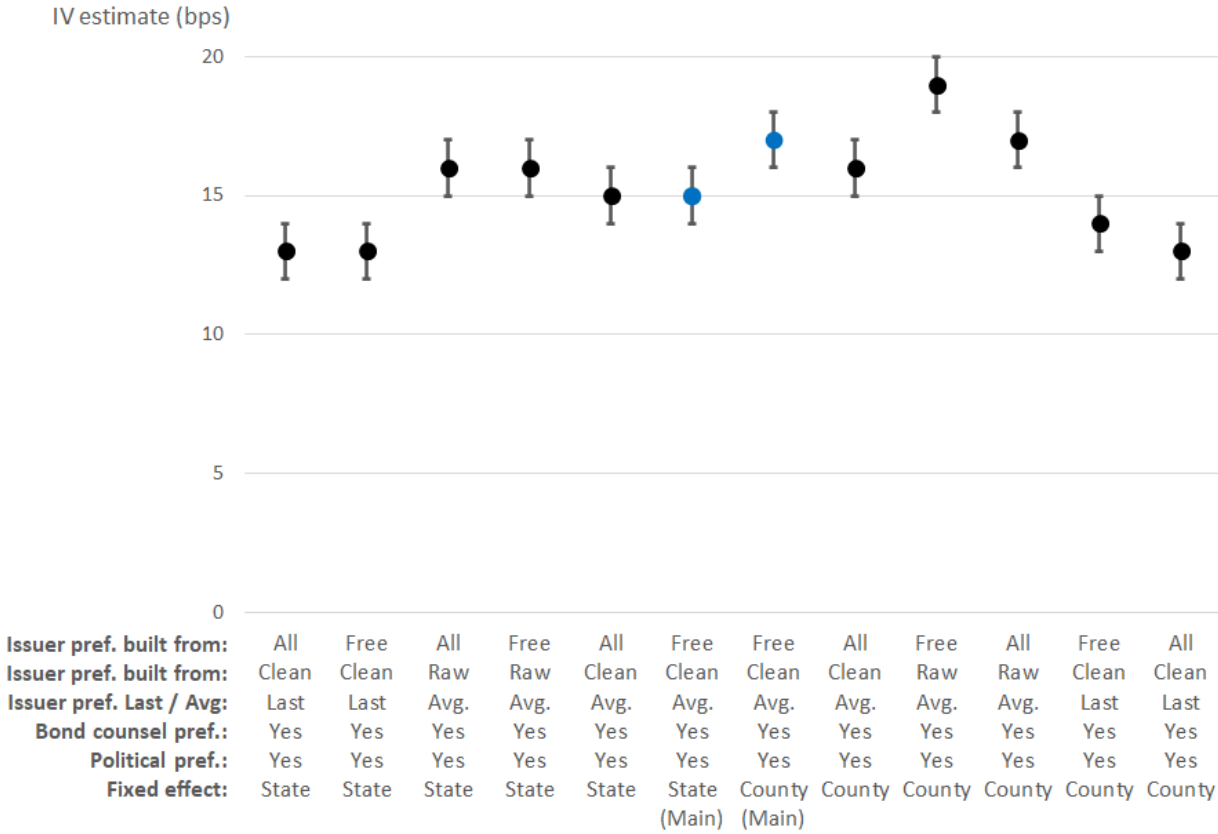


Figure 2: IV estimate for δ. The figure reports IV estimates for δ when we vary the construction of the issuer preference instrument and the fixed effects between state and county.

A plots $E[\delta_t]$ against $E[d_t]$ and reveals that the covariance between the two over time is close to zero. It is clear that large movements in δ have small effects on the choice of the sales method. This result suggests that B_i is a first-order determinant of d_i, and δ_i has a marginal effect, if any. Thus, we argue that $E\left[\delta_i \middle| d_i = 1\right] \approx E\left[\delta_i\right] =$ IV estimate, and Δ_t-bound is approximately the IO effect.

Panel B of Figure 3 compares $E[\lambda_t^*]$ from Table 5 to $-E[\delta_t] \times E[d_t]$. The former term varies over time, and the later term moves little over time. The scatter plot to the right in Panel B reveals that, while the two comove over time, they are not always equal to each other. Therefore, although we do not reject it on average, we reject Hypothesis 2 for some years.

While the Δ_t-bound is around zero on average, it exhibits sizeable variation over time.

Table 9: Time variation in λ_t^*, δ_t, and Δ_t-bound. The table documents the time-series variation in λ_t^*, δ_t and Δ_t-bound. To obtain the IV estimates for δ_t, we use the specification with issuer, counsel, and political preference instruments and all controls, year-month fixed effects, and state fixed effects. The Δ_t-bound is defined as $E[\lambda_t^*] + E[\delta_t] \times E[d_t]$, which is the bound on the IO effect defined in Hypothesis 2.

