The Impact of Capping the SALT Deduction on Municipal Bond Pricing

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ABSTRACT

We examine the impact of the Tax Cuts and Jobs Act's (TCJA, also known as the "Trump tax cuts") cap on the state and local tax (SALT) deduction on the relationship between municipal bond spreads and real estate tax revenues by examining California counties' financial and municipal bond data from 2016 to 2019. Consistent with the SALT deduction limitation negatively affecting counties' expected future ability to raise more real estate tax revenues, we find that after the passage of TCJA, this relationship weakens. We further find that this impact is more pronounced for counties where taxpayers are less likely to support future tax increases and for counties that are less financially constrained and thus are less reliant on future real estate tax increases for their financing. Using California's voting data, we find that its voters are less likely to support tax ballot proposals in counties with higher extant real estate tax revenues in the post-TCJA period. Overall, our evidence indicates that, in the post-TCJA period, real estate tax revenue becomes a less important determinant of municipal bond spreads—likely because of voters' unwillingness to continue supporting future real estate tax increases due to voters' expected inability to derive tax deduction benefits from additional local tax payments.

Keywords: municipal bonds, Trump tax cuts, SALT deduction, real estate taxes

1. Introduction

We examine how the municipal bond prices were impacted by the capped taxpayer state and local tax (SALT) deduction passed under the 2017 Tax Cuts and Jobs Act of 2017 (TCJA)—informally called the "Trump tax cuts". A key provision of the TCJA, which is set to expire in 2025, caps the extent of the SALT deduction to $10,000 for married-filing-jointly taxpayers ($5,000 for married individuals filing separately) who itemize their deductions (Franck, 2021). The cap was a major controversy at the time of its enaction and faced much debate afterwards. However, to our knowledge, there is scant academic research yet to assess its economic impact.

Policy analysts and journalists have identified several forms of economic impact by the SALT deduction cap. Many view this cap to primarily benefit wealthier taxpayers from high local income and property tax rate jurisdictions (National Taxpayers Union, 2021).[1] Thus the perceived biggest, resultant "losers" are higher net worth taxpayers living in higher-income tax states, such as California, New York, New Jersey, and Massachusetts.[2] The SALT deduction limitation effectively raised taxes on individuals living in these locations, leading many of their federal legislators to try to repeal or significantly raise the SALT cap (Franck, 2021).[3] Marr, Bryant, and Leachman (2019) claim that the SALT cap could hinder some states and localities from raising revenues from progressive tax sources if it reduces overall support for state and local tax increases.

Many dynamic factors make assessing the Act's economic impact difficult to assess. First,

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[1]Per the Tax Policy Center, 57% of the benefits of eliminating the SALT deduction cap would go to the top 1% of filers. The TCJA also provides the top 1% of taxpayers an average tax cut of more than $35,000 (vs. just $37 for middle-class taxpayers) (McGurn, 2021).

[2] Since passage of the bill, some states have passed SALT deduction "workarounds" to allow the deduction of state and local taxes on these states' business tax returns as business expenses by pass-through entities. In California, the workaround is effective for 2021-2025, which does not affect our results since our sample period ends in 2019 (see, e.g., Kathcart, 2022).

[3] The cap is set to expire in 2025. But many U.S. Congressional Republicans oppose removing the cap (see, e.g., Murakami, 2022), and many U.S. Republican Senators have sought to have sought to extend the cap (Hussey, 2022).

lost TCJA deductions may cause wealthier taxpayers to move to Florida, Texas, and other non-income tax states (Hageman Robb, and Schwebke 2021). Second, facing the SALT deduction cap, some states and localities have greatly increased high-income residents' tax rates to support better schools and other capital needs. For example, since 2017, New Jersey has raised income taxes for households with incomes over $5 million, New York has extended an existing millionaires' tax, and Connecticut, New York, and Washington have adopted or expanded a real estate transfer tax on high-value homes (often called a "mansion tax"). Moreover, to avoid the cap, at least 27 states have recently enacted laws to allow law firms, hedge funds, car dealerships, and other closely held businesses to reclassify capped personal income taxes into uncapped business taxes, deriving more than $10 billion in lost federal income taxes (Rubin, 2022).

