Annenberg Institute
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EdWorkingPaper No. 26-1500

Property Tax Salience and Public Good Investment: Evidence from School Bond Elections

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In 2019, Texas passed legislation requiring that the ballot text for all school bond referenda include the phrase "THIS IS A PROPERTY TAX INCREASE," highlighting property taxes as their primary funding mechanism. Evaluating the impact of this policy change on voter behavior in a difference-in-differences framework, we find that the introduction of property tax disclaimers reduced the passage rate of school bonds by approximately 9 percentage points relative to other municipal bonds, which were unaffected by the policy. The effects are driven by November elections, where less-informed voters may be more influenced by ballot language. Two survey experiments reinforce our finding that property tax disclaimers increase the salience of property taxes, erode support for school bonds, and shift attention away from the benefits of bonds. When bonds are projected to substantially increase property tax obligations, Texas-style property tax disclaimers reduce voter errors, but for bonds with small impacts on property taxes, these disclaimers increase voter errors.

VERSION: June 2026

Suggested citation: Heller, Blake H., and Melissa Arnold Lyon. (2026). Property Tax Salience and Public Good Investment: Evidence from School Bond Elections. (EdWorkingPaper: 26-1500). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/e3sf-0h64

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Keywords: public goods, school bonds, ballot language, property taxes, government transparency, natural experiment, difference-in-differences, survey experiment

Last Updated: May 5, 2026

JEL Codes: I22, I28, H41, H71, D72

Acknowledgements: Heller and Lyon are equal coauthors; names are in alphabetical order. We thank Eric Brunner, Vladimir Kogan, Julien Lafortune, Claire Mackevicius, Lori Taylor, Ron Zimmer and seminar participants at the University of Houston, AEFP, APPAM, Syracuse/Cornell FLX Economics of Education Workshop, and the WPSA Education Politics & Policy Workshop for helpful comments and suggestions. We are grateful to Aidan Connaughton, Matthew Dardet, Ethan Miles, and all of the staff at the Harvard Digital Lab for the Social Sciences for survey implementation support. Adam Shepardson provided excellent research assistance. This study was approved by the University of Houston Institutional Review Board (STUDY00005532). All experiments were pre-registered on the AEA RCT Registry and all pre-registrations, materials, survey instruments, data, and code can be accessed at https://www.socialscienceregistry.org/trials/16319. Correspondence can be addressed to bhheller@central.uh.edu (Heller) or mlyon@albany.edu (Lyon). All errors are our own.

Funding Declaration: Financial support from the Consortium on the American Political Economy is gratefully acknowledged. The research reported here was also supported in part by the National Academy of Education via the National Academy of Education/Spencer Postdoctoral Fellowship. Opinions expressed do not represent the views of the Consortium on the American Political Economy, the Spencer Foundation, or the National Academy of Education. No external

partner or funder played any role in the design, execution, data analysis, data interpretation, or writing of the study.

Competing interests: The authors declare none.

Manuscript word count: 10,436
Abstract word count: 156

Abstract

In 2019, Texas passed legislation requiring that the ballot text for all school bond referenda include the phrase "THIS IS A PROPERTY TAX INCREASE," highlighting property taxes as their primary funding mechanism. Evaluating the impact of this policy change on voter behavior in a difference-in-differences framework, we find that the introduction of property tax disclaimers reduced the passage rate of school bonds by approximately 9 percentage points relative to other municipal bonds, which were unaffected by the policy. The effects are driven by November elections, where less-informed voters may be more influenced by ballot language. Two survey experiments reinforce our finding that property tax disclaimers increase the salience of property taxes, erode support for school bonds, and shift attention away from the benefits of bonds. When bonds are projected to substantially increase property tax obligations, Texas-style property tax disclaimers reduce voter errors, but for bonds with small impacts on property taxes, these disclaimers increase voter errors.

I. Introduction

In 2024 alone, municipal governments and school districts issued $500 billion in bonds, nearly equaling state and local governments' total revenue from sales tax in that year (Bagley et al. 2025; U.S. Census Bureau 2025). Bonds represent a substantial share of local public finance in the United States because municipal governments and school districts rely on them to fund large, long-term infrastructure projects that substantially exceed short-term budget constraints. When residents approve—or reject—a local bond, they are deciding whether the additional cost burden (e.g., increased property taxes) is justified by the public and private benefits of the investment.

Bonds and their resulting investments in public goods matter for communities. Biasi and colleagues (2025) find that school bond authorization raises both home prices and test scores. Specifically, they find that each "$1,000 per pupil increase in capital spending over five years leads to a 0.04 standard deviation (SD) increase in district-level test scores" and a 5-10% increase in home values over the following decade. These impacts align with other recent work documenting the positive impacts of public school investments on academic outcomes and home values (Jackson and Mackevicius 2024; Lafortune and Schönholzer 2022).

The public determines whether to support bonds by voting in local ballot referenda, a highly visible form of borrowing (Bifulco et al. 2012). However, voters often possess limited information about ballot measures they vote on, misperceive the costs of public projects associated with the referenda they consider, and are more influenced by perceived costs than actual costs (Barth et al. 2020; Burnett and Kogan 2015; Lang et al. 2025). Because ballot

language constitutes a significant share of available information at the voting booth, the salience of the tax language in local ballot referenda may affect voters' willingness to approve said taxes.

Multiple experimental and quasi-experimental studies demonstrate that tax salience significantly influences individual behavior and preferences (e.g., Finkelstein 2009; Chetty et al. 2009; Cabral and Hoxby 2012). Descriptive evidence from Minnesota and Wisconsin also suggests that property tax disclaimers reduce support for school bonds by 4–7 percentage points (pp) in vote shares and 10 pp in passage rates A related survey experiment on the impacts of hypothetical scenarios similarly shows that property tax disclaimers reduce support by 6-9 pp (Brunner et al. 2021). Increasing tax salience reduces average consumption of taxed goods and can be leveraged as a "degree of freedom" for policymakers seeking to shape public behavior (Goldin 2015).

In this project, we provide the first credibly causal evidence on how increasing property tax salience affects electoral outcomes in an applied setting. We first develop a model of voter choice where voter approval of a ballot referendum is decreasing in both the perceived property tax and other costs of the referendum, and preferences for these categories of costs are separable. We then use quasi-experimental evidence from a 2019 policy change, the passage of Texas House Bill 3 (HB3), that required new language stating "THIS IS A PROPERTY TAX INCREASE" in the ballot text of all school bond measures in the state. We leverage the fact that such language did not appear on other municipal bonds to estimate the impact of property tax disclaimers on bond elections over a 13-year period in a difference-in-differences (DiD) framework. We complement our quasi-experimental analyses with experimental evidence from two survey experiments in samples of U.S. adults (Experiment 1) and Texas adults (Experiment

2) where we simulated the policy change by presenting respondents with hypothetical school bonds that randomly vary as to whether they include a property tax disclaimer before asking respondents whether they would support a given bond.

We consistently find that adding the phrase "THIS IS A PROPERTY TAX INCREASE" to the ballot language substantially decreases voter support for affected bonds. In our DiD analysis, we estimate that the enactment of the new Texas law requiring such property tax disclaimers decreases both the overall passage rate and vote share in favor of school bonds by 9.1 pp and 7.5 pp, respectively. This result is driven by November school bond elections, where the vote share in favor declines by 12.5 pp, reducing their passage rate by 22.8 pp. Using North Carolina as a placebo state, we also use a difference-in-difference-in-differences (DDD) approach to demonstrate that changes in bond passage rates are not an artifact of changing perceptions of schools during and after the COVID-19 pandemic or other education-specific secular trends, including the effects of the 2018 nationwide cap on the State and Local Tax deductions on school districts' ability to raise revenue via bonds (Ambrose and Valentin 2024). A series of additional robustness tests, described in section VI.B demonstrates that the substantial decrease in support for school bonds following the introduction of property tax language is durable over time, not sensitive to any specific empirical specification, and not driven by outlier elections.

Additionally, focusing on different parts of the election cycle in the DiD analysis provides suggestive evidence regarding the conditions where the outcome of a bond election is most likely to be sensitive to property tax salience. Theory suggests that less informed voters are more likely to be influenced by ballot language, as this may be the only information they have to base their

decision upon (Burnett and Kogan 2015; Barth et al. 2020; Lang et al. 2025). Additionally, a quirk of Texas property tax law suggests that older voters are likely to be less sensitive to changes in property tax rates, as homeowners aged 65 or older are insulated from these changes by state law, which requires property tax obligations for seniors to remain constant (Kogan et al. 2018). Consistent with these predictions, the decline in school bond passage rates is entirely driven by November (general election) bond elections, where turnout is higher and the median voter tends to be younger and less informed (Anzia 2011, 2014, 2019).

In our survey experiments, we similarly find that randomly being assigned to consider a hypothetical school bond proposal that includes the ambiguous property tax disclaimer "THIS IS A PROPERTY TAX INCREASE" (identical to the language required by Texas law) decreases the share of individuals that support hypothetical school bonds in their community by roughly 10 pp (point estimates range from 9.6 pp to 12.0 pp), roughly the same reduction in school bond support observed among Texas voters following the passage of HB3. The survey experiments replicate and extend the quasi-experimental analyses, demonstrating that property tax disclaimers substantially reduce support for local school bonds and that increased attention to property tax obligations is an important driver of this effect, diverting attention away from the benefits of the bond and toward its costs.

Experiment 2 (the Texas sample) further extends these findings by adding two more treatment arms to the design, allowing us to interrogate whether and how more detailed information about bond effects on property taxes, in addition to the ambiguous information in an HB3-style disclaimer, influences preferences. In our detailed property tax disclaimer treatments, we

manipulate the implications of a bond election for projected property tax obligations, holding the size of the bond constant. We find that respondents are sensitive to the projected magnitude of a bond election's impact on property taxes and that increasing the salience of property taxes in deliberations does not necessarily degrade support for school bonds. When projected tax increases are small, detailed property tax disclaimers that specify the projected change in property tax obligations per $100,000 in home value do not influence support for school bonds, despite substantially increasing the share of respondents who report considering property taxes in their deliberations (+28.6 pp). However, when the projected tax increases are substantive, the reduction in support induced by a property tax disclaimer (-18.4 pp) is 50% larger than the ambiguous property tax treatment (-12.0 pp), suggesting that voters are sensitive to additional information. Comparing the full set of treatment effects suggests that, relative to no property tax disclaimer, ambiguous disclaimers decrease voter errors when projected property tax increases are large, but increase voter errors when projected property tax increases are small.

After considering a hypothetical school bond proposal (where a property tax disclaimer was included for treated respondents) and indicating whether they would support such a bond if it were proposed in their community, respondents were asked "[…] what was on your mind as you decided whether to support or oppose the school bond proposition?" In both samples, respondents in the ambiguous property tax treatment, who rejected hypothetical school bonds at much higher rates, were substantially more likely to select "property taxes" among their explanations, validating the notion that increased property tax salience is a key mediator of voter preferences. In the nationwide sample, increases in considerations related to "property taxes" (+14.1 pp) were partially offset by a reduction in the share of participants who reported

considering “sales tax” as they evaluated the hypothetical proposition (-5.6 pp). In Texas, similar increases in considerations related to “property taxes” (+16.2pp) were offset by a reduction in the share of participants who reported considering “school building conditions” (-11.8 pp) or “technology in schools” (-11.4 pp), alongside a similar change in the share of respondents who reported considering “sales tax” (-3.7 pp) as they evaluated the hypothetical proposition.

The paper makes three key contributions. First, we extend prior descriptive work on the role of property tax salience as a determinant of public support for school bonds (Brunner et al. 2018). We use a quasi-experimental design that isolates the causal impact of the ballot language reform in a new, applied setting. Additionally, we supplement this evidence with two pre-registered survey experiments that directly test the tax salience mechanism, extending past work showing that property tax disclaimers reduce public willingness to support school bonds and that vague disclaimers are treated similarly to those specifying large increases in property tax rates (Brunner et al. 2021). By varying the size of the property tax impacts in our survey experiment, we reveal that voters are sensitive to the magnitude of changes in property tax obligations, and property tax disclaimers that project only small increases in tax rates do not erode bond support. We further demonstrate that ambiguous disclaimers like those introduced by HB3 reduce voting errors when true property tax increases are large and increase voting errors when true property tax increases are small. Moreover, despite recent doubts on the efficacy of “nudge”-style informational interventions (Hreha 2020; DellaVigna and Linos 2022), we demonstrate that such interventions–even when vague–can reshape public finance at scale.

II. Texas Context

In 2019, the Texas Legislature unanimously passed House Bill 3 (HB3), a comprehensive school finance bill. Buried within the bill was a relatively minor but highly consequential provision: a requirement that all future school bond ballot propositions include the phrase "THIS IS A PROPERTY TAX INCREASE." This reform did not apply to other types of bond propositions.

Prior to HB3 (between 2012 and 2019 in our analytic sample), Texans approved over 75% of school bond propositions, investing an average of 11.1 billion dollars per year in local school districts in real 2024 dollars (2024$). The mandatory property tax language requirement can be viewed as a state-level intervention aimed at curbing this kind of local fiscal expansion. Noting the success of HB3 in reducing school bond passage rates, fiscal conservatives in Texas saw ballot language reforms as a tool to promote fiscal restraint. Analyzing Texas State Senator Paul Bettencourt's 2023 proposal to add an HB3-style tax disclaimer to all municipal bonds, the Texas House Committee on Ways and Means (2023) Bill Analysis notes that "[s]ince the addition of this phrase[...] the number of bond propositions that pass has declined. Voters are able to make informed decisions on the amount of school district bond debt they want to approve and for what purpose."

Figure 1 provides example bond propositions from recent Texas bond elections in Travis County. Subfigure 1A shows a 2025 school bond proposition that includes the required property tax disclaimer. Subfigure 1B shows a municipal bond proposition from the same year, which does not include any new property tax language relative to the pre-HB3 period.