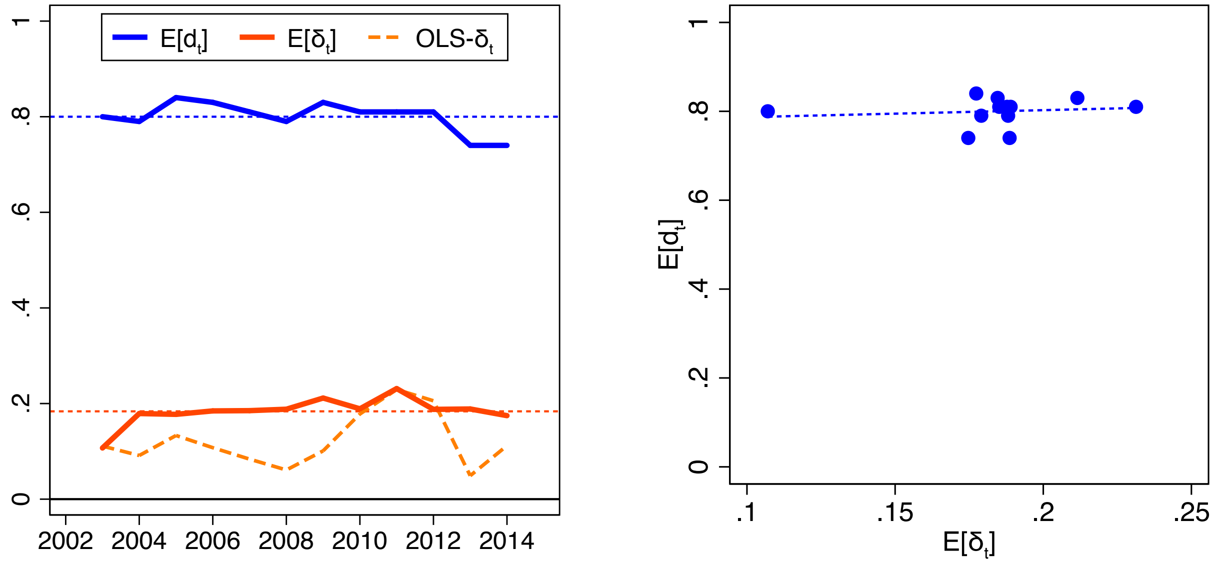
Year	$E[\lambda_t^*]$	$E[d_t]$	$E[\delta_t]$	Δ_t-bound
All	-0.15	0.80	0.18	0.00
2003	-0.10	0.80	0.11	-0.01
2004	-0.11	0.79	0.18	0.03
2005	-0.17	0.84	0.18	-0.02
2006	-0.09	0.83	0.18	0.07
2007	-0.10	0.81	0.19	0.05
2008	-0.14	0.79	0.19	0.01
2009	-0.19	0.83	0.21	-0.02
2010	-0.22	0.81	0.19	-0.07
2011	-0.32	0.81	0.23	-0.13
2012	-0.17	0.81	0.19	-0.02
2013	-0.09	0.74	0.19	0.05
2014	-0.09	0.74	0.17	0.04

Table 9 shows that it drops below zero in the financial crisis and peaks at -13 bps in 2011 when the cost of the restriction $E[\lambda_t^*]$ is the lowest. This is also the time when OLS and IV estimates for $E[\delta_t]$ coincide, as depicted in the top left figure. The restriction on the sales method thus seems to act as a hedge for restricted issuers against higher offering yields in negotiated sales, especially in crisis times when underwriters may have higher bargaining power than in normal times and issuers do not make sales choices based on yield.

6. Counterfactual Analysis

Table 10 reports the results from two counterfactual experiments. In the first counterfactual experiment, the λ^*–counterfactual, we measure the effect on free bond proceeds if a negotiated sales restriction was imposed on all free bonds. In the second counterfactual experiment, the δ–counterfactual, we measure the effect on negotiated bond proceeds if all negotiated bonds were sold competitively. We calculate counterfactual bond prices

Panel A: Time-series of negotiated sales, $E[d_t]$, and cost of negotiation, $E[\delta_t]$.



Panel B: Time-series of cost of restriction, $E[\lambda_t^*]$, $-E[\delta_t] \times E[d_t]$, and Δ_t-bound.