In this study, we assess the economic impact of the SALT deduction cap using forward-looking financial market data. Specifically, we focus on the impact on the public financing cost of local governments. We measure such financing costs by the yields on municipal bonds that reflect market participants' anticipation of how taxpayers and local governments will react to the SALT deduction cap in the future.

We posit that the SALT deduction limitation should make taxpayers who have reached the cap less likely to support property tax renewals or increases,[4] which are an important source of local government revenues.[5] This unwillingness to pay additional property taxes, in turn, has two implications for local governments' cost of debt financing. First, it makes l, ocal governments'

[4] Evidence indicating such lack of willingness to increase taxes is already emerging. For example, in 2018, voters refused to approve infrastructure-related income tax increases in Colorado (Stein, 2018); California voters also refused to approve removing caps on property taxes (Spagat, 2020).

[5] In addition, Urban Institute reports that in 2017, intergovernmental transfers constituted 36% of the local government revenue and property taxes constituted 30%. The rest consisted of charges and miscellaneous revenue (23%), sales taxes (7%), and other types of taxes (4%) (Tax Policy Center, 2017)

ability to raise property taxes more uncertain. Second, the potential loss of property tax revenues may force local governments to seek other sources of funds to settle their debt obligations. We thus expect that the pricing of municipal bonds, as measured by the bonds' yields, will depend less on the current level of local real estate tax revenues and more on such sources as state revenue sharing and local sales taxes. We expect this effect to be particularly more pronounced for municipalities where residents are more resistant to tax increases.

Using California counties' data on municipal bonds, we find evidence consistent with this prediction. In the year after the TCJA (2019) passage, municipal bond yields in the secondary market became substantially less sensitive to county-level property tax revenues than in the period before and immediately after the tax cut (2016–2018).[6] We show that in the year after the tax cut, a one-percentage point increase in counties' per-capita property tax revenues is associated with a 13-basis-point increase in yield spreads relative to the period before the tax change. Given our sample's median issuance size of $124 million and a median maturity of 6.4 years, this 13-basis-point increase identifies about $1.03 million of additional borrowing costs per issue over the typical maturity period, for high-property-tax counties relative to low-property-tax counties. Thus, the reduced sensitivity materially affects counties' borrowing costs.

To further test the heterogeneous effects of the SALT deduction cap on municipal yields, we construct several proxies for local residents' willingness to support local tax increases based on their wealth, education, age, or party affiliation. We find that the reduction in the sensitivity of muni yields to local property tax revenues is greater for counties with local residents who will less

[6] As we discuss subsequently, we include 2018 in the pre-tax cut period because we argue that it took at least a year for the consequences of the SALT deduction elimination to be fully impounded into the bond returns. This is because (1) the tax cut was passed late in 2017; (2) most municipalities have a June 30 year-end, and municipal financial statements are typically filed much later, usually around 6 months or so later; and (3) taxpayers would not feel the "pinch" of the SALT deduction limitation until they actually filed their taxes in 2018.

willingly support tax increases.

We also provide more direct evidence that the TCJA significantly affects local residents' support for taxes. Using county-level data on ballots and election outcomes, we find that in general, both the likelihood of a (1) property tax proposal appearing on a local election ballot and (2) ballot passing are negatively related to local property tax revenue. The TCJA makes this relationship significantly more negative. That is, post-TCJA, increasing property taxes further is more difficult for counties with high property tax revenues. Combined, these findings support the hypothesized effect of the increased reluctance of local residents in high tax municipalities to support tax increases, which reduces the sensitivity of muni debt pricing to local property tax revenues.