III. Conceptual Framework

We build on Lang and colleagues' (2025) model of voter choice, expanding their model to include considerations related to property tax costs separably from other perceived costs. Lang and coauthors demonstrate that misperceptions of costs can lead to voting errors and bias estimates of willingness to pay (WTP) for public goods. Separating costs introduces an additional degree of freedom, allowing voters to reasonably oppose referenda based on their perception (or misperception) that approving the referenda would increase their property tax obligations (or represent a foregone opportunity to lower property taxes) versus other funding mechanisms. Consider the following model for an individual voter *i*, adapted from Lang and colleagues (2025):

$$Y_i^* = f(costs_i, benefits_i)$$

$$Y_i^* = \pi_0 + \pi_1 PropertyTaxCosts_i + \pi_2 OtherCosts_i + \boldsymbol{X_i}\boldsymbol{\gamma} + \epsilon_i, \quad (1)$$

where Y_i^* is the latent expected utility voter *i* would experience if the bond referendum passes; $PropertyTaxCosts_i$ measures the annual value of the additional property tax costs the voter would pay if the referendum passes; $OtherCosts_i$ measures the annual value of the additional non-property-tax costs the voter would pay if the referendum passes; and $\boldsymbol{X_i}$ is a vector of individual characteristics. Following Lang and coauthors (2025), we focus on the relationship between perceived costs and voter support for bond referenda, assuming that considerations related to the benefits of the bond are held constant.

Ceteris paribus, the law of demand implies $\pi_1 < 0$ and $\pi_2 < 0$, however their relative magnitudes are a priori ambiguous. If voters have a particular distaste for property taxes, then $\pi_1 < \pi_2 < 0$, with the magnitude of π_1 increasing in the strength of voter preferences. If voters care only about the magnitude of additional costs, but are indifferent regarding their source, then

$\pi_1 = \pi_2$, and if voters have a taste for property taxes relative to other funding mechanisms (e.g., increasing sales tax, administrative fees, etc.), then it could be the case that $\pi_1 > \pi_2$. A voter's decision of whether to support the referendum (i.e., cast a vote in favor of the referendum or report an intention to do so) is a function of the sign of Y_i^*, where $SupportBond_i = \mathbf{1}(Y_i^* > 0)$.

The model above implicitly assumes that voters have accurate and full information regarding the costs of the referendum. In practice, there is ample evidence that voters regularly misperceive referenda costs and have limited information about how these costs will manifest (e.g., Burnett and Kogan 2015; Barth et al. 2020; Lang et al. 2025). Following Lang and colleagues (2025), we add misperception parameters, $\kappa_{1i} \geq 0$ and $\kappa_{2i} \geq 0$. These misperception parameters reflect the fact that perceived costs (property tax costs and other costs) may be distorted by voters, but are, in general, increasing in true costs. $\kappa_{1i} = \kappa_{2i} = 1$ implies no distortion, where perceived costs equal true costs; $\kappa_{1i} < 1$ implies property tax costs are underestimated; and $\kappa_{1i} > 1$ implies property tax costs are overestimated. Likewise, $\kappa_{2i} < 1$ implies other costs are underestimated; and $\kappa_{2i} > 1$ implies other costs are overestimated, where:

$$PropertyTaxCosts'_i = \kappa_{1i} PropertyTaxCosts_i, \text{ and}$$

$$OtherCosts'_i = \kappa_{2i} OtherCosts_i, \quad \text{and}$$

$$\begin{aligned} Y'^*_i &= \pi'_0 + \pi'_1 PropertyTaxCosts'_i + \pi'_2 OtherCosts'_i + \boldsymbol{X_i \gamma'} + \epsilon'_i \\ &= \pi'_0 + \pi'_1 \kappa_{1i} PropertyTaxCosts_i + \pi'_2 \kappa_{2i} OtherCosts_i + \boldsymbol{X_i \gamma'} + \epsilon'_i, \end{aligned} \quad (2)$$

where Y'^*_i is the voter's expected utility if the referendum passes with expected costs $\kappa_{1i} PropertyTaxCosts_i$ and $\kappa_{2i} OtherCosts_i$, and the voter supports the referendum if and only if $\mathbf{1}(Y'^*_i > 0)$.

Differencing equations (1) and (2) shows that when $\kappa_{1i} \neq 1$ or $\kappa_{2i} \neq 1$, voters may misperceive the utility they stand to experience relative to if true costs were known, where:

$$Y'^{*}_{i} - Y^{*}_{i} = \begin{cases} < 0, if\ \kappa_{1i} > 1\ and\ \kappa_{2i} \geq 1 \\ < 0, if\ \kappa_{1i} \geq 1\ and\ \kappa_{2i} > 1 \\ = 0, if\ \kappa_{1i} = 1\ and\ \kappa_{2i} = 1 \\ > 0, if\ \kappa_{1i} < 1\ and\ \kappa_{2i} \leq 1 \\ > 0, if\ \kappa_{1i} \leq 1\ and\ \kappa_{2i} < 1 \\ ??, if\ \kappa_{1i} < 1\ and\ \kappa_{2i} > 1 \\ ??, if\ \kappa_{1i} > 1\ and\ \kappa_{2i} < 1. \end{cases} \quad (3)$$

If both cost categories are over-estimated, voters underestimate the utility they would enjoy if the referendum passed and fewer voters support the referendum than under perfect information, while if both cost categories are under-estimated, voters overestimate the utility they would enjoy if the referendum passed and more voters support the referendum than under perfect information. When one category of costs is over-estimated while the other category is under-estimated, the net effect on expected utility and voter behavior is ambiguous.

In theory, introducing an ambiguous property tax disclaimer like the language required by HB3 weakly increases the salience of property taxes for voters relative to a pre-period where bond referenda did not include any property tax disclaimer ($\kappa_{1i}^{POST} \geq \kappa_{1i}^{PRE}$). If voters initially underestimate the impact of a bond on their property tax obligations ($\kappa_{1i}^{PRE} < 1$), an ambiguous property tax disclaimer could decrease misperception and reduce voting errors, depending on the magnitude of the correction.

If voters initially accurately estimate or overestimate the impact of a bond on their property tax obligations ($\kappa_{1i}^{PRE} \geq 1$), an ambiguous property tax disclaimer is likely to increase voting errors, and the resulting change in κ_{1i} is more likely to lead to a voting error in cases where property tax

increases are small. More detailed property tax disclaimers could increase or decrease κ_{1i}, depending upon the information they convey, but more detailed information about the expected property tax burden of a bond referendum can only decrease voting errors, assuming cost projections are accurate and property taxes are the exclusive funding mechanism (a common practice for school bonds in Texas). We return to this nuance in our discussion of our survey experiments' results.

IV. Data

We assemble a dataset of all bond referenda that were proposed to Texas voters between 2012 and 2024, representing 5,053 unique bond elections. For each bond referendum presented to Texas voters during our analysis period, public records from the Texas Bond Review board posted on data.texas.gov (and available in our replication package posted on the AEA RCT Registry) document the proposing government name, proposing government type (including a category for school districts), the county (or counties) associated with the proposing government, the election date, the proposed bond amount, the bond purpose, the proposition number, the number of votes for, the number of votes against, and the result of the bond election ("Carried" or "Defeated").[1] To these bond records, we merge county-level demographic data collected from the American Community Survey, including median income, median property taxes collected,

[1] Administrators from the Texas Bond Review Board informed us that public vote count data is only reliable starting in 2012, following a change in their data collection process. Before 2012, nearly half of bond elections in the public records (49.6%) are recorded as being decided by a single vote, with 1 vote for (0 against) if the bond measure passed and 0 votes for (1 against) if the bond measure failed. Administrators explained that before 2012, many bonds election results were recorded this way when election outcomes were documented but the vote count data was not available. However, they also indicated that a non-trivial share of water district bond elections are actually decided this way (e.g., when new land is being developed). Therefore, while bond elections outcomes are recorded reliably going back to 1997, we limit our sample to 2012–2024 to ensure we are able to estimate intensive and extensive margin impacts in a common sample. Robustness checks, described in section VI.B below, confirm that the results do not depend upon this sample restriction.

urbanicity, and population, which allow us to control for the socioeconomic context of the bond votes we analyze and conduct subgroup analyses based on county characteristics. We supplement these data with complementary information on North Carolina bonds, which we web-scraped from referenda webpages (on https://er.ncsbe.gov/) by launching a botted Firefox browser out of R and interacting with HTML source code.

For our survey experiments, we collect administrative data from Harvard's Digital Lab for the Social Sciences (National Sample; Experiment 1; N=737) and CloudResearch Connect (Texas sample; Experiment 2; N=1,485). All experiments were pre-registered on the AEA RCT Registry, and all pre-registrations, materials, survey instruments, data, and code can be accessed via the AEA RCT Registry. We report how we determined our sample size, all exclusions, all manipulations, and all measures in all experiments (Simmons et al. 2012).

In addition to recording participants' stated preferences regarding whether or not they would support a hypothetical bond measure (our primary dependent variable), we collect data on what factors respondents considered while making their decision to support or oppose a hypothetical school bond, their general preferences regarding state spending on K-12 education in their state, whether or not (to the best of their knowledge) their local school district had held a bond election since November 2020, and their expectations regarding how the hypothetical bond would affect their housing costs (TX sample only).

Experiment 1 was conducted online between March 18, 2025 and April 2, 2025 in a volunteer sample of 749 U.S. adults (737 passed the study's attention check) recruited by Harvard's Digital

Lab for the Social Sciences. Experiment 2 was conducted online between July 14, 2025 and October 31, 2025 via the CloudResearch Connect platform in a sample of 1,503 Texas adults (1,485 passed the study's attention check). The convenience samples that comprise the analytic sample for each study are not representative of the population of the United States (Experiment 1) or the state of Texas (Experiment 2). Appendix Table A1 describes the observable characteristics of respondents in each sample by treatment assignment. Respondents who participated in Experiment 2 were compensated with a payment of $1.00, corresponding to an average hourly rate of $13.48 (median hourly rate of $16.98) based on the average response time of 4 minutes 27 seconds and median response time of 3 minutes 32 seconds.

IV.A Summary Statistics

Table 1 documents that, prior to HB3, 76% of school bonds and 92% of municipal bonds passed in their relevant referenda. In the period after HB3, the percentage of school bonds that pass dropped to 63% while the percentage of municipal bonds fell only slightly (to 88%). The intensive margin of support follows a similar trend, with the average vote share in favor of school bonds falling from 60% to 54%, while the average vote share in favor of municipal bonds remained mostly flat, rising slightly from 79% to 80%.

Table 1 presents additional summary statistics on counties with local bond referenda using data from the U.S. Census: while counties that propose school bonds and municipal bonds have similar age distributions, rates of homeownership, and shares of residents who are parents of school-aged-children, school bonds were proposed in areas with lower median household incomes, educational attainment, median real estate taxes, total populations, and share of

residents living in urban areas, relative to municipal bonds. School bonds were proposed in areas with a slightly larger share of white residents, slightly smaller shares of Black or Asian residents, and similar share of Hispanic residents. This is reinforced by Figure 2, a heatmap of bond elections, which shows that while nearly every Texas county considered at least one school bond during our study period, municipal bond elections are primarily concentrated in and around East Texas's major urban centers. Our empirical strategy, presented below, can be adapted to account for these observed differences, as well as for other potential sources of unobservable heterogeneity. The separate school bond intercept in all models accounts for time invariant differences between school and non-school bonds. To specifically address concerns related to observable differences, we show results are consistent when we directly control for these factors using time-varying controls as a sensitivity test and estimate the models in subgroups by urbanicity tercile.

V. Econometric Approach

We estimate the influence of property tax disclaimers on voter preferences with a series of complementary analyses, beginning with DiD and DDD models that provide ecologically valid estimates of the effect of the ambiguous property tax disclaimers required by HB3 in Texas. Our quasi-experimental results narrow the set of scenarios that are consistent with our conceptual model following an exogenous increase in κ_{1i}. We pair these analyses with a series of survey experiments that replicate main findings and interrogate the specific mechanisms by which the property tax disclaimers affect cost perceptions and bond support.

V.A Differences-in-Differences Analysis

We estimate the causal effect of HB3 by exploiting differences in exposure to the "THIS IS A PROPERTY TAX INCREASE" language over time and across types of bonds. In our DiD approach, we compare changes over time in bond passage rates and votes in favor where voters saw the new language to contemporaneous changes in other types of municipal bond elections where voters did not see the new language. This strategy estimates the causal effect of HB3 under the assumption that changes in bond outcomes in non-school bonds provide a valid counterfactual for the changes that would have occurred had HB3 not been enacted. We specify our DiD approach as follows:

$$y_{imt} = \alpha_0 + \alpha_1 School\ Bond_i + \alpha_2 School\ Bond_i\, x\ Post_{mt} + \lambda_m + \pi_t + \epsilon_{imt}, \quad (4)$$

where y_{imt} is the electoral outcome for bond measure i in year t and month m. $School\ Bond_i$ is a binary variable indicating whether measure i was a bond for a K-12 school district, and $Post_{mt}$ is a binary variable equal to one if the month and year in which the measure is voted on is after the enactment of HB3 *following* the November 2019 election (Brock 2019). Functionally, this means that $Post_{mt}$ takes on a value of one for all elections held in 2020 or later. The parameter of interest is α_2, which represents the average effect of HB3 on bond outcomes under the core assumption noted above. We also show results with a non-parametric approach that allows the effect to vary in each of the post periods and allows us to examine the trends in school and non-school bonds prior to the enactment of HB3.

The terms λ_m and π_t represent month and year fixed effects, non-parametrically accounting for any year or month-specific variation in voter behavior and ensuring that the interaction between bond type and the post-period captures our estimand of interest. This approach mirrors the

specification that Grosz and Milton (2025) used in a recent study of the effect of the change in voting requirements on bond votes in California.