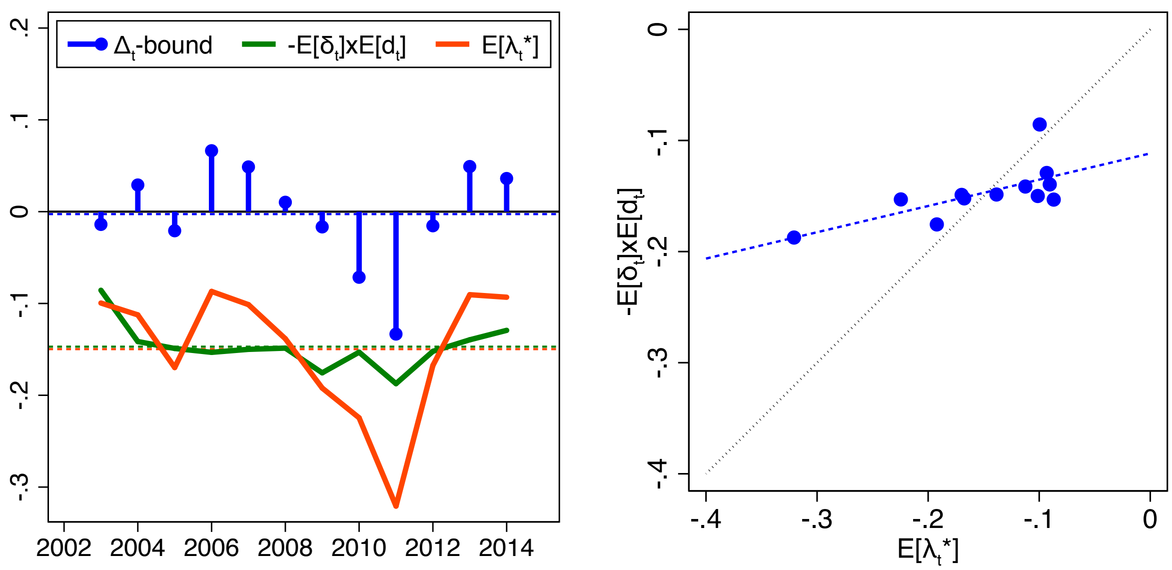


Figure 3: Time variation in λ_t^*, δ_t, and Δ_t-bound. The figure reports in Panel A the choice probability $E[d_t]$ and IV and OLS estimates for $E[\delta_t]$ for each year t. To obtain the IV estimates, we use the specification with issuer, counsel, and political preference instruments and all controls, year-month fixed effects, and state fixed effects. Panel B reports $E[\lambda_t^*]$, $-E[\delta_t] \times E[d_t]$, and the Δ_t-bound $E[\lambda_t^*] + E[\delta_t] \times E[d_t]$, which is the bound on the IO effect defined in Hypothesis 2. In the left plots, the dotted lines represent the sample averages. In the right plots, the dotted lines represent the regression lines.

Table 10: Counterfactual analysis. The table documents the impact on bond proceeds from two counterfactual experiments. In the first experiment, we measure the impact if a sales restriction is imposed on all free bonds. In the second experiment, we explore if all negotiated bonds are sold competitively. We use state fixed effects on the left and county fixed effects on the right. The proportion of negotiated sales in the 2003-2014 deal sample is 83%.

	Impact on bond proceeds					
	State FE			County FE		
	Yield	+ Gross spread	= Total	Yield	+ Gross spread	= Total
Counterfactual #1: Free deals are restricted (λ^*–counterfactual)						
Increase in bond proceeds for free deals (%)	1.05	-0.20	0.85	1.05	-0.20	0.85
Dollar amount in 2003-2014 deal sample ($B)	4.45	-0.86	3.59	4.51	-0.86	3.59
Counterfactual #2: Free negotiated deals are issued competitively (δ–counterfactual)						
Increase in negotiated bond proceeds (%)	1.24	0.00	1.24	1.41	0.00	1.41
Dollar amount in 2003-2014 deal sample ($B)	4.34	0.02	4.36	4.93	0.02	4.94

by adjusting the observed yield by either $E[\lambda_i^*]$ or $E[\delta_i]$[15] and keeping all other variables, such as coupon rate and maturity, the same. We multiply the counterfactual bond prices by the bond notional amounts and aggregate across all free bonds and all negotiated bonds, respectively. We compare against observed bond proceeds to calculate the percentage effect. We compare the price effect to the gross spread effect and form a total effect. To calculate the effect on gross spreads we use nearest-neighbor matching by notional amount.

Bond proceeds would increase by 1.05% if free bonds were restricted. Gross spreads would increase by 0.20%, yielding 0.85% total increase in issuer proceeds.[16] Negotiated bond proceeds would increase by between 1.24% and 1.41% if they were sold competitively, while gross spreads would not change significantly.

[15]We implicitly assume that $E\left[\delta_i \mid d_i = 1\right] \approx E\left[\delta_i\right] =$ IV estimate. Our results show that B_i is the first order determinant of the sales method, making it a good approximation.

[16]We have substantial missing data on gross spreads which renders the counterfactual analysis of gross spreads less reliable than of yields.

7. Conclusion

The choice of sales method is an important determinant for the costs of issuing municipal bonds. Several jurisdictions restrict municipal bond issuers from using negotiated sales for several types of bonds. We hand collect data on the legal provisions and restrictions on the bond sales methods used by independent school districts from 40 states between 1990 and 2014 with the help of over 150 bond lawyers.

Restrictions on the use of negotiated sales reduce offering yields by 13 basis points for a typical school bond issuer, with the cost savings being the largest after the financial crisis. We use an instrumental variables approach to estimate the causal impact of negotiated sale on yields. We instrument the non-yield benefits that issuers expect to receive from using negotiation with variables that capture bond counsel incentives, local political conditions, and historically revealed issuer preferences. We estimate that negotiated yields are 15–17 basis points higher than competitive yields. Despite these costs, unrestricted issuers have a revealed preference for negotiated sales over auctions with over 80% of issuers choosing negotiated offerings when they can.

Our estimates indicate that the effect of the sales restriction is two-fold. It disciplines issuers by forcing them to make the right choice. The restriction to competitive sales also has an industrial organization effect that lowers yields during crisis times.

We find legal restrictions on negotiated sales reduce average school bond financing costs—government should restrict negotiations to reduce average yields. But issuers also decide on the sales method based on non-yield information. Further research is needed to determine which states and types of issuers benefit more from state-level sales restrictions into other benefits. That requires exploring the local political economy to obtain direct measures for the non-yield benefits and the industrial organization to identify the biggest winners and losers from banning negotiated sales.