We contribute to several streams of literature. First, our study provides evidence of the impact of TCJA on public financing costs proxied by municipal bond yields. To our knowledge, extant municipal bond literature has not examined the impact of municipalities' revenue structure on municipal bond prices. Such an examination is important, given recent anecdotal reports of municipal yields not showing the expected associations with the underlying economic fundamentals.[7] Second, we expand the literature on the impact of changes to the tax law on financial markets (e.g., Cutler, 1988; Michaely, 1991; Shackelford, 2000). We thus shed greater light on municipal bond pricing.

2. Background and Hypotheses Development

2.1 Expected Effects of SALT Deduction Limitation on Local Government Finances

States and municipalities rely on local property, sales, and income taxes as primary

[7] For example, Banerji (2019) reported that "[b]ond-ratings firms are struggling to judge the creditworthiness of cities and local governments with deep financial problems. There have been widely disparate ratings, errors in analysis and a fight for market share that may have produced optimistic outlooks."

financing sources, as well as other important sources such as fees and state and federal government transfers (i.e., grants).[8] According to the Government Financial Officer Association (2017), all 50 states use some sort of property taxes as a source of revenue; they then typically pass such revenues on to municipalities. Municipalities' ability to raise property tax revenue is thus a key measure of their financial health—especially for those with no other significant revenue sources. Consistent with this view, prior research demonstrates that a greater reliance on property taxes is associated with a higher probability of municipal bond ratings downgrades (Copeland and Ingram, 1982) and that the overall ratio of municipalities' indebtedness to total property value explains municipal bond yield premiums (Liu and Seyyed, 1991).

State and local governments generally must attain taxpayer approval to renew or increase property taxes. Kim, Plumlee, and Stubben (2022, p. 129) argue that because the government ultimately controls tax revenue, "[c]itizens are required to pay taxes whether or not they directly benefit from the services they fund." Thus, unlike a typical business exchange, citizens cannot simply reject the governmental services they do not like by refusing to pay taxes. Because taxes are involuntary, dissatisfied taxpayers' only immediate potential recourse is the costly option of relocating (Kim et al., 2022). Gore (2009) shows that municipalities with more limited sources of financing must retain more cash than financially stronger ones.

Citizens' only other recourse is to vote against property tax increases. For example, in 1978, Californians approved Proposition 13, which limited property tax increases, cut half of the 1979 property tax revenues, and put many municipalities into financial distress (California State Board of Equalization, 2018). In 2020, voters rejected Proposal 15 to partially dismantle the state's

[8]As Kim et al. (2022, p. 130) report, "In 2017 … state government revenues were comprised primarily of taxes ($943 billion) and transfers from the federal government ($639 billion)."

42-year-old cap on property taxes. This proposition would have allowed local governments to reassess commercial and industrial properties every three years, with residential properties and home-based businesses remaining under the 1978 rules. The change would have generated up to $12.5 billion in revenue (Spagat, 2020). A similar phenomenon occurred in Massachusetts (Cutler et a l., 1999; Bradbury et al., 2001).

Gale Gelfond, Krupkin, Mazur, and Toder (2018) and Hemel (2018) note that limiting SALT deductions should raise higher-income residents' net after-tax price to fund state and local government services. Such caps most heavily affect those who must disproportionately support sustained or increased levels of state spending. Any adverse effects will likely be regressive, as most sub-national government spending goes to items such as education, health, and income support mainly for low- and moderate-income households (Lav and Leachman, 2017). Consistent with these expectations on the passage of the TCJA, recent research shows that the SALT cap adversely affected homeowners in the highest real estate tax areas, lowering growth in home prices and reduced construction (Li and Yu, 2020).

Voters' willingness to approve or renew property tax increases depends primarily on such perceived cost-benefit trade-offs. Consistent with the existence of this trade-off, Feldstein and Metcalf (1987) show that federal tax deductibility affects state and local governmental spending policies. Thus, a key determinant of property tax costs is their federal tax deductibility (i.e., the SALT deduction). Until 2017, U.S. taxpayers who itemized their deductions could fully deduct such costs on their federal tax returns, which reduced their federal income tax burden—especially for residents of states and municipalities with higher levels of local taxation.