V.B Difference-In-Differences-In-Differences (Triple-Difference) Analysis

We supplement our DiD analysis with a DDD specification in which we also include bond election outcomes from North Carolina as a counterfactual secular trend. From 2013 to 2023, North Carolina had a similar regulatory framework to that of Texas prior to HB3, did not experience any changes to the required ballot text for bond referenda, and had a common policy governing all general obligation bonds (i.e., treating school bonds and other municipal bonds equivalently). We modify the baseline DiD model by limiting the sample to bond elections held between September 2013 and December 2023[2], adding a Texas state indicator (TX_s), a Texas-by-post interaction term ($TX_s\, x\, Post_{mt}$), a Texas-by-bond-type interaction term ($School\ Bond_i\, xTX_s$), and the triple interaction term of interest ($School\ Bond_i\, x\, Post_{mt} x\, TX_s$). This specification takes the following form:

$$y_{imtgs} = \alpha'_0 + \alpha'_1 School\ Bond_i + \alpha'_2 TX_s + \alpha'_3 School\ Bond_i\, xTX_s \quad (5)$$
$$+\alpha'_4 School\ Bond_i\, x\, Post_{mt} + \alpha'_5 TX_s\, x\, Post_{mt} + \alpha'_6 School\ Bond_i\, x\, Post_{mt} x\, TX_s$$
$$+\lambda'_m + \pi'_t + \epsilon'_{imts}.$$

For this DDD analysis, the parameter of interest is α'_6, as it represents the effect of HB3 under a somewhat less restrictive assumption because the DDD specification allows for differential

[2] In 2013 and 2024, North Carolina enacted two substantive changes to the required ballot language for all general obligation bonds, covering school bonds as well as other municipal bonds. On September 1, 2013, North Carolina enacted the provisions of House Bill 248, requiring a general tax disclaimer that "[…]that additional taxes may be levied in an amount sufficient to pay the principal of and interest on the bonds be approved" be added to all general obligation bond elections (N.C. Gen. Stat. § 159-61 2013). This language is similar to the required text for Texas bonds prior to HB3. On January 1, 2024, North Carolina enacted the provisions of House Bill 259, which require that detailed projections of the impact of bond referenda on property tax obligations be included in the ballot text of all bond referenda, similar to the detailed disclaimer in Experiment 2 (N.C. Gen. Stat. § 159-61 2024). These changes motivate our sample restriction for the DDD analysis.

trends between school and non-school bonds, so long as any such differential trends are common across Texas and North Carolina. This addresses potential concerns that estimated effects in the DiD analysis might have been driven by changing perceptions of schools during and after the COVID-19 pandemic or other education-specific secular trends, including the effects of the 2018 nationwide cap on the State and Local Tax deductions on school districts' ability to raise revenue via bonds (Ambrose and Valentin 2024).

V.C Survey Experiments

Our experimental design allows us to estimate the impact of our informational treatment via a simple regression of each outcome of interest on an indicator for being assigned to a particular Property Tax treatment group (Experiment 1 has two arms; Experiment 2 has four arms). In Experiment 1, we estimate the impact of an ambiguous property tax disclaimer on respondents' probability of expressing support for a hypothetical school bond proposal (versus an active control that omits the property tax language) via the following model:

$$y_i = \beta_0 + \beta_1 Treat1_i + \sum_{j=1}^{J} \gamma_j Char_{ji} + \phi_c + \epsilon_{ic}, \quad (6)$$

where y_i corresponds to a target outcome of interest [primary outcome = $1(VoteYes_i = 1)$] and $Treat1_i$ is a binary indicator for being assigned to the ambiguous property tax disclaimer (Treatment group 1).

Since we ask respondents to "[…]imagine that the school district proposing the bond were the school district where you live. Assume that all school district policies, conditions, and personnel are otherwise the same as they are today," we vary the size of the hypothetical bond based on the size of the county *c* where a respondent lives (<100,000 people; 100,000 – 1,000,000 people;

more than 1,000,000 people), which we account for with a set of county size fixed effects, ϕ_c. In both treatment conditions and both experiments, respondents in counties with populations under 100,000 evaluated a hypothetical bond proposal of $9,500,000; respondents in counties with populations between 100,000 and 1,000,000 evaluated a hypothetical bond proposal of $26,500,000; and respondents in counties with populations over 1,000,000 evaluated a hypothetical bond proposal of $82,500,000. We add a set of detailed covariates to the primary specification ($\sum_{j=1}^{J} Char_{ji}$) to improve precision and assess the stability of the results to adjustments for chance differences in observable characteristics between treatment groups. $\sum_{j=1}^{J} Char_{ji}$ is a vector of the observable characteristics reported in Appendix Table A1.

In Experiment 2, in addition to re-estimating the impact of the ambiguous property tax disclaimer in our sample of Texas adults, we add two additional treatment arms: a small property tax disclaimer and a large property tax disclaimer, mimicking the property tax messaging mechanisms in other states that have adopted similar reforms aimed at transparency in taxation and allowing us to probe the psychological mechanisms that mediate the effects of property tax disclaimers on preferences for public spending. We estimate the impact of each type of property tax disclaimer on respondents' probability of expressing support for a hypothetical school bond proposal via the following model:

$$y_i = \beta'_0 + \beta'_1 Treat1_i + \beta'_2 Treat2_i + \beta'_3 Treat3_i + \sum_{j=1}^{J} \gamma'_j Charj_i + \phi_c + \epsilon_{ic}, \quad (7)$$

where y_i, ϕ_c, and $\sum_{j=1}^{J} Charj_i$ are defined as above. $Treat1_i$ is a binary indicator for being assigned to the ambiguous property tax disclaimer, Treatment 1 (i.e., considering a hypothetical school bond proposal that ends with "THIS IS A PROPERTY TAX INCREASE"); $Treat2_i$ is a binary indicator for being assigned to the small property tax disclaimer, Treatment 2 (i.e.,

considering a hypothetical school bond proposal that ends with "THIS IS A PROPERTY TAX INCREASE. THE BONDS ARE EXPECTED TO RESULT IN A TAX INCREASE OF $0.011 PER $1,000 OF ASSESSED VALUATION OR $1.10 PER YEAR FOR A $100,000 HOME."); and $Treat3_i$ is a binary indicator for being assigned to the large property tax disclaimer, Treatment 3 (i.e., considering a hypothetical school bond proposal that ends with "THIS IS A PROPERTY TAX INCREASE. THE BONDS ARE EXPECTED TO RESULT IN A TAX INCREASE OF $0.11 PER $100 OF ASSESSED VALUATION OR $110.00 PER YEAR FOR A $100,000 HOME."). Appendix Table A1 provides descriptive statistics for our survey experiment samples, confirming that the treatment and control groups are balanced on observable characteristics, and suggesting that the randomizations were successful.

VI. Results

VI.A.1 DiD Effects on Bond Passage and Vote Share

Requiring school bond ballots to end with "THIS IS A PROPERTY TAX INCREASE" substantially decreases bond passage rates and vote shares in favor of school bonds. Column 1 of Table 2 displays quasi-experimental estimates of the effect of requiring property tax disclaimers on bond timing, passage rates, and votes in favor in our full sample of bond elections. Panel A shows the results from the DiD analysis in Texas. We find that the new property tax disclaimer decreases the overall probability of school bond passage by 9.1 percentage points, an 11.9% decrease relative to the control mean of 76.0%. This effect is concentrated in November elections, when the property tax disclaimer decreases the probability of passage by 22.8 percentage points (column 3), contrasting sharply with the null results during off-cycle elections

(column 2). Additionally, we find no evidence that requiring property tax disclaimers causes districts to strategically shift the timing of their school bond elections.

Figure 3 displays the mean passage rate in November elections over time in the raw data, and Figure 4 shows results from a traditional event study relative to the timing of the law. First, there is no evidence of differential trends in ballot passage prior to the law. After the property tax disclaimers begin to appear, school bond passage rates in November elections drop by roughly 15–30 percentage points, while the passage rate for other municipal bonds is unchanged. The policy impacts are stable and persistent through five years.

Results in Table 2 also demonstrate that the property tax disclaimers persistently lower the vote share in favor of school bonds. On average, the property tax disclaimer decreases the overall vote share in favor of bond passage by 7.5 percentage points, a 12.5% decrease relative to the control mean of 60.1%. This effect is again concentrated in November elections, when the property tax disclaimer decreases vote share in favor of passage by 12.5 percentage points. Notably, the property tax disclaimer to ballot language did not appear to affect school bond vote shares or passage rates during off-cycle elections, suggesting that general election voters are more responsive to ballot language. This is not surprising, given that off-cycle voters are older, and older voters are insulated from property tax changes by Texas state law (Kogan et al. 2018).

Interpreted through the lens of our conceptual model, the introduction of the ambiguous property tax disclaimers to school bonds following the passage of HB3 increases the value of the misperception parameter for the property tax share (κ_{1i}) for these bond referenda, such that

$\kappa_{1i}^{POST} > \kappa_{1i}^{PRE}$, while the change in κ_{2i} is ambiguous. The substantial decline in voter support for school bonds following the passage of HB3 suggests that the increase in κ_{1i} was decisive for a non-trivial share of voters [i.e., making $P\left(Y'^{*,POST}_i < 0\right) > P(Y'^{*,PRE}_i < 0)$]. Ceteris paribus, if κ_{1i}^{PRE} was less than 1, this recalibration could have the effect of reducing voting g errors, depending on the magnitude of the initial error (i.e., $1 - \kappa_{1i}^{PRE}$) and the magnitude of the correction (i.e., $\kappa_{1i}^{POST} - 1$). If κ_{1i}^{PRE} was greater than or equal to 1, this recalibration could only increase voting errors, holding all else equal.

VI.A.2 DDD Effects on Bond Passage and Vote Share

We use bond passage rates for school and non-school bonds in North Carolina in a differences-in-differences-in-differences (DDD) framework to address a potential concern that the effects observed above are driven by spurious secular trends in school bond passage rates. In particular, some may be concerned about the specific timing of the law in relation to the COVID-19 pandemic. We therefore scraped school and non-school bond passage rates in North Carolina from 2013-2023, a period where North Carolina experienced no policy changes related to the ballot text of bond referenda. North Carolina is an ideal additional counterfactual because, in North Carolina during this period both school bond and municipal bond referenda were treated equivalently and had similar requirements to Texas bonds prior to HB3 (Brunner et al. 2021; N.C. Gen. Stat. § 159-61 2013; N.C. Gen. Stat. § 159-61 2024). North Carolina also had a roughly similar experience with pandemic-induced school closures as Texas, making it a reasonable counterfactual for COVID-related changes in voter support for public school investments (COVID-19 School Data Hub, n.d.).[3]

[3] We also show results dropping 2020 in Table 3.

Results are presented in Panel B of Table 2, estimated via Equation (5) above. The DDD estimates replicate the results from the DiD analysis—if anything the DDD estimates suggest that the decline in Texas school bond passage rates is even larger than expected once the out-of-state secular trend is accounted for. The estimated coefficient on the triple interaction term implies that HB3 reduced school bond passage rates by 12.4 pp in Texas. This effect is considerably larger in November elections, where we estimate a 24.1 pp decline in passage rates. Figure 5 presents the DDD findings as an event study, confirming that the relative decline in school bond election outcomes was contemporaneous with the passage of HB3. Relatedly, the new property tax disclaimer decreases vote shares in favor by 8.4 pp in the full sample and 12.1 pp in November elections.

VI.A.3 DiD Effects on Amount Raised

There is some evidence that local school districts' ability to raise funds through bond measures decreases after the language change. We conduct a DiD analysis using a panel of governments in five-year bins (2010-2014; 2015-2019; 2020-2024) with the outcome as the total amount raised (in millions of real 2024$) for that government in a given five-year period. We include fixed effects for five-year bins (λ_f) and government names (δ_g), and standard errors are clustered at the government level. We use a specification that takes the following form:

$$y_{fg} = \alpha_0 + \alpha_1 School_g \, x \, Post_{fg} + \lambda_f + \delta_g + \epsilon_{fg}. \quad (8)$$

First, we show the residualized amount raised in the 10 years prior to the policy change in Appendix Figure A1. The plotted clusters of residuals demonstrate that the amount raised in school districts and other governments trend similarly between 2010 and 2019. Second,

Appendix Table A2 shows the estimated impact of requiring ballot language on the amount that governments raise. On average, we estimate that Texas's ambiguous property tax disclaimers caused school districts to lose roughly $61 million over the five-year period after enactment, an over 50% reduction relative to a control mean of $109 million, and representing $127 per resident of a school-bond-proposing county. Consistent with other analyses, the treatment effect is larger and remains significant if we limit it to the amount raised in November elections (-$69 million) and marginally significant but still large (-$53 million) in off-cycle elections.

VI.A.4 Survey Experiment Results

Our survey experiments allow us to A) validate property tax disclaimers as a credible explanation for the substantial decline in bond passage rates following the passage of HB3 in Texas, B) probe the underlying mechanisms that explain the influence of property tax disclaimers on voter preferences, C) assess whether the effects of property tax disclaimers vary by the precision of information conveyed by the disclaimer, and D) assess whether and when ambiguous property tax disclaimers improve information (i.e., by shifting κ_{1i} closer to 1). In both experiments, we measure rates of school bond support among respondents who consider a bond that ends with the ambiguous property tax disclaimer (Treatment 1; "THIS IS A PROPERTY TAX INCREASE") mandated by HB3 versus a control group who consider an identical bond that omits this disclaimer. In Experiment 2, we add two additional treatment arms that provide specific information about changes in property taxes that are projected to be required to finance the proposed bond, where Treatment 2 describes a scenario where the bond proposal that is expected to have minimal effects on property tax obligations ("THIS IS A PROPERTY TAX INCREASE. THE BONDS ARE EXPECTED TO RESULT IN A TAX

INCREASE OF $0.011 PER $1,000 OF ASSESSED VALUATION OR $1.10 PER YEAR FOR A $100,000 HOME."), and Treatment 3 describes a scenario where the bond proposal is expected to have larger effects on property tax obligations ("THIS IS A PROPERTY TAX INCREASE. THE BONDS ARE EXPECTED TO RESULT IN A TAX INCREASE OF $0.11 PER $100 OF ASSESSED VALUATION OR $110.00 PER YEAR FOR A $100,000 HOME."). Appendix Figure A2 shows examples of the hypothetical bond propositions considered by the control group (panel A), the ambiguous property tax treatment group (panel B), and the large, detailed property tax treatment group (panel C) in a mid-sized county.

Table 4 demonstrates that in both experimental samples, we find that respondents who consider a bond proposal that ends with the ambiguous property tax disclaimer language mandated by HB3 are substantially less likely to say they would vote for that school bond (-9.6 pp in Experiment 1; -12.0 pp in Experiment 2), and in neither case does respondents' diminished enthusiasm for financing public investment in schools via the proposed bond reduce their general support for increasing spending on public schools.