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A. Data and Sales Laws

A.1. Data filters

Thomson Reuter's SDC Platinum database comprises 423,424 municipal bond deals with 1,864,081 individual bonds, or CUSIPs issued between 1966 and 2014. Data on bond deals is scarce before 1990, and data on individual CUSIPs is sparse before July 2003. For ISDs, SDC Platinum comprises 90,401 ISD bond deals with 489,645 individual school bonds issued between 1990 and 2014. Following the advice of industry experts, from this sample we drop 20,235 temporary borrowings and 6,858 certificates of participation (COPs). Temporary borrowings allow to anticipate bond proceeds to be collected within a two-year period or expected tax proceeds to be collected in the ongoing fiscal year, and they are collateralized by the advanced revenues. COPs are issued by third-party borrowers in connection to long-term capital leases and are generally not considered school debt because schools can terminate the lease each new fiscal year. Both types of securities are regulated in statutes different from those of common bonds. We also drop 464, including the only 61 observations from Tennessee, because we cannot identify their type.

For the sample of 62,844 school deals, we have yield data for 335,215 ISD bonds. We drop variable-rate bonds, convertible bonds, and bonds without coupon type data, which brings down our sample to 334,065. We drop bonds with price data but without the necessary information to calculate offering yields. We apply a series of filters to eliminate duplicate data in the SDC database (duplicate data are potentially created when a bond observation is re-entered in the database to correct one or more values, and the previous entry is not eliminated). We use several filters based on the yield curves of the deals and on ranking-year averages to eliminate bonds with clearly wrong and outlier yields. We also eliminate bonds with inconsistent credit rating data. This bring down our sample to 327,314 bonds.

We take a conservative approach and drop bonds with price data and no yield data to avoid measurement error. We need eight variables to calculate offering yields: price, dated date, coupon rate, maturity date, coupon type, callable status, call date or dates, and the redemption percentages. After analyzing the official statements of a large sample of bonds, we found that each of these SDC variables has a small but non-trivial amount of errors. These errors accumulate when calculating yields. The filter is based on how the data is reported and is therefore not related to the characteristics of the issuer or the bonds, and does not bias the sample. Our main results (Table 4) change by +/- 1 bps without this filter. This filter brings down the sample to 284,353 bonds. We have no issuer CUSIPs to calculate issuer size for 2,076 bonds, and 364 bonds do not have complete data for all other controls. Overall, we have complete data for 281,913 bonds.

A.2. Alternative new-money bonds

Election waivers for essential purposes: These bonds are identical to general new-money bonds. They only differ in the approval process. Essential purposes commonly include environmental cleanup, accessibility, life safety, transportation, and school equipment.

Tax and revenues anticipation bonds (TRABs): States laws sometimes create special and dedicated revenues to fund certain school capital expenditures. The special revenues may include special ad-valorem taxes collected by the ISD, sales taxes collected by the county and apportioned to the school district, or state aid and federal aid apportioned to the school district. In every case, the ISD is authorized to issue TRABs to anticipate such revenues without a bond election, since the new debt does not create new taxes. In some states, the TRABs are payable from the revenues they anticipate. In other states, they have the same security as general new-money bonds, but they produce an offsetting reduction in the special levy. In some states, ISDs can also issue energy conservation bonds, which anticipate and are payable from the savings produced by the energy conservation improvements. In states where schools collect a fixed-rate general property tax, ISDs can often advance the general levy to pay for minor capital expenses, such as school equipment and buses.

A.3. Statutory security by state and bond type

Table A.1 documents the statutory security of general and alternative new-money bond deals in the school deal sample by state. The issuer may be required to provide a general revenue pledge or a specific revenue pledge, or both to secure the bond payments. A general pledge includes all "available" revenues unless they are legally committed for other purposes. Specific pledges include one specific source of revenue. However, for technical and historical reasons, they are easier to enforce in case of a lawsuit. Bonds backed by a general pledge are known as general obligations (GO), but both types of pledges admit different qualities.

We distinguish six types of statutory securities according to the type of pledge required by law: all available resources, full faith and credit, limited and specific pledge of revenues excluding ad-valorem property taxes, limited and specific pledge of ad-valorem property taxes, limited and specific pledge of ad-valorem property taxes plus the FFC, and unlimited and specific pledge of ad-valorem property taxes plus the FFC. 91% of general new-money bonds fall into the last category. Alternative new-money bonds have varied securities because they tend to be backed by the specific revenues they anticipate. In some states, such as Pennsylvania and Illinois, general new-money bonds show multiple statutory securities due to changes in the state constitution.

It is clear that the legislator does no factor in the statutory security of the bond types to design the sales provisions. Almost all general new-money bonds in the sample have the same statutory security, however, they have varied sales restrictions. Very few alternative new-money bonds have the same statutory security as general new-money bonds, however, they tend to have the same sales laws.

A.4. Other statutory characteristics

Public sale procedures: The requirements, which vary by bond type, include number, place, and time of the publication of the notice of bond sale, the basis to select the auction winner, the amount of the bid security deposit, the maximum interest rate, whether it admits open or close bids, sealed or electronic bids, and whether any or all bids may be rejected. Of particular interest are the publication requirements because they commit the issuer to a date certain for pricing the bonds, which runs the risk of entering the market at an inopportune time. We construct a rating that increases by one point for each required publication, and by half a point for each of the remaining items, up to a total of three points.