Critics of the SALT federal deductibility claim that this deduction disproportionately benefits higher-income taxpayers who can meet the itemized deduction threshold by combining mortgage interest, SALT, and charitable contribution deductions that typically form the most significant components of itemized deductions.[9] Consistent with this argument, Altig, Auerbach, Higgins, Koehler, Kotlikoff, Terry, and Ye (2020) show that in 2017, $99.7 of $109 billion of the total value of SALT deductions accrued to taxpayers with incomes exceeding $100,000. Also in 2017, approximately 42 million taxpayers received an average $2,590.56 in tax savings, which in 2018 fell to roughly 16 million taxpayers saving $1,220.57. In addition, Tax Foundation reported at the time that the U.S. Congress' Joint Committee on Taxation expected that the number of taxpayers who itemize their deductions would fall from 46 million in 2017 to 18 million in 2018—a decline exceeding 60%[10]. Thus, Congressional Democrats have tried to repeal the SALT deduction cap, attempts that as of this writing have not been successful.[11]

Furthermore, Barker (2019) claims that the SALT deduction has essentially allowed states to shift the cost of increased local taxation to the federal government. The SALT deduction is a key indirect federal subsidy to state taxation; its loss will likely lower state and local funding. This could well harm economic development and increase tax competition among state and local governments, thereby likely further increasing their financing constraints.

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[9]To itemize deductions, taxpayers' total itemized deductions must exceed the amount of the standard deduction. Ceteris paribus, higher SALT taxes improve the level of excess of itemized deductions over standard deduction amounts. Watson's (2021) detailed analysis examines who benefits from the 2017 changes in the SALT deduction.

[10] See https://taxfoundation.org/itemized-deduction-benefit/#:~:text=The%20Joint%20Committee%20on%20Taxation,flow%20to%20higher%2Dincome%20taxpayers. Accessed on November 8, 2022.

[11]On November 19, 2021, the U.S. House of Representatives passed the Build Back Better legislation, which, among other provisions, caps the annual SALT tax deduction from $10,000 for joint returns and $5,000 for married taxpayers filing separately to $80,000 and $40,000 (Rappeport and McGeehan, 2021). While these balances are still in flux, Republicans claim that this change will benefit higher-income taxpayers, thus penalizing "hardworking families to reward liberal elites." The Urban-Brookings Tax Policy Center similarly stresses that those who earn more than $1 million per year will receive about two-thirds of the tax savings of this proposed increase SALT caps. Moreover, regardless of perceived equity issues, many taxpayers in high property tax states maintained that they could not afford to lose these income tax deductions.

2.2 Expected Effect of the SALT Cap on Municipal Bond Yields

Municipal bond issuance provides a major source of state and local government external financing by relying on such funds to finance both their day-to-day operations and larger construction capital projects. To spur investors' demand for municipal bonds, the federal government exempts municipal bond interest from federal taxation (Driessen, 2018). Many states also exempt municipal bond interest from state taxation (Pirinsky and Wang, 2011; Schwert, 2017; Babina et al., 2021). These exemptions have spurred investors in high tax brackets to buy municipal bonds, especially given the recent low interest rates on other types of debt investments and infrequent municipal bond defaults (McGurn, 2021; Cohick, 2021). Municipal bond defaults do occur (Gorina et al., 2018; Gao et al., 2019), and prior research shows that municipal bond ratings and corresponding yields are affected by the issuing municipalities' financial condition, as reflected in municipalities' financial statement variables, such as overall revenue and debt levels (Raman, 1981; Palumbo and Zaporowski, 2012; Gorina et al., 2018), and in their macro- and socio-economic indicators (e.g., Wescott, 1984; Loviscek and Crowley, 1990; Liu and Seyyed, 1991; Gao et al., 2020; Cheng, 2021).

Anecdotal evidence suggests that recent demands for municipal bonds is high, even for distressed municipalities (McGurn, 2021)—a demand that has grown steadily from 2017 to 2020 (Gillers, 2