Table 5 allows us to interrogate the psychological processes underlying this change in preferences. In both samples, adding an ambiguous property tax disclaimer to a standard school bond proposal substantially increases the probability that respondents report ex-post that they considered property taxes when deciding whether or not to support the proposed bond (+14.1 pp in Experiment 1; +16.2 pp in Experiment 2). Our survey experiment provides direct evidence for the hypothesis that property tax disclaimers increase the salience of property tax costs in voters' deliberative process, showing that $\kappa_{1i}^{TREATMENT1} > \kappa_{1i}^{CONTROL}$ and providing evidence to support

the interpretation that $\kappa_{1i}^{POST} > \kappa_{1i}^{PRE}$ in our quasi-experimental setting. The substantial decline in support for hypothetical school bonds that included property tax disclaimers [i.e., $P\left(Y'^{*,TREATMENT1}_{i} < 0\right) > P(Y'^{*,CONTROL}_{i} < 0)$] shows that the change in perception induced by ambiguous property tax disclaimers is decisive for a meaningful share of voters.

In both samples, this increased focus on property taxes is partially offset by a reduced focus on sales taxes (-5.6 pp in Experiment 1; -3.7 pp in Experiment 2), suggesting that respondents who believe a school bond will be financed by raising sales taxes are more likely to support bonds relative to their peers who believe that bond passage will impact their property tax obligations.

However, when we consider how the property tax disclaimers influence respondents' attention to the benefits of the hypothetical bond propositions they considered, a stark contrast emerges. In Experiment 2, respondents who were exposed to the ambiguous property tax disclaimer were much less likely to report considering factors related to stated goals of the proposed school bond. Treated respondents were 11.8 pp less likely to consider school building condition and 11.4 pp less likely to consider technology in schools, suggesting a shift in focus away from the potential benefits of the bond (i.e., improvements in school building condition or investments in technology in schools) and toward its costs (i.e., property taxes). In Experiment 1, there is no relationship between exposure to the ambiguous property tax treatment and respondents' consideration of these types of benefits. The contrasting patterns of substitution suggest that the considerations that motivate voters to support or oppose school bonds likely varies by context.

Experiment 2 also allows for a more nuanced examination of when and why property tax disclaimers reduce support for school bonds. Most straightforwardly, property tax disclaimers increase the salience of the bond proposal's cost, likely increasing respondents' perceptions regarding the effect of the bond on their housing costs (either directly via property taxes or indirectly via landlords charging higher rents). If this is a primary driver of preference formation, we would expect respondents' preferences to be sensitive to the projected impact of the bond on their property tax obligations. Alternatively, it could be the case that property tax disclaimers function as political "dog whistles" signaling to fiscal conservatives that they should oppose this type of fiscal expansion, with or without accounting for their personal exposure to increased costs. If this is a primary driver of preference formation, we would expect changes in preferences to be driven by political conservatives and for these respondents' preferences to be less sensitive to the projected impact of the bond on their property tax obligations, reflecting a general rather than conditional aversion to new taxes.

Tables 4, 5, and 6 provide evidence to test these hypotheses. While respondents are just as likely to attend to property tax considerations when projected impacts on property taxes are provided alongside the ambiguous property tax disclaimer language required by HB3 (+28.6 pp for the small tax change [Treatment 2]; +29.2 pp for the large tax change [Treatment 3]), the "small tax change" described in Treatment 2 did not reduce school bond support relative to the control group that received no information regarding property taxes. Conversely, the "large tax change" Treatment 3 group was even less likely to support the bond (-18.4 pp) than their peers who were exposed to an ambiguous tax disclaimer (Treatment 1; -12.0 pp).

Comparing the effects of ambiguous property tax disclaimers versus the effects of detailed property tax disclaimers allows us to assess whether and when ambiguous property tax disclaimers improve or degrade voter information, reducing or increasing voter errors. Table 4 demonstrates that ambiguous property tax disclaimers reduce voter support for hypothetical school bonds by about 12 pp, about two-thirds of the impact of a detailed disclaimer that projects a large impact on property taxes. This suggests that in this hypothetical case, where the bond referendum is projected to substantively increase property tax obligations (e.g., +$110 per $100,000 in home value), the ambiguous property tax disclaimer reduces voter errors relative to the control condition of no property tax disclaimer, attenuating κ_{1i} toward 1 on average ($|1 - \kappa_{1i,Large}^{TREATMENT1}| < |1 - \kappa_{1i,Large}^{CONTROL}|$). However, the fact that the large, detailed property tax disclaimer degrades support for the bond even more (-18 pp versus -12 pp) suggests that in this case, even with the ambiguous disclaimer, voters underestimate the impact of the bond on property taxes (i.e., $\kappa_{1i,Large}^{TREATMENT1}$ remains less than 1), and a detailed disclaimer would further improve information in this case ($|1 - \kappa_{1i,Large}^{TREATMENT3}| < |1 - \kappa_{1i,Large}^{TREATMENT1}|$).

In the case of a bond referendum that is projected to have a small impact on property taxes (e.g., +$1.10 per $100,000 in home value), the ambiguous property tax disclaimer *increases* voter errors relative to the control condition of no property tax disclaimer, shifting κ_{1i} away from 1 ($|1 - \kappa_{1i,Large}^{TREATMENT1}| > |1 - \kappa_{1i,Large}^{CONTROL}$). In this case, removing the disclaimer altogether would reduce voter errors relative to including an ambiguous disclaimer, since voters are indifferent between bonds with and without the detailed disclaimer when the impacts on property tax obligations are small.

If voter preferences scale linearly with the size of the change in property tax obligations (a non-trivial assumption), we can use the magnitudes of the effect of Treatment 3 (the large property tax treatment) and Treatment 2 (the small property tax treatment) to make a back-of-the-envelope estimate of the size of the detailed property tax disclaimer that would lead to a reduction in support equivalent to the observed impact of the ambiguous property tax disclaimer (-11.95 pp). Fitting a line between the two points ($1.10/$100K = -1.58 pp and $110/$100K = -18.36 pp) suggests that voters treat ambiguous property tax disclaimers as if the bond included a detailed disclaimer projecting that the bond would increase property taxes by $68.40 per $100,000 in home value. Under these conditions, the extent to which an ambiguous property tax disclaimer increases or decreases voting errors will depend upon the magnitude of the actual projected change in property taxes relative to this benchmark.

Puzzlingly, neither the ambiguous property tax disclaimer nor the large, detailed property tax disclaimer substantively influenced participants' estimates regarding the impact of the school bond's passage on their housing costs. However, respondents who considered the small, detailed property tax disclaimer reported $18.71 *lower* expected changes in housing costs (suggesting that $\kappa_{1i}^{TREATMENT2} < \kappa_{1i}^{CONTROL}$), representing a substantial reduction relative to the control mean of $43.03 and suggesting that even in the absence of property tax disclaimers, voters do recognize that large public investments in school quality can influence housing costs. This demonstrates that when bond obligations are projected to be offset or fully financed using new revenue from a growing tax base (e.g., because of new construction or rising home values) rather than increasing property tax rates, detailed cost projections may actually influence voters to revise their estimates of the bond's impacts on housing costs *downward*.

Table 6 demonstrates that the effect of property tax disclaimers on bond support is similar across the political spectrum. Respondents of all political affiliations are less likely to support bond propositions that include an ambiguous or large, detailed property tax disclaimers, and no political subgroup is discouraged from supporting bonds that include small, detailed property tax disclaimers. Despite substantive differences in baseline levels of support for bond propositions, we find no clear evidence that the relationship between property tax salience and bond support is moderated by political affiliation. Altogether, this suggests that concerns about fiscal impacts rather than partisan politics are central drivers of voters' aversion to ballot measures that include property tax disclaimers.

VI.B Robustness

VI.B.1 DiD Robustness

In Table 3, we show that the large, substantive decline in support for school bonds following the introduction of property tax disclaimers is not driven by any particular modeling approach. When we add controls for population, urban share, a full set of county-year covariates, prune potential outlier periods (COVID and Great Recession), exclude single-vote elections, or expand the study period back to 1996, we estimate that the passage of HB3 led to 9.1pp-11.3pp declines in the passage rate of school bonds relative to municipal bonds and 7.5pp-13.0pp decline in school bonds' vote share. In column 2 of Table 3 we add a control for the natural logarithm of the population, to directly account for the fact that municipal bond elections are more common in larger, urban centers. In column 3, we replace the population control with a mutually inclusive and exhaustive set of indicators for locale type (i.e., rural, town, city), as measured by the U.S.

Census Bureau. In column 4, we add all of the time-varying county-level covariates in Table 1 (including the controls in columns 2 and 3): percent Hispanic, percent Black, percent Asian/Pacific Islander, percent parents, percent aged 30-40, percent aged 50 or above, percent with bachelor's degrees, percent homeowners, percent residing in urban areas, median household income (real 2024$), median property tax (real 2024$), and total population (logged). In column 5, we remove the year in which the COVID pandemic began (2020). In column 6, we remove single-vote elections (primarily water district bond elections). In column 7, we expand the panel to include bond election outcomes going back to 1997 (acknowledging that vote counts are poorly measured before 2012). Across all modifications, we find that the timing of HB3's enactment is associated with large, sustained declines in school bonds passage rate and vote share, and, if anything, these negative effects grow in magnitude relative to our preferred specification. Impacts on passage rates range from -9.1 pp to -11.3 pp in all elections and -21.9 pp to -24.7 pp in November elections (all *p*s<.01). Likewise, impacts on the share of votes in favor range from -6.0 pp to -13.0 pp in all elections and -11.0 pp to -16.4 pp in November elections (all *p*s<.001).

Additionally, we examine whether effects vary based on the share of the county residing in urban areas in Appendix Table A3. We create three subsamples based on urbanicity. Effects are consistently large and negative within each of these urbanicity subsamples, though standard errors increase in these smaller subsamples, with the largest effects on bond passage concentrated in the most urban tercile.

Table A4 replicates our DiD and DDD results over different time periods. Because North Carolina introduced new ballot language in 2013 and 2024, our preferred DDD sample excludes bond elections held before September 1, 2013 or after January 1, 2024. The results in Table A4 estimate our DiD and DDD models in four common samples, systematically including or excluding all groups of bond elections that were affected by the North Carolina policy changes. These results demonstrate that the large, substantive decline in support for school bonds following the introduction of property tax disclaimers in Texas is not driven by any particular sample restriction. However, models that include North Carolina bond elections in 2024 as a part of the comparison group lead to much more negative point estimates, suggesting that municipal bonds and school bonds in North Carolina may have been differentially impacted by the 2024 policy change requiring detailed property tax disclaimers on all general obligation bond elections, justifying our sample restriction (and presenting a potential pathway for future research).

We also examine whether voters become desensitized to the new language by allowing the effect of the ballot language to vary between the first affected election and later affected elections. We implement this by interacting our main effect with a binary indicator that takes on a value of one if a school bond election occurs after the first school bond election after the policy change, and zero otherwise. We find no evidence of desensitization (see Appendix Table A5). Interacting our main treatment with the later treatment indicator slightly attenuates the main effect, and the coefficient on the interaction term is negative but not significant. This suggests that, if anything, the effect of the ballot language may *grow* in subsequent bond elections.

Finally, we test for possible policy spillover onto municipal bond passage rates and vote shares by examining whether municipal bonds that co-occur with school bonds are affected by the policy change. Specifically, we limit the sample to municipal bonds and mark as "treated" any municipal bond issued at the same time and in the same county as a school bond in the post period in Appendix Table A6. We also test this in a sample limited to municipal bonds in counties that ever issue school bonds. We find no evidence of spillover effects on co-occurring municipal bonds.

VI.B.2 Survey Experiment Robustness

Appendix Tables A7 and A8 assess the robustness of the main results from our survey experiments in the nationwide and Texas samples, respectively. Our preferred estimates of the impact of property tax disclaimers on survey respondents' stated preferences condition on the baseline covariates listed in Appendix Table A1 and exclude respondents who failed an embedded attention check. In each survey sample, we re-estimate our findings without any covariates and without any sample exclusions. The results are substantively unchanged, regardless of these differences in specification. Appendix Table A9 presents estimates of the impact of property tax language on the complete set of exploratory mechanisms we considered, as outlined in our study pre-registration.

VII. Discussion

In 2019, the Texas Legislature unanimously passed House Bill 3 (HB3), requiring that all school bond ballot propositions include the phrase "THIS IS A PROPERTY TAX INCREASE." We demonstrate that this property tax disclaimer sharply reduces support for Texas school bonds. As

a result of the policy change, average vote share in favor of school bonds has fallen by 7.5 pp, and by 12.5 pp in November elections. School bond passage rates have declined by 9.1 pp overall with a staggering 22.8 pp decline in November elections. These declines have led to measurable decreases in the ability of school districts to raise funds through bonds, causing a greater than 50% reduction in the funds raised in bond-proposing districts over the five-year period following the enactment of HB3. This is consistent with prior descriptive and experimental evidence on property tax disclaimers in the public finance literature (Brunner et al. 2018, 2021).

We validate and expand on these findings in a series of survey experiments. In both Texas and national samples, property tax disclaimers cause roughly 10–20 pp decreases in support for hypothetical school bonds. The larger effects stem from tax disclaimers that are either ambiguous or paired with information about larger property tax increases, while support for bond measures that include the disclaimer alongside information about smaller property tax increases is unaffected by the disclaimer. However, we find no evidence that property tax language affects overall support for school spending, suggesting that the impact reflects aversion to property taxes rather than reduced support for public schools. The survey experiment also allows us to measure mechanisms, demonstrating that property tax disclaimers shift voters from thinking about potential benefits (e.g., technology or instruction) to potential costs (the sources of tax revenues).

VII.A Theoretical Implications

These findings speak to broader questions about cost perceptions and voter welfare. We expand prior work from Lang and colleagues (2025) to develop a generalizable model of voter choice where voter approval of a ballot referendum is decreasing in both the perceived property tax costs *and* the other costs associated with the referendum, and preferences for these costs are separable. We then provide evidence that voter preferences over property tax costs and other costs are indeed separable. We find both a sharp decline in voter support and passage rates for school bonds after the property tax disclaimers, and experimental evidence demonstrating that property tax disclaimers significantly reduce voter support for school bonds even when total costs do not change and when voters do not expect bond passage to lead them to experience higher housing costs. Additionally, when voters consider a small property tax disclaimer alongside a bond proposition that they expect will decrease their housing costs, they do not increase their support for the bond, suggesting that merely considering property tax obligations may influence voter preferences negatively (e.g., by raising the bar for support).