State enhancement programs: There are seven types of enhancements. In descending order of quality, they are: a general pledge of the full faith and credit of the state, a specific pledge of state revenues, a permanent state fund, a periodically-appropriated state fund, an unlimited intercept of state monies appropriated for the school district, a limited intercept thereof, a fiscal agent. In addition, the enhancement programs may be triggered prior to the payment date, on the payment date, after the payment date. They also have different participation requirements: mandatory, opt-out, or opt-in. First, we construct a ranking of descending quality based on the type and timing of the program, and then, we divide the proportion of participating issuers by the aforementioned ranking. Hence, our measure of the bond type's enhancement program increases with quality and participation.

A.5. Additional details on data collection

Enabling laws: We relied on the SDC variable *issue description* to assign the enabling laws. The variable gives a name to the issue that depends on the bond type and the statutory security of the bonds. Deals with the same state-issue description pair tend to have the same enabling laws. There are 6783 state-issue description pairs in the sample of ISD deals, several times more than the true number of enabling laws. We matched, by hand, each pair to an enabling law. When there was no one-to-one correspondence, we assigned the enabling laws deal by deal. The official statements do not always indicate the exact sections of the statutes with the enabling laws. Many times they just make a broad reference to a chapter or division of the code. In such cases, we had to comb the statutes for the corresponding enabling laws, or consult a bond lawyer.

Sales laws: There are *general* sales provisions that may apply to multiple bond types or governments, and *specific* sales provisions that offer alternatives to or that supersede the general provisions for a subset of governments or bond types without repealing or amending the general laws. For example, the general sales provisions established by section 43627 of the Government Code of the California Law apply to general new-money bonds of all municipal governments including school districts. These sales provisions were later superseded by section 15140 of the Education Code only for school districts, and years later, section 53508.9 of the Government Code offered *optional* provisions to all municipal governments including school districts. None of these laws were repealed. School issuers in California must abide by the three sets of laws regarding the sale of bonds.

When the sales provisions were not stipulated in the enabling laws or in another enabling law by cross-reference, we had to comb the statutes for the sales provisions, with the added difficulty that, as mentioned above, different sales laws apply to different bond types and different governments, and have different hierarchies, so the search has to be exhaustive. Sometimes the laws do not have a simple interpretation, or the same text can have opposite meanings in different states. In every case, we contacted bond lawyers to interpret the legal texts correctly.

Sales laws amendments: Hundreds and even thousands of legislative acts are passed during each legislative session. The online repositories of bills at each state legislature allow the user to search for legislative acts by number and year of the act, or by keywords. The number and year of the legislative acts that amended or added a section of the law are often indicated, fully or partially, at the bottom of each section of the codified laws of the state. In states where this was not the case, we examined all legislative acts passed in each legislative session between 1990 and 2014 containing the section numbers of the sales provisions using the keywords search tool. We generally obtained the section numbers of amended laws from the official statements. However, this was hardly the case for repealed laws. To search for their section numbers and texts we had to examine the history of all legislative acts in each state and legislative session since 1990 containing key phrases related to the sale of bonds. Naturally, the search was cumbersome and time-intensive since hundreds of results were returned each time.

B. Instrumental Variables

B.1. Construction of local political conditions

Uncontested seats in State lower-house elections: We obtained data on the results of the state lower house elections by house district from Klarner (2018). This database provides data on the "type" of district, the designation given on ballot, election seats, the election type, election date, candidates' names, the "true" party of the candidates, candidates' votes, and the election outcome, among many other variables. Regarding the type of district, there are single-member districts and multimember districts. Multimember districts may be divided into single-member *posts* with separate elections and district designations, or have multiple seats up for election in one single ballot. The election seats variable indicates the number of seats up for election. Regarding the election type, we have special elections—when a legislator resigns or is removed from office— and general elections. We also have primary elections and run-offs. The true party variable is original from this database. It indicates the true affiliation of the candidate regardless of the party designation on ballot. They typically differ when candidates lose in the primaries and run with different parties in the general election, or when voters can assign any party to any candidate—principally in New England.

We calculate the average proportion of uncontested Democratic seats in the last state lower house election in the lower house districts that overlap the school district. To build this instrument we used the results from special and general elections only. First, we calculated the number of defeated candidates per election by subtracting the number of seats up for election from the number of candidates. For example, if three candidates compete for two seats, only one candidate is defeated. Next, we calculated the number of uncontested Democratic seats by subtracting the number of defeated candidates from the number of Democratic candidates. For example, if two Democratic candidates and one Republican candidate compete for two seats, only one candidate is defeated. Given that there are two Democratic candidates, at least one Democrat will be elected. Then, we computed the ratio of uncontested Democratic seats to total seats up for election. In the case of multi-post districts, we calculated the average ratio.

We merged the election data to the school bond data, and for each school, we calculated the average proportion of uncontested Democratic seats in the house districts that overlap the school. To merge the election data with the school bond data we used a database from the US Census Bureau that contains the lists of the lower house districts that overlap with each the school district. School district names in the Census database differed from the SDC names, so we had to hand-replicate these lists for the nearly 9,000 school districts in our database. Additionally, the Census names are based on the 2010 census, but there are several districts in the SDC database that disappeared before 2010. They commonly involve elementary school districts that merged with high-school districts in the same area, or mergers of adjacent school districts. It required careful analysis to assign house districts to disappeared school districts. We proceeded analogously to construct Republican uncontested seats. This instrument however, does not meet our instrument selection criteria.

State lower-house elections: We determine the average percentage of Democratic votes in the last state lower house election in the lower house districts that overlap the school district. To construct this instrument we used the election results from special and general elections only. First, we measured the total votes for "true" Democratic candidates per lower house district, not by post, regardless of the number of Democratic candidates and the outcome of the elections. Next, we calculated the ratio of Democratic votes to total votes per lower house district. Finally, for each school district, we calculated the average proportion of Democratic votes in the house districts that overlap the school according to the above lists. This instrument does not meet our instrument selection criteria.

Congress elections: We calculate the average proportion of Democratic votes in the last US Congress election in the congressional districts that overlap the school district. We obtained data on the Congress elections by Congressional district from Data & Lab (2017). We obtained the list of congressional districts that overlap each county from the US Census Bureau. First, we computed the average percentage of Democratic votes in the congressional districts that overlap each county. Notice that congressional districts cover much broader areas than counties, so district preferences may not represent county preferences. Also, congressional districts do not respect county lines, and some counties overlap with multiple congressional districts, which exacerbates the aforementioned problem. Next, we calculated the average proportion of Democratic votes in the counties that overlap each school district. Again, there is no one-to-one correspondence between counties and school districts, so county preferences might not represent school district preferences. Additionally, some school districts overlap with small areas of multiple counties, exacerbating the above problem. This instrument does not meet our instrument selection criteria.

Presidential elections: We measure the average percentage of Democratic votes in the last presidential election in the counties that overlap the school district. We obtained presidential election results data by county from Data & Lab (2018). The degree of overlap between counties and school districts varies considerably across states. Some states tend to have a one-to-one correspondence between counties and school districts, while in other states, school districts overlap with small portions of multiple states. Therefore, the measurement error varies systematically from one state to another. The main drawback of this instrument, however, is that presidential elections take place every four years. As consequence, the instrument does not vary much, and is collinear with county fixed effects when the issuer did not tap the market in more than one presidential term. Consistent with Cestau (2018) who finds that Republican governors are associated with lower default risk of state bonds, this instrument does not meet our instrument selection criteria.

B.2. Other excluded instrumental variables

1. **Underwriter specialization:** Underwriters often specialize in competitive sales or negotiated sales (Cestau 2020). The specialization of a prior underwriter is unlikely to affect future yields but can affect future choices if issuers have formed a relationship with the underwriter and underwriters promote their preferred method of sale. We construct a variable where we take the lead manager of the last bond sale, and measure the proportion of negotiated underwritings of the underwriter in that year. Because we measure specialization of the previous underwriter, our instrument is not susceptible to simultaneity between specialization and current yields. We include fixed effects to control for correlation with time-invariant determinants of both yields and choices. However, this instrument does not meet our instrument selection criteria.

2. **Underwriting fees:** Presumably, underwriting fees should not be a determinant of yields, as yields reflect the investors' valuations of the issue, but they are a direct component of B_i and they certainly affect choices. It is a promising instrument in theory but it is poorly balanced between competitive and negotiated sales in the data. We observe the gross spread for 50% of the negotiated sales in our sample, but only 12% for the competitive sales. Therefore, we omit this instrument.

3. **Financial advisor's preference:** The logic behind this instrument is similar to that of the bond counsel instrument. Bergstresser & Luby (2018) and Garrett (2020) document the importance of the bond advisor for outcomes in municipal markets. However, we ruled out this instrument because financial advisors are badly balanced between negotiated sales and competitive sales. Most competitive sales have a financial advisor, while only half the negotiated sales have one.

4. **Bond statutory security:** The premise is that different statutory securities may have marginal effects on yields but great effects on choices. However, we ruled out this instrument because restricted bonds tend to have the same security within states, so the instrument is poorly identified in the exclusion restriction test when we include state fixed effects.

5. **Exits of major underwriters:** The arguably exogenous exits of Lehman Brothers and Bear Stearns in 2008 might have affected choices going forward but not yields. These instrument candidates are unique in the sense that they are the only ones based on a particular event, so we analyzed them separately. We divided our sample into two periods, pre-crisis and post-crisis, and defined Lehman's default as the treatment variable. The treatment intensity in the state is determined by the Lehman's market share in the state's underwriting industry before the crisis. Although we do not include it in the paper, we experimented with different windows for before and after the crisis and we always found significant effects on restricted yields. We proceeded analogously with Bear Sterns with similar results. We conclude that Lehman and Bear exits are not suitable instruments for they fail the exclusion restriction test.