Proponents advocate for tax disclaimers on the grounds that they improve voter welfare by increasing the information that voters have at the ballot box (Texas House Committee on Ways & Means 2023; Quintero and Solin 2024). More broadly, literature on "fiscal illusion" has also shown that citizens misperceive tax burdens, particularly when they are not salient (Cabral and Hoxby 2012; Kim and Sorensen 2019; Afonso 2014; Lang et al. 2025). However, our findings demonstrate that property tax disclaimers can also introduce a new form of distortion by selectively increasing the salience of one cost dimension (property taxes) without providing commensurate information about total costs or benefits. Salience is not the same as accuracy, and

policies designed to increase transparency can worsen voter decision-making if they provide incomplete information.

VII.B Policy Implications

This study has implications for policymakers in Texas and beyond. The most relevant finding for school district policymakers in Texas is that voters treat ambiguous property tax disclaimers similarly to detailed disclaimers that forecast large property tax increases. This matters because ambiguous tax disclaimers are not necessarily informing voters about costs; they are spurring them to think more about property taxes as they are making decisions about whether to support their local school bonds—in Texas, the same disclaimer is attached to bonds that require substantial increases in property tax obligations as those that are projected to be financed without increasing property tax rates at all (e.g., Khan 2024). Districts that want their voters to make informed decisions now have an even stronger incentive to go beyond required statutory language and provide specific, credibly accurate estimates of the expected property tax impacts, a practice many districts already employ (Quintero and Solin 2024). Indeed, even in the absence of a genuine desire to improve voter information, districts that are issuing bonds with small projected property tax increases will likely see higher bond passage rates if they include precise information about the expected property tax increases.

For state policymakers in Texas and legislators in other states considering similar disclaimers, the findings point to a straightforward design problem with the ambiguous property tax disclaimer as implemented in HB3. A disclosure requirement intended to improve voter information should, at a minimum, provide voters with accurate and specific information about

what they are being asked to approve. Ambiguous property tax disclaimers do not clearly meet this standard. If the goal is to have more informed voters, replacing ambiguous disclaimers with specific, quantified cost estimates would better serve that goal.

However, if the goal is to curb expansions of government spending, our results suggest that the ambiguous property tax disclaimers are an effective tool. Ambiguous property tax disclaimers produce large, persistent negative effects on school districts' abilities to raise revenues through bonds. Fiscal conservatives may view this as evidence that the policy is working as intended, on the grounds that voters who oppose bond passage after seeing a disclaimer are responding rationally to information about their tax obligations. Our evidence is consistent with this possibility, though voters are sensitive to the details in tax disclaimers, when provided. Property tax disclaimers paired with more detailed information about small increases do not deter voters, implying that maximal transparency does not always reduce spending. Given that ambiguous tax disclaimers are treated similarly to those paired with large increases, there are cases where an ambiguous disclaimer may be less of a transparency measure and more of a blunt instrument for deterring bond passage.

Here, we should recognize that the normative implications of the findings depend critically on the choice of the counterfactual. Specifically, the mandatory disclaimer requirement implicitly invites voters to consider what they would pay in property taxes in the absence of bond passage. But this counterfactual is not clearly defined. On the one hand, absent bond passage, local governments' total fiscal obligations could fall, and savings could be returned to taxpayers via lowering property taxes (Quintero and Solin 2024). On the other hand, excess revenues may

simply be redirected toward other public expenditures if property tax rates are not lowered. In contexts where rising home values render projected bond-related millage increases negligible, the latter counterfactual may be more relevant (Khan 2024). Which counterfactual is more accurate varies depending on contextual circumstances, including state and local laws, the structure of existing tax levies, and political norms. Regardless, our results underscore that the choice is not neutral: the disclaimer requirement produces large and durable effects on voter behavior at least in part because it foregrounds one counterfactual over the other.

The design of ballot measures and the information that they are required to contain are not neutral, technical details. A property tax disclaimer requirement is a political choice with distributional consequences for public education. In effect, it is a school finance policy, operating through a voter nudge. Indeed, despite the failure of many "nudge"-style informational interventions to replicate at scale (Hreha 2020; DellaVigna and Linos 2022), our findings suggest that such an intervention is actively reshaping school finance for Texas's 5.5 million public school children.

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Figure 1

Sample Bond Propositions

(A) School Bond

Dripping Springs Independent School District Bond Election

Del Distrito Escolar Independente de Dripping Springs Aviso De Elección Para La Emision De Bonos

Dripping Springs ISD Proposition A

"THE ISSUANCE OF $399,700,000 OF BONDS FOR THE CONSTRUCTION, ACQUISITION AND EQUIPMENT OF SCHOOL BUILDINGS IN THE DISTRICT, INCLUDING A NEW HIGH SCHOOL, FOR THE PURCHASE OF NECESSARY SITES FOR SCHOOL BUILDINGS, FOR THE PURCHASE OF NEW SCHOOL BUSES, AND FOR THE RETROFITTING OF SCHOOL BUSES WITH EMERGENCY, SAFETY, OR SECURITY EQUIPMENT, AND THE LEVYING OF A TAX SUFFICIENT TO PAY THE PRINCIPAL OF AND INTEREST ON THE BONDS AND THE COSTS OF ANY CREDIT AGREEMENTS EXECUTED IN CONNECTION WITH THE BONDS. THIS IS A PROPERTY TAX INCREASE."

Dripping Springs ISD Propuesta A

"LA EMISIÓN DE $399,700,000 EN BONOS PARA LA CONSTRUCCIÓN, LA ADQUISICIÓN, Y EL EQUIPAMIENTO DE ESTABLECIMIENTOS EDUCATIVOS DEL DISTRITO, INCLUIDA UNA NUEVA ESCUELA SECUNDARIA, PARA LA COMPRA DE LOS TERRENOS NECESARIOS PARA LOS ESTABLECIMIENTOS EDUCATIVOS, PARA LA COMPRA DE AUTOBUSES ESCOLARES NUEVOS, Y PARA LA MODERNIZACIÓN DE LOS AUTOBUSES ESCOLARES CON EQUIPOS DE EMERGENCIA, PROTECCIÓN O SEGURIDAD, ASÍ COMO LA APLICACIÓN DE UN IMPUESTO SUFICIENTE PARA PAGAR EL CAPITAL Y EL INTERÉS DE DICHOS BONOS Y LOS COSTOS DE CUALQUIER ACUERDO DE CRÉDITO IMPLEMENTADO O AUTORIZADO EN RELACIÓN O EN CONEXIÓN CON DICHOS BONOS. ESTO ES UN AUMENTO DEL IMPUESTO SOBRE LA PROPIEDAD."

◯ For / *A Favor*

◯ Against / *En Contra*

(B) Municipal Bond

Collins Municipal Utility District No. 1 A Confirmation Election, A System Facilities Bond Election, A Park And Recreational Facilities Bond Election, A Maintenance Tax Election, A Road Bond Election, A Refunding Bond Election and A Permanent Directors Election

Una Elección De Confirmación, Una Elección De Bonos Para Instalaciones Del Sistema, Una Elección De Bonos Para Parques E Instalaciones Recreativas, Una Elección De Impuesto De Mantenimiento, Una Elección De Bonos Para Caminos, Una Elección De Bonos De Reembolso Y Una Elección De Directores Permanentes Para El Distrito De Servicios Públicos Municipales Nro. 1 De Collins

Collins Municipal Utility District No. 1 Proposition E

The Issuance Of $18,450,000 Bonds For Park And Recreational Facilities. Taxes Sufficient To Pay The Principal Of And Interest On The Bonds Will Be Imposed.

Distrito De Servicios Públicos Municipales Nro. 1 De Collins Propuesta E

La Emisión De $18,450,000 En Bonos Para Parques E Instalaciones Recreativas. Se Impondrán Impuestos Suficientes Para Pagar El Capital Y El Interés De Los Bonos.

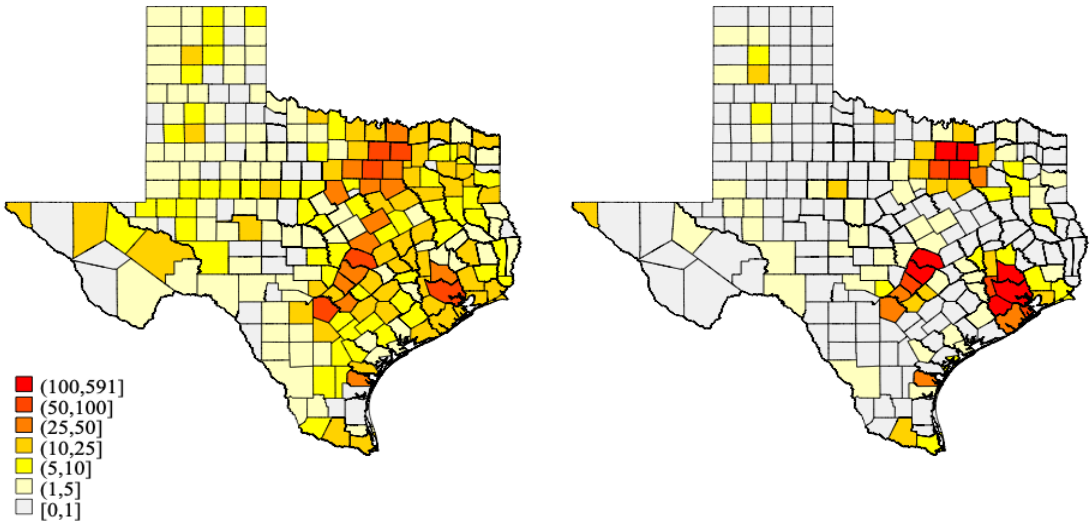
◯ For / *A Favor*

◯ Against / *En Contra*

Figure 2

Bond Heatmap by County and Bond Type (Number of Bond Elections 2012-2024)

(A) School Bonds (B) Municipal Bonds



Notes. The maps display the number of school and municipal bonds at the county level in Texas between 2012-2024.

Table 1

Baseline Government Characteristics 2012-2024

	School Bonds				Municipal Bonds			
	Mean	S.D.	Min.	Max.	Mean	S.D.	Min.	Max.
Panel A. Outcomes 2012-2019								
Percentage Won	75.99	42.74	0.00	100	92.04	27.08	0.00	100
Percentage Voting For	60.13	15.64	5.98	100	78.80	20.48	0.00	100
Observations	1,037				1,369			
Panel B. Outcomes 2020-2024								
Percentage Won	63.23	48.24	0.00	100	87.77	32.77	0.00	100
Percentage Voting For	54.39	13.13	0.00	100	79.96	25.66	0.00	100
Observations	1,281				1,366			
Panel C. County Characteristics 2012-2019								
Percentage White	56.99	18.96	0.95	98.05	48.00	16.41	2.65	88.19
Percentage Hispanic	30.93	19.99	1.08	98.71	31.69	14.23	4.73	95.50
Percentage Black	8.24	6.98	0.00	33.74	12.00	6.89	0.00	33.74
Percentage Native	0.33	0.30	0.00	1.91	0.24	0.14	0.00	1.80
Percentage Asian/Pacific Islander	1.99	2.84	0.00	19.77	6.20	5.20	0.00	20.07
Percentage Other Race	2.93	1.60	0.00	13.84	3.58	1.12	0.13	8.23
Percentage Parents	19.42	3.84	5.45	27.93	21.32	3.04	9.47	27.30
Percentage Age 5-17	18.29	2.23	5.03	27.67	19.25	1.67	10.74	25.02
Percentage Age 18-29	16.11	3.71	5.28	38.83	16.55	2.72	8.14	27.78
Percentage Age 30-49	25.36	3.14	16.20	41.84	28.23	2.63	16.44	33.28
Percentage Age 50+	33.53	6.33	19.44	59.01	28.87	4.50	20.52	58.93
Percentage B.A. or Higher	21.59	9.57	4.38	52.35	32.54	9.88	5.43	52.35
Percentage Homeowners	69.54	8.16	44.79	91.65	64.79	8.80	50.01	85.33
Median Household Income (Real 2024$)	33889.72	6849.96	17115.57	57029.22	40812.25	7388.42	19443.54	57029.22
Median Real Estate Taxes Paid (Real 2024$)	2592.16	1475.36	395.89	7419.60	4461.99	1503.40	395.89	7419.60
Total Population (Millions)	0.40	0.86	0.00	4.69	1.51	1.62	0.00	5.77
Percentage Urban	51.74	34.95	0.00	99.50	85.34	20.66	0.00	99.50
Observations	1,037				1,369			

Notes. Panels A and B split key outcomes by the periods prior to, and then following, the 2019 property tax ballot language change that impacted Texas school bond measures. County characteristics are derived from Census data prior to the ballot language change and contain linearly intrpolated values for missing observations.

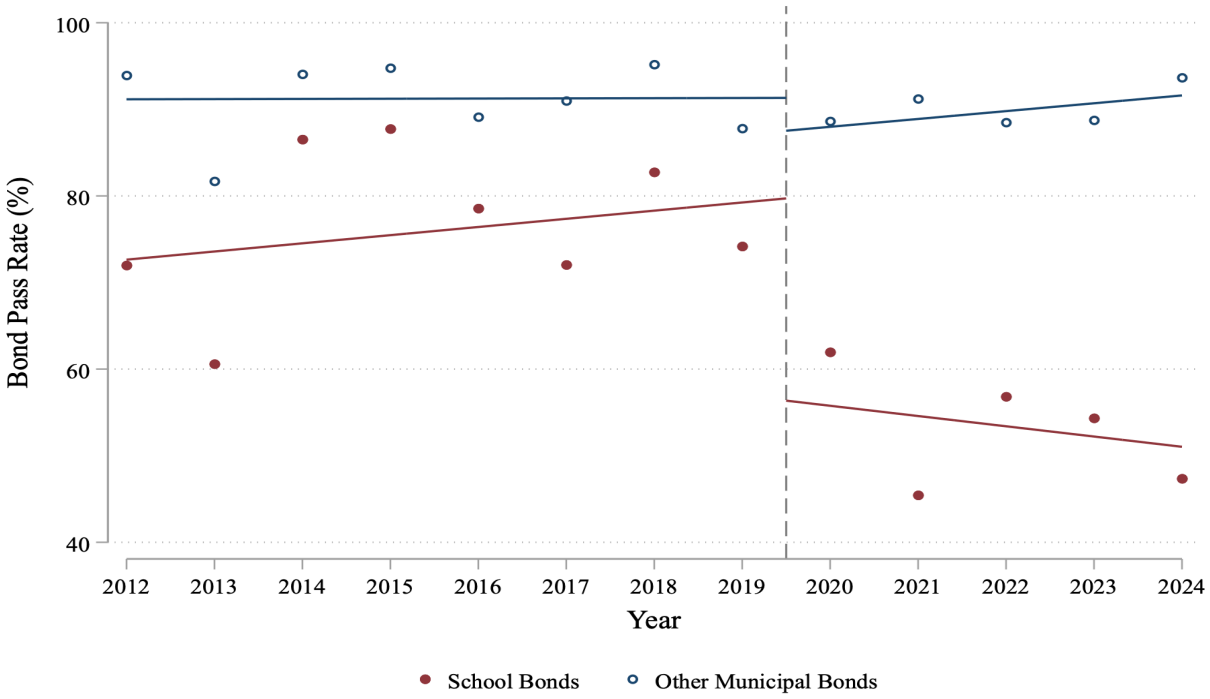
Table 2

Impact of Property Tax Ballot Language on Bond Passage Rates (DiD and DDD)

Outcome (pp)	Control Mean	All Elections	Off-Cycle Elections	November Elections
		(1)	(2)	(3)
Panel A. DiD in Texas (2012-2024)				
Bond Election Held in November	39.34	3.41		
		(4.36)		
Observations		5,053		
School Bond Pass Rate	75.99	-9.07**	2.61	-22.79***
		(3.02)	(4.06)	(4.23)
Observations		5,053	2,654	2,399
School Bond Vote Share	60.13	-7.54***	-.10	-12.46***
		(1.79)	(2.94)	(2.27)
Observations		5,053	2,654	2,399
Panel B. DDD in Texas and North Carolina (2012-2023)				
Bond Election Held in November	39.98	19.30		
		(18.05)		
Observations		4,521		
School Bond Pass Rate	75.99	-12.44+	18.75	-24.14**
		(6.88)	(12.15)	(7.85)
Observations		4,521	2,251	2,270
School Bond Vote Share	60.05	-8.36*	10.52	-12.12*
		(4.06)	(10.71)	(5.01)
Observations		4,521	2,251	2,270

Notes. Where property tax ballot language changes after 2019; all specifications include fixed effects for year, an indicator for a school bond election, and the interaction term of interest; and standard errors are clustered at the government level. Specifications in column 1 include month fixed effects. Specifications in Panel B include an indicator for Texas, a Texas-by-post interaction term, a Texas-by-bond-type interaction term, and the triple interaction term of interest. The control means reflect average values for each outcome from school bond elections prior to property tax ballot language changes. +=p<0.10 *p<0.05 **p<0.01 ***p<0.001

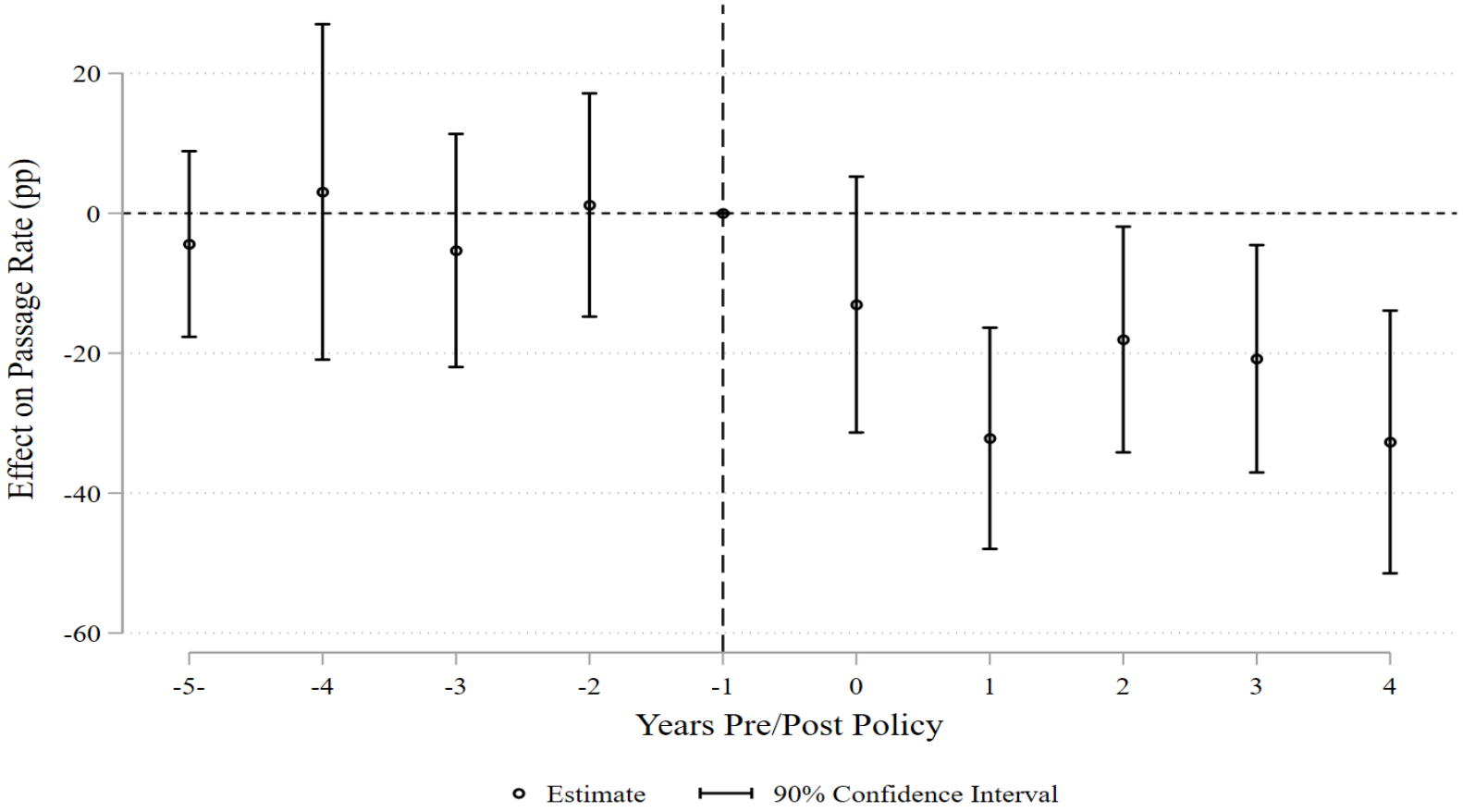
Figure 3
Bond Proposition Pass Rate by Year and Bond Type



Notes. This figure captures binned scatter plots of bond passage rate trends for school bonds and other municipal bonds over time in the U.S. State of Texas. The dashed line indicates the timing of the enactment of the 2019 property tax ballot language change that only impacted school bond measures.

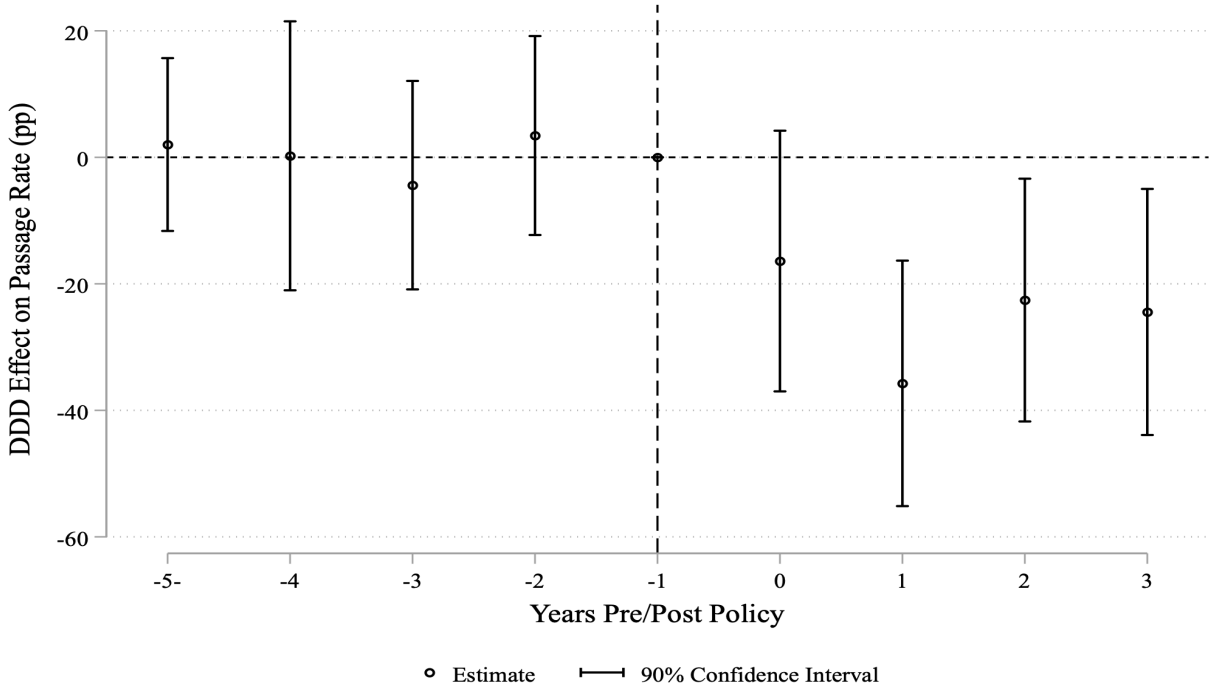
Figure 4

Event Study Estimates of the Effect of Property Tax Disclaimers on Bond Passage Rates



Notes. This figure presents event study-style effect estimates from a differences-in-differences model restricted to November bond elections in Texas. Coefficients represent dynamic differences-in-differences estimates relative to the year immediately preceding the 2019 change in property tax ballot language that impacted school bond measures. The model includes fixed effects for year and government type and clusters standard errors at the government level.

Figure 5
Event Study DDD Estimates of the Effect of Property Tax Disclaimers on Bond Passage Rates



Notes. This figure presents event study estimates from a differences-in-differences-in-differences (DDD) specification using data from November bond elections in Texas and North Carolina. Coefficients represent dynamic triple-difference estimates relative to the year immediately preceding the 2019 change in property tax ballot language that impacted school bond measures in Texas. The model includes school bond, Texas-by-school bond, school-bond-by-post, year, and Texas-by-year fixed effects. Standard errors are clustered at the government level.

Table 3

Sensitivity Checks for the Impact of Property Tax Ballot Language on Bond Passage Rates

Outcome (pp)	Preferred	Add Ln(Population)	Locale Type FE	Add all Covariates	Omit COVID	> 1 Vote Only	1997+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A. All Elections				
School Bond Pass Rate	-9.07**	-9.46**	-9.20**	-10.21***	-9.13**	-11.27***	-9.44***
	(3.02)	(3.03)	(3.05)	(3.02)	(3.08)	(3.31)	(2.68)
Observations	5,053	5,053	5,053	5,053	4,927	4,218	8,672
School Bond Vote Share	-7.54***	-7.62***	-5.96***	-7.71***	-7.79***	-12.98***	-10.64***
	(1.79)	(1.80)	(1.76)	(1.81)	(1.84)	(1.85)	(1.70)
Observations	5,053	5,053	5,053	5,053	4,927	4,218	8,672
			Panel B. November Only				
School Bond Pass Rate	-22.79***	-23.29***	-23.95***	-24.07***	-23.85***	-23.87***	-21.92***
	(4.23)	(4.25)	(4.26)	(4.20)	(4.40)	(4.51)	(3.89)
Observations	2,399	2,399	2,399	2,399	2,284	2,026	3,408
School Bond Vote Share	-12.46***	-12.69***	-11.08***	-12.77***	-13.56***	-16.41***	-14.83***
	(2.27)	(2.28)	(2.39)	(2.29)	(2.37)	(2.30)	(2.17)
Observations	2,399	2,399	2,399	2,399	2,284	2,026	3,408

Notes. Where property tax ballot language changes after 2019, all specifications include fixed effects for year and government type, and standard errors are clustered at the government level. Month fixed effects are also present in the specifications from Panel A. Covariates include all of the time-varying county-level covariates in Table 1. After 2019, covariates are linearly extrapolated based on the trends prior to the policy change. +=p<0.10 *p<0.05 **p<0.01 ***p<0.001

Table 4

Impact of Property Tax Ballot Language on Support for School Bonds
(Survey Experiments)

Outcome	CM (USA Sample) (1)	β (USA Sample) (2)	CM (TX Sample) (3)	β (TX Sample) (4)
A. Primary Confirmatory Outcomes				
Outcome: Would Support Proposed School Bond				
(T1 - Ambiguous Tax)	63.66%	-9.64**	85.09%	-11.95***
		(3.44)		(2.84)
(T2 - Small Tax)				-1.58
				(2.66)
(T3 - Large Tax)				-18.36***
				(3.00)
Observations		736		1,485
Outcome: Support Increasing School Spending				
(T1 - Ambiguous Tax)	51.10%	3.48	77.78%	-.93
		(3.39)		(3)
(T2 - Small Tax)				-1.13
				(3)
(T3 - Large Tax)				-5.71+
				(3.12)
Observations		729		1,482

Notes. Columns (1) and (3) present the control group means of the outcome listed in the previous row within the sample indicated in the column header. Columns (2) and (4) presents the estimated difference in the outcomes listed in each row between the treatment and control groups within the sample indicated in the column header, followed by its standard error in parentheses. All estimates include fixed effects for the size of the hypothetical bond that was evaluated by respondents and the covariates listed in Appendix Table A1. All standard errors are robust to heteroskedasticity. +=p<0.10 *=p<.05 **=p<.01 ***=p<.001.