Table A.1: Statutory security by state and bond type. The table documents the statutory security of general and alternative new-money bonds in the deal sample. We distinguish six types of statutory securities according to the type of pledged revenues: all available resources (GF), full faith and credit (FFC), limited and specific pledge of revenues excluding ad-valorem property taxes (Lt1), limited and specific pledge of ad-valorem property taxes (Lt2), limited and specific pledge of ad-valorem property taxes plus the FFC (Lt3), and unlimited and specific pledge of ad-valorem property taxes plus the FFC (Utd). † indicates that alternative and general new-money deals share the same statutory security, and $*$ indicates that only alternative bonds have the given security.

State	GF	FFC	Lt1	Lt2	Lt3	Utd
AL	–	–	221	–	–	–
AR	–	–	–	1,004	–	–
AZ	–	–	10*	–	–	955
CA	–	–	–	434*	–	2,818
CO	–	–	–	–	–	411
CT	–	–	–	–	–	93
FL	–	–	29*	–	–	49
GA	–	–	–	–	–	490
IA	21*	–	378*	220*	–	581
ID	–	–	–	–	–	227
IL	–	393*	–	–	852	3,022†
IN	–	–	–	–	172	180
KS	–	–	–	–	–	489
LA	47*	–	39*	47*	–	509
MA	–	–	–	–	177	26
ME	–	–	–	–	–	11
MI	–	–	–	–	203†	1,738
MN	–	–	–	–	–	1,429†
MO	–	–	–	–	–	1,451
MS	–	–	91*	–	65*	157
MT	–	–	–	–	–	311
ND	–	–	–	81*	–	104
NE	–	–	–	229*	–	365
NH	–	–	–	–	–	55
NJ	–	–	–	–	–	1042
NM	–	–	–	–	–	725
NV	–	–	–	–	117†	–
NY	–	–	–	–	–	4,689
OH	–	–	–	–	347*	1,234
OK	–	–	–	–	–	3,762
OR	–	–	–	–	–	400
PA	–	–	–	–	857	2,505
SC	–	–	–	–	–	814
SD	–	–	–	228*	–	111
TX	–	–	–	–	451†	3,717
UT	–	–	–	–	–	221
VT	–	–	–	–	–	21
WA	–	48*	–	–	30*	1,048
WI	–	–	–	–	–	948†
WV	–	–	–	–	–	49
WY	–	–	–	–	–	79

Internet Appendix

This internet appendix provides additional analysis and extended tables.

A. Regression results with full list of controls

Tables IA.1, IA.2, and IA.3 provide regression results from the main text with full list of controls.

Table IA.1: Impact of sales restrictions on offering yields. The table reports λ^* estimates in equation (17). Controls include indicators for refunding, alternative new-money, fixed effects for statutory security, public sales procedures, state enhancement program, interaction state enhancement with non-rated status, indicators for taxable and multiple-deal in one issue, zero-coupon, bank-qualified, callable deal, non-callable bonds in callable deals, term bond, and sinkable term bond, issuer size quintile, bond size ($M), natural logarithm of deal size ($M), difference between deal and total issue size ($M), fixed effects for rating categories, and fixed effects for years-to-maturity, year-month, and state or county. ***1%, **5%, *10%.

Variable	(1)	(2)	(3)
λ^*	-0.21***	-0.13***	-0.13***
Refunding bond	-0.09***	-0.06***	-0.06***
Alternative new-money	0.07***	0.04***	0.05***
Statutory security FFC	-0.08	-0.30**	-0.53***
Statutory security Ltd 1	-0.12	-0.11	-0.34**
Statutory security Ltd 2	-0.11	-0.07	-0.34**
Statutory security Ltd 3	-0.09	-0.24**	-0.49***
Statutory security Ultd	-0.16	-0.23**	-0.49***
Public sales procedure	0.03***	0.03***	0.02***
State enhancement	-0.11***	-0.12***	-0.09***
State enhance×Non-rated	-0.43***	-0.66***	-0.84***
Taxable	0.98***	0.97***	0.98***
Multiple deal	0.09***	0.08***	0.10***
Zero coupon	0.78***	0.77***	0.78***
Bank-qualified	-0.14***	-0.13***	-0.13***
Callable deal	-0.00	0.03***	0.04***
Non-callable bond	0.08***	0.05***	0.05***
Term deal	-0.05***	-0.04***	-0.04***
Sinkable	0.01*	0.01	0.01
Bond size	-0.00***	-0.00***	-0.00***
Deal size	-0.01***	-0.01***	0.00
Issue–deal size	-0.00***	-0.00***	-0.00***
AAA	-0.39***	-0.42***	-0.42***
AA+	-0.33***	-0.35***	-0.34***
AA	-0.25***	-0.28***	-0.27***
AA-	-0.13***	-0.17***	-0.15***
A+	-0.13***	-0.15***	-0.16***
A	-0.09***	-0.12***	-0.12***
A-	-0.04***	-0.07***	-0.06***
BBB+	0.37***	0.29***	0.28***
BBB	0.37***	0.30***	0.27***
BBB-	0.08*	-0.04	-0.16***
Constant	4.48***	4.48***	4.60***
Issuer size FE	Yes	Yes	Yes
Years-to-maturity FE	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
State FE		Yes	
County FE			Yes
N	281,913	281,913	281,913
R-sq	0.87	0.88	0.89

Table IA.2: Impact of sales restrictions on offering yields—Variation across subsamples. The table documents the impact of sales restrictions on offering yields by reporting estimates for the coefficients in equation (17). Controls include the same set as in Table 4. The sample contains no COPs. Specifications (1)-(2) drop taxable bonds. Specifications (3)-(4) drop deal sizes under \$2M. Specifications (5)-(6) drop zero-coupon bonds. Specifications (7)-(8) combine all filters. ***1%, **5%, *10%.