Table 5

Other Factors Influenced by Property Tax Ballot Language

(Survey Experiments)

Outcome	CM (USA Sample) (1)	β (USA Sample) (2)	CM (TX Sample) (3)	β (TX Sample) (4)
A. Exploratory Outcomes: Mechanisms Related to Perceived Costs				
Outcome: Considered Property Taxes				
(T1 - Ambiguous Tax)	47.81%	14.07***	29.00%	16.24***
		(3.63)		(3.36)
(T2 - Small Tax)				28.63***
				(3.41)
(T3 - Large Tax)				29.21***
				(3.37)
Observations		737		1,485
Outcome: Considered Sales Taxes				
(T1 - Ambiguous Tax)	12.84%	-5.62*	8.67%	-3.66*
		(2.27)		(1.86)
(T2 - Small Tax)				-1.50
				(1.96)
(T3 - Large Tax)				-1.21
				(2.00)
Observations		737		1,485
Outcome: Expected Change in Housing Costs				
(T1 - Ambiguous Tax)	—	—	$43.03	4.48
				(6.43)
(T2 - Small Tax)				-18.71**
				(6.13)
(T3 - Large Tax)				7.43
				(6.41)
Observations				1,417
B. Mechanisms Related to Perceived Benefits or Need				
Outcome: Considered School Building Condition				
(T1 - Ambiguous Tax)	62.30%	-.87	75.61%	-11.78***
		(3.55)		(3.39)
(T2 - Small Tax)				-4.07
				(3.25)
(T3 - Large Tax)				-15.92***
				(3.38)
Observations		737		1,485
Outcome: Considered Technology in Schools				
(T1 - Ambiguous Tax)	46.99%	.32	64.5%	-11.44**
		(3.68)		(3.55)
(T2 - Small Tax)				-5.96+
				(3.58)
(T3 - Large Tax)				-18.28***
				(3.57)
Observations		737		1,485

Notes. Columns (1) and (3) present the control group means of the outcome listed in the previous row within the sample indicated in the column header. Columns (2) and (4) presents the estimated difference in the outcomes listed in each row between the treatment and control groups within the sample indicated in the column header, followed by its standard error in parentheses. All estimates include fixed effects for the size of the hypothetical bond that was evaluated by respondents and the covariates listed in Appendix Table A1. All standard errors are robust to heteroskedasticity. *=p<.05 **=p<.01 ***=p<.001.

Table 6

Impact of Property Tax Ballot Language by Political Affiliation

(Survey Experiments)

	USA Sample			TX Sample		
Outcome	Dem.	Rep.	Ind.	Dem.	Rep.	Ind.
	(1)	(2)	(3)	(4)	(5)	(6)
A. Primary Confirmatory Outcomes						
Outcome: Would Support Proposed School Bond (pp)						
(T1 - Ambiguous Tax)	-11.42*	-9.78	-9.00	-6.97+	-17.40*	-12.63*
	(5.37)	(7.37)	(6.88)	(3.70)	(8.08)	(4.97)
(T2 - Small Tax)				.005	4.65	-4.98
				(3.25)	(7.74)	(4.70)
(T3 - Large Tax)				-17.82***	-16.82*	-20.62***
				(4.48)	(8.12)	(4.93)
Observations	241	188	208	589	306	529
Outcome: Support Increasing School Spending (pp)						
(T1 - Ambiguous Tax)	3.16	-2.43	8.74	.63	-5.90	2.01
	(5.23)	(7.15)	(6.75)	(3.93)	(8.01)	(5.38)
(T2 - Small Tax)				5.00	-5.31	-3.47
				(3.76)	(8.27)	(5.57)
(T3 - Large Tax)				-3.45	-6.80	-8.10
				(4.38)	(8.36)	(5.39)
Observations	239	187	205	588	306	527

Notes. Each cell presents the estimated difference in the outcomes listed in each row between the treatment and control groups within the sample and political party indicated in the column header, followed by its standard error in parentheses. All estimates include fixed effects for the size of the hypothetical bond that was evaluated by respondents and the covariates listed in Appendix Table A1. All standard errors are robust to heteroskedasticity. +=p<0.10 *=p<.05 **=p<.01 ***=p<.001.

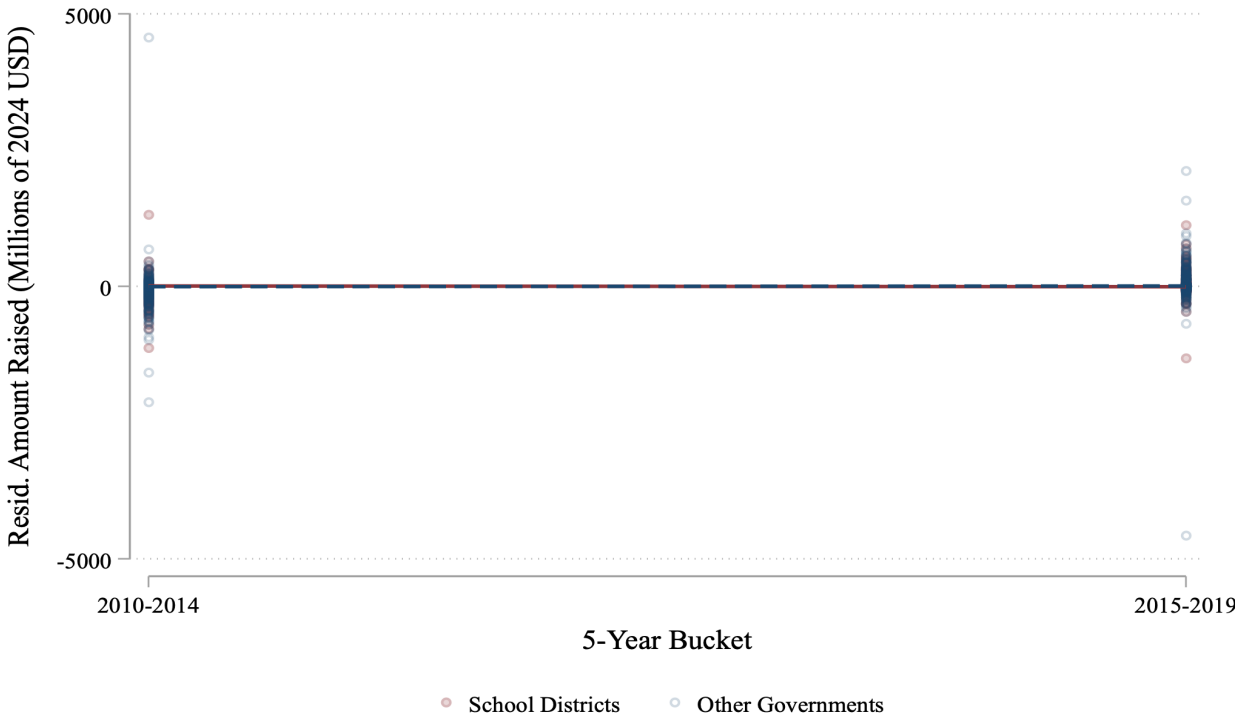
APPENDIX TABLES AND FIGURES

Appendix Table A1: Summary Statistics and Balance Test (Survey Experiments)

Characteristic	USA Sample Control (1)	USA Sample Treatment (2)	P-Value (1)=(2)	TX Sample Control (3)	TX Sample Treat1 (4)	TX Sample Treat 2 (5)	TX Sample Treat 3 (6)	P-Value (3)=(4)=(5)=(6)
A. Demographics								
Male	.665	.665	.910	.397	.416	.409	.368	.472
Female	.317	.326	.707	.579	.559	.556	.602	.489
Above Median Age	.506	.524	.677	.463	.531	.472	.532	.086
Parent of School-Aged Children	.137	.143	.830	.293	.272	.353	.273	.063
Non-White	.102	.123	.404	.308	.327	.286	.300	.579
B. Education and Income								
Highest Education = HS	.123	.155	.242	.417	.402	.350	.401	.277
Highest Education = AA/AS	.151	.091	.020	.092	.102	.113	.086	.602
Highest Education = BA/BS	.302	.334	.384	.352	.356	.394	.364	.678
Highest Education = MA/MS	.312	.293	.599	.095	.116	.105	.102	.804
Highest Education = PhD/JD/MD	.111	.114	.861	.030	.011	.030	.032	.068
Median Income or Below	.506	.521	.611	.539	.523	.544	.550	.838
C. Politics								
Democrat	.387	.369	.680	.460	.404	.421	.369	.082
Republican	.310	.280	.408	.209	.222	.219	.209	.967
No Major Party Affiliation	.304	.351	.222	.331	.374	.360	.422	.111
D. Housing								
Homeowner	.757	.775	.536	.464	.459	.533	.455	.103
Renter	.170	.165	.802	.415	.423	.352	.457	.034
Other Housing Situation	.073	.060	.500	.121	.118	.115	.088	.460
E. Missing Data								
Missing Gender Data	.112	.084	.186	.003	.003	.005	.005	.896
Missing Race Data	.115	.081	.117	.005	.011	.003	.011	.395
Missing Party Affiliation	.145	.124	.372	.041	.040	.040	.043	.996
Missing Housing Information	.213	.232	.543	.014	.019	.019	.029	.571
Observations	366	371	737	369	371	371	374	1,485

Notes. Columns (1) – (6) present group means of the variables listed in each row. P-value columns present the p-value from a test of whether the indicated means are statistically distinguishable from one another.

Appendix Figure A1: Pre-trends in Amount Raised Before 2019



Notes. This figure residualizes the 5-year binned amount raised in millions of 2024 $USD.

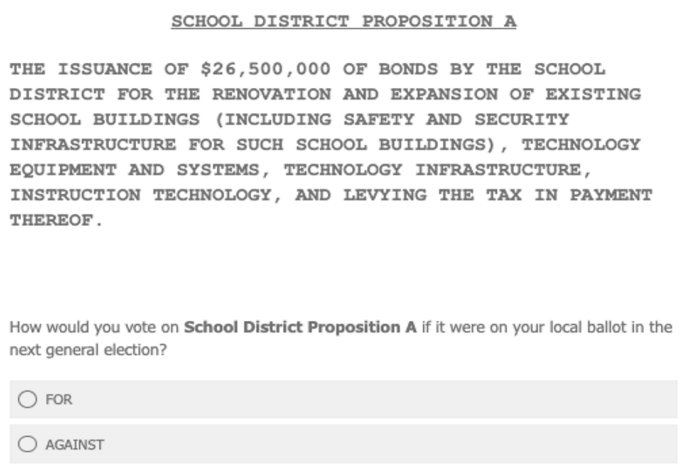
Appendix Table A2: Impact of Property Tax Ballot Language on Dollars Raised, 2010-2024 (DiD)

Outcome (Millions of 2024 USD)	All Elections	Off-Cycle Elections	November Elections
	(1)	(2)	(3)
Amount Raised	-61.079**	-53.390+	-69.350**
	(21.340)	(27.788)	(24.605)
Control Mean	109.261	74.471	129.600
Observations	5079	3402	3024

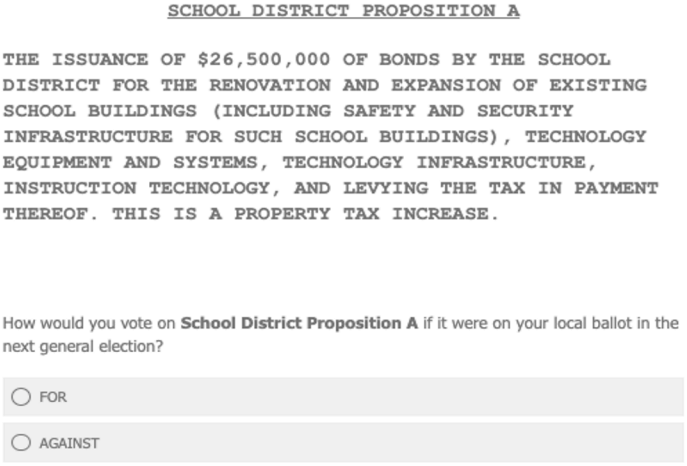
Notes. Funds are grouped into 5-year bins per local government. Property tax ballot language changes after 2019, all specifications include fixed effects for 5-year bins and government names, and standard errors are clustered at the government level. Columns (2) and (3) filter out elections that do not meet the specified timing scheme prior to collapsing the data into 5-year bins. The control mean reflects average dollars raised across the 5-year bins from all Texas school bond elections from 2010-2019. +=p<0.10 *p<0.05 **p<0.01 ***p<0.001.