	Tax-exempt		Deal size $\geq$ \$2M		Fixed-rate		Tax-exempt & deal size $\geq$ \$2M & fixed-rate	
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
λ^*	-0.20***	-0.11***	-0.21***	-0.14***	-0.18***	-0.11***	-0.16***	-0.10***
Refunding bond	-0.09***	-0.05***	-0.09***	-0.06***	-0.09***	-0.05***	-0.06***	-0.03***
Public sales procedure	0.02***	0.02***	0.02***	0.03***	0.01***	0.01***	-0.003*	0.01***
State enhancement	-0.12***	-0.12***	-0.12***	-0.09***	-0.10***	-0.22***	-0.09***	-0.23***
State enh.xNon-rated	-0.37***	-0.64***	-1.12***	-1.14***	-0.45***	-0.68***	-1.14***	-1.17***
Taxable			0.99***	0.98***	1.03***	1.02***		
Multiple deal	0.06***	0.05***	0.10***	0.08***	0.05***	0.03***	0.01**	0.00
Zero coupon	0.79***	0.78***	0.80***	0.80***				
Bank-qualified	-0.15***	-0.14***	-0.14***	-0.14***	-0.14***	-0.13***	-0.14***	-0.14***
Callable deal	0.00	0.03***	0.02***	0.04***	0.02***	0.06***	0.04***	0.06***
Non-callable bond	0.07***	0.04***	0.07***	0.05***	0.05***	0.03***	0.04***	0.01***
Term deal	-0.07***	-0.06***	-0.03***	-0.02***	0.03***	0.03***	0.04***	0.04***
Sinkable	-0.02**	-0.01*	0.02**	0.02**	-0.01	-0.01	-0.04***	-0.04***
Bond size	0.00***	0.00***	0.00**	0.00***	0.00	0.00	0.00***	0.00***
Deal size	-0.01***	-0.01***	-0.01***	0.00**	-0.01***	0.00***	-0.01***	0.00***
Issue–deal size	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***
Constant	4.46***	4.21***	4.43***	4.10***	4.53***	4.23***	4.51***	4.21***
Issuer size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Security FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Maturity FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE		Yes		Yes		Yes		Yes
N	269,787	269,787	246,666	246,666	265,459	265,459	223,779	223,779
R-sq	0.88	0.89	0.88	0.88	0.88	0.89	0.89	0.90

Table IA.3: Impact of sales restrictions on offering yields—Robustness to other filters. The table documents the impact of sales restrictions on offering yields by reporting estimates for the coefficients in equation (17). Controls include the same set as in Table 4. The sample contains no COPs. ***1%, **5%, *10%.

	No shared liability school districts		Year $\geq$ 2004		Non-callable		Dual security	
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
λ^*	-0.21***	-0.13***	-0.22***	-0.13***	-0.27***	-0.22***	-0.18***	-0.13***
Refunding bond	-0.10***	-0.06***	-0.10***	-0.07***	-0.16***	-0.12***	-0.10***	-0.07***
Public sales procedure	0.02***	0.02***	0.03***	0.03***	0.03***	0.06***	0.02***	0.02***
State enhancement	-0.13***	-0.12***	-0.13***	-0.14***	-0.15***	-0.13***	-0.12***	-0.25***
State enh.xNon-rated	-0.44***	-0.69***	-0.40***	-0.63***	0.28***	-0.29***	0.11**	-0.20***
Taxable	0.98***	0.97***	0.98***	0.97***	0.86***	0.84***	1.00***	0.98***
Multiple deal	0.09***	0.08***	0.09***	0.08***	0.08***	0.08***	0.09***	0.08***
Zero coupon	0.77***	0.77***	0.78***	0.78***	0.65***	0.61***	0.78***	0.78***
Bank-qualified	-0.14***	-0.13***	-0.14***	-0.14***	-0.06***	-0.06***	-0.12***	-0.12***
Callable deal	0.00	0.03***	0.00	0.03***			0.00	0.02***
Non-callable bond	0.08***	0.05***	0.08***	0.06***			0.08***	0.06***
Term deal	-0.05***	-0.04***	-0.05***	-0.04***	-0.12***	-0.09***	-0.06***	-0.06***
Sinkable	0.01*	0.01	0.02**	0.02**	0.17***	0.16***	0.05***	0.04***
Bond size	0.00**	0.00***	0.00***	0.00***	0.00***	0.00***	0.00*	0.00**
Deal size	-0.01***	-0.01***	-0.01***	-0.01***	-0.03***	-0.03***	-0.01***	-0.01***
Issue–deal size	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***
Constant	4.46***	4.17***	2.06***	1.76***	3.24***	2.87***	4.29***	4.50***
Issuer size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Security FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Maturity FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE		Yes		Yes		Yes		Yes
N	279,859	279,859	270,012	270,012	50,474	50,474	255,789	255,789
R-sq	0.875	0.878	0.873	0.877	0.878	0.882	0.876	0.879