Appendix Figure A2: Sample Bond Propositions
(Survey Experiment)

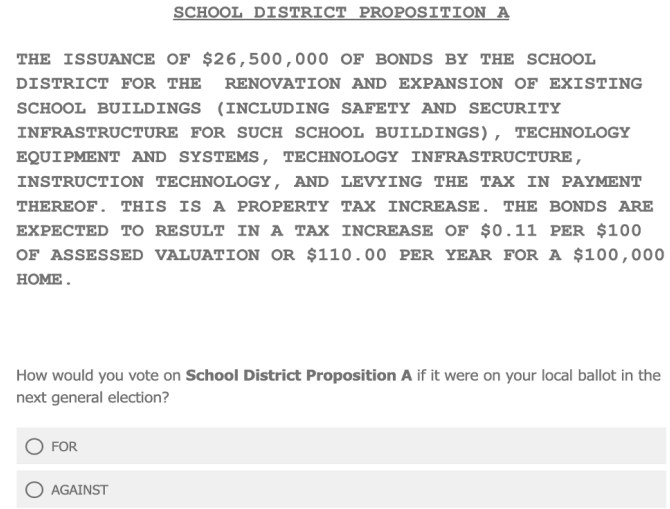
(A) Control Group



(B) Ambiguous Property Tax Disclaimer Treatment



(C) Large, Detailed Property Tax Disclaimer Treatment



Appendix Table A3: DiD Results by Tier of Urbanity

Outcome (pp)	All Counties	Least Urban (1st Tercile)	Moderately Urban (2nd Tercile)	Most Urban (3rd Tercile)
	(1)	(2)	(3)	(4)
	Panel A. All Elections			
School Bond Pass Rate	-9.072**	-6.102	-5.966	-17.691**
	(3.019)	(5.415)	(5.830)	(5.420)
Observations	5,053	1,728	1,756	1,550
School Bond Vote Share	-7.545***	-8.764**	-5.498+	-10.455**
	(1.793)	(3.115)	(3.071)	(3.425)
Observations	5,053	1,728	1,756	1,550
	Panel B. November Only			
School Bond Pass Rate	-22.788***	-19.411*	-19.761*	-37.662***
	(4.226)	(7.805)	(8.423)	(6.990)
Observations	2,399	856	825	713
School Bond Vote Share	-12.459***	-6.753	-11.287**	-23.577***
	(2.272)	(4.436)	(3.685)	(3.705)
Observations	2,399	856	825	713

Notes. Where property tax ballot language changes after 2019, all specifications include fixed effects for year and an indicator for a school bond election, and standard errors are clustered at the government level. Specifications in Panel A also include month fixed effects. +=p<0.10 *p<0.05 **p<0.01 ***p<0.001

Appendix Table A4: Alternative Samples for DiD and DDD Estimates

Outcome (pp)	Elections	2012-2024	2013-2024	2012-2023	2013-2023
		(1)	(2)	(3)	(4)
Panel A. DiD in Texas					
School Bond Pass Rate	All	-9.07**	-9.21**	-8.37*	-8.50*
		(3.02)	(3.07)	(3.31)	(3.36)
Observations		5053	4861	4506	4314
School Bond Vote Share	All	-7.54***	-7.14***	-6.08**	-5.68**
		(1.79)	(1.82)	(2.00)	(2.02)
Observations		5053	4861	4506	4314
School Bond Pass Rate	November	-22.79***	-23.37***	-21.17***	-21.75***
		(4.23)	(4.27)	(4.44)	(4.49)
Observations		2399	2308	2184	2093
School Bond Vote Share	November	-12.46***	-12.11***	-11.27***	-10.92***
		(2.27)	(2.3)	(2.45)	(2.47)
Observations		2399	2308	2184	2093
Panel B. DDD in Texas and North Carolina (2012-2023)					
School Bond Pass Rate	All	-20.22**	-20.4**	-12.29+	-12.44+
		(7.35)	(7.39)	(6.80)	(6.88)
		5305	5099	4727	4521
School Bond Vote Share	All	-11.66**	-11.08*	-8.94*	-8.36*
		(4.45)	(4.44)	(4.06)	(4.06)
		5305	5099	4727	4521
School Bond Pass Rate	November	-30.4***	-31.03***	-23.51**	-24.14**
		(8.00)	(8.06)	(7.73)	(7.85)
		2616	2512	2374	2270
School Bond Vote Share	November	-13.93**	-13.38**	-12.67*	-12.12*
		(5.09)	(5.09)	(4.99)	(5.01)
		2616	2512	2374	2270

Notes. Where property tax ballot language changes after 2019; all specifications include fixed effects for year, an indicator for a school bond election, and the interaction term of interest; and standard errors are clustered at the government level. Specifications in column (1) include month fixed effects. Specifications in Panel B include an indicator for Texas, a Texas-by-post interaction term, a Texas-by-bond-type interaction term, and the triple interaction term of interest. The control means reflect average values for each outcome from school bond elections prior to property tax ballot language changes. +=p<0.10 *p<0.05 **p<0.01 ***p<0.001

Appendix Table A5: Desensitization Analysis

	All Elections		November Only	
Outcome (pp)	(1)	(2)	(3)	(4)
School Bond Pass Rate				
Property Tax Ballot Language	-9.072**	-5.307+	-22.788***	-17.044***
	(3.019)	(3.184)	(4.226)	(4.702)
Property Tax Ballot Language*Later	—	-6.332*	—	-9.593*
		(2.518)		(4.370)
Observations	5,053	5,053	2,399	2,399
School Bond Vote Share				
Property Tax Ballot Language	-7.545***	-6.775***	-12.459***	-11.523***
	(1.793)	(1.839)	(2.272)	(2.324)
Property Tax Ballot Language*Later	—	-1.294+	—	-1.563+
		(0.693)		(0.911)
Observations	5,053	5,053	2,399	2,399

Notes. Where property tax ballot language changes after 2019, all specifications include fixed effects for year and an indicator for a school bond election, and standard errors are clustered at the government level. Specifications with sufficient variation also include month fixed effects, which are always present in the "All Elections" columns. Here, "Later" bonds are those measures classified as occuring two or more years after a school bond. +=p<0.10 *p<0.05 **p<0.01 ***p<0.001

Appendix Table A6: Ballot Language Spillover Effects on Non-School Bond Measures Within Same County

Outcome (pp)	All Counties	Drop Counties Without Any School Bonds
	(1)	(2)
Panel A. All Texas Municipal Bond Elections		
Non-School Bond Pass Rate	5.158	6.021
	(3.939)	(4.399)
Observations	2,735	2,576
Non-School Bond Vote Share	-0.084	0.262
	(3.532)	(3.940)
Observations	2,735	2,576
Panel B. November Only		
Non-School Bond Pass Rate	1.108	0.911
	(4.368)	(4.650)
Observations	1,476	1,394
Non-School Bond Vote Share	0.110	-0.015
	(3.596)	(3.554)
Observations	1,476	1,394

Notes. Where property tax ballot language changes after 2019, all specifications include fixed effects for year and an indicator for a school bond election, and standard errors are clustered at the government level. Month fixed effects are also present in the specifications from Panel A. All Elections. Here, we defines spillover effects as influences exerted by the ballot language change over pass rates and vote share for non-school bond measures that occur in the same county as a school bond. +=p<0.10 *p<0.05 **p<0.01 ***p<0.001.

Appendix Table A7: Alternative Specifications
(Survey Experiment, USA Sample)

Outcome	CM	All Covariates from Table A1	No Covariates	Ignore Attn. Check
	(1)	(2)	(3)	(4)
A. Primary Confirmatory Outcomes				
Would Support Proposed School Bond (pp)	63.66	-9.64**	-9.38**	-9.21*
		(3.44)	(3.62)	(3.59)
		736	736	748
Support Increasing School Spending (pp)	51.10	3.48	3.34	3.05
		(3.39)	(3.71)	(3.67)
		729	729	741
B. Exploratory Outcomes - Mechanisms Related to Perceived Costs				
Considered Property Taxes (pp)	47.81	14.07***	13.80***	14.41***
		(3.63)	(3.64)	(3.60)
		737	737	749
Considered Sales Taxes (pp)	12.84	-5.62*	-5.59*	-5.44*
		(2.27)	(2.22)	(2.18)
		737	737	749
C. Exploratory Outcomes - Mechanisms Related to Perceived Benefits or Needs				
Considered School Building Condition (pp)	62.30	-.87	-1.73	-2.21
		(3.55)	(3.60)	(3.56)
		737	737	749
Considered Technology in Schools (pp)	46.99	.32	-.39	-.17
		(3.68)	(3.69)	(3.66)
		737	737	749

Notes. Column (1) presents the control group means of the outcome listed in the previous row within the nationwide sample from Experiment 1. Columns (2) – (4) present the estimated difference in the outcomes listed in each row between the treatment and control group, estimated under the conditions described in the column header. All estimates include fixed effects for the size of the hypothetical bond that was evaluated by respondents. All standard errors are robust to heteroskedasticity. +=p<0.10 *=p<.05 **=p<.01 ***=p<.001.

Appendix Table A8: Alternative Specifications
(Survey Experiment, TX Sample)

Outcome	CM	All Covariates from Table A1	No Covariates	Ignore Attn. Check
	(1)	(2)	(3)	(4)
A. Primary Confirmatory Outcomes				
Outcome: Would Support Proposed School Bond (pp)				
(T1 - Ambiguous Tax)	85.09%	-11.95***	-13.08***	-12.72***
		(2.84)	(2.99)	(3)
(T2 - Small Tax)		-1.58	-2.37	-1.76
		(2.66)	(2.71)	(2.71)
(T3 - Large Tax)		-18.36***	-19.49***	-18.86***
		(3)	(3.09)	(3.1)
Observations		1,485	1,485	1,503
Outcome: Support Increasing School Spending (pp)				
(T1 - Ambiguous Tax)	77.78%	-.93	-2.36	-1.92
		(3)	(3.12)	(3.12)
(T2 - Small Tax)		-1.13	-2.04	-1.68
		(3)	(3.11)	(3.1)
(T3 - Large Tax)		-5.71+	-7.04*	-6.57*
		(3.12)	(3.21)	(3.2)
Observations		1,482	1,482	1,499
B. Exploratory Outcomes - Mechanisms Related to Perceived Costs				
Outcome: Considered Property Taxes (pp)				
(T1 - Ambiguous Tax)	29.00%	16.24***	17.49***	15.68***
		(3.36)	(3.52)	(3.35)
(T2 - Small Tax)		28.63***	30.39***	28.61***
		(3.41)	(3.49)	(3.39)
(T3 - Large Tax)		29.21***	30.85***	28.65***
		(3.37)	(3.49)	(3.36)
Observations		1,485	1,485	1,503
Outcome: Considered Sales Taxes (pp)				
(T1 - Ambiguous Tax)	8.67%	-3.66*	-3.56+	-2.96
		(1.86)	(1.87)	(1.88)
(T2 - Small Tax)		-1.50	-1.65	-1.08
		(1.96)	(1.99)	(1.95)
(T3 - Large Tax)		-1.21	-1.16	-1.02
		(2.00)	(2.02)	(1.99)
Observations		1,485	1,485	1,503
Outcome: Expected Change in Housing Costs				
(T1 - Ambiguous Tax)	$43.03	4.48	2.66	5.25
		(6.43)	(6.45)	(6.43)
(T2 - Small Tax)		-18.71**	-19.18**	-18.45**
		(6.13)	(6.10)	(6.08)
(T3 - Large Tax)		7.43	6.81	7.20
		(6.41)	(6.39)	(6.36)
Observations		1,417	1,417	1,433
C. Exploratory Outcomes - Mechanisms Related to Perceived Benefits or Needs				
Outcome: Considered School Building Condition (pp)				
(T1 - Ambiguous Tax)	75.61%	-11.78***	-13.14***	-11.49***
		(3.39)	(3.37)	(3.38)
(T2 - Small Tax)		-4.07	-4.36	-3.76

		(3.25)	(3.24)	(3.24)
(T3 - Large Tax)		-15.92***	-17.13***	-15.82***
		(3.38)	(3.40)	(3.38)
Observations		1,485	1,485	1,503
Outcome: Considered Technology in Schools (pp)				
(T1 - Ambiguous Tax)	64.50%	-11.44**	-12.23***	-11.65***
		(3.55)	(3.59)	(3.53)
(T2 - Small Tax)		-5.96+	-6.74+	-6.22+
		(3.58)	(3.57)	(3.55)
(T3 - Large Tax)		-18.28***	-19.61***	-18.07***
		(3.57)	(3.58)	(3.56)
Observations		1,485	1,485	1,503

Notes. Column (1) presents the control group means of the outcome listed in the previous row within the nationwide sample from Experiment 1. Columns (2) — (4) present the estimated difference in the outcomes listed in each row between the control group and the indicated treatment group, estimated under the conditions described in the column header. All estimates include fixed effects for the size of the hypothetical bond that was evaluated by respondents. All standard errors are robust to heteroskedasticity. +=p<0.10 *=p<.05 **=p<.01 ***=p<.001.

Appendix Table A9: Other Factors Impacted by Property Tax Ballot Language (Survey Experiments)

Outcome	CM (USA Sample) (1)	β (USA Sample) (2)	CM (TX Sample) (3)	β (TX Sample) (4)
A. Exploratory Outcomes (Other Considerations)				
Outcome: Considered Academic Achievement (pp)				
(T1 - Ambiguous Tax)	34.97%	-3.72	37.13%	-5.05
		(3.52)		(3.47)
(T2 - Small Tax)				-3.85
				(3.51)
(T3 - Large Tax)				-6.95*
				(3.48)
Observations		737		1,485
Outcome: Considered Current School Spending (pp)				
(T1 - Ambiguous Tax)	43.17%	-1.62	29.00%	-.07
		(3.64)		(3.36)
(T2 - Small Tax)				-6.93*
				(3.23)
(T3 - Large Tax)				-2.42
				(3.31)
Observations		737		1,485
Outcome: Considered Overall Government Spending (pp)				
(T1 - Ambiguous Tax)	24.59%	1.39	14.63%	1.59
		(3.07)		(2.65)
(T2 - Small Tax)				-1.50
				(2.54)
(T3 - Large Tax)				3.36
				(2.72)
Observations		737		1,485
Outcome: Considered Inflation (pp)				
(T1 - Ambiguous Tax)	9.29%	.91	9.76%	3.87
		(2.14)		(2.37)
(T2 - Small Tax)				2.83
				(2.33)
(T3 - Large Tax)				6.97**
				(2.50)
Observations		737		1,485
Outcome: Considered Local Education Leadership (pp)				
(T1 - Ambiguous Tax)	27.05%	2.42	16.53%	-2.67
		(3.35)		(2.64)
(T2 - Small Tax)				4.48
				(2.88)
(T3 - Large Tax)				.39
				(2.73)
Observations		737		1,485
Outcome: Considered State Education Leadership (pp)				
(T1 - Ambiguous Tax)	17.49%	-1.45	13.28%	4.53+

		(2.79)		(2.65)
(T2 - Small Tax)				6.70*
				(2.71)
(T3 - Large Tax)				3.62
				(2.55)
Observations		737		1,485
Outcome: Considered Teacher Salaries (pp)				
(T1 - Ambiguous Tax)	23.50%	-4.41	26.02%	-4.62
		(3.05)		(3.11)
(T2 - Small Tax)				.27
				(3.18)
(T3 - Large Tax)				-4.82
				(3.09)
Observations		737		1,485
Outcome: Considered Something Else (pp)				
(T1 - Ambiguous Tax)	18.31%	-1.25	7.59%	.78
		(2.86)		(2.02)
(T2 - Small Tax)				-1.01
				(1.87)
(T3 - Large Tax)				.41
				(1.99)
Observations		737		1,485

Notes. Columns (1) and (3) present the control group means of the outcome listed in the previous row within the sample indicated in the column header. Columns (2) and (4) presents the estimated difference in the outcomes listed in each row between the treatment and control groups within the sample indicated in the column header, followed by its standard error in parentheses. All estimates include fixed effects for the size of the hypothetical bond that was evaluated by respondents and the covariates listed in Appendix Table A1. All standard errors are robust to heteroskedasticity. +=p<.10 *=p<.05 **=p<.01 ***=p<.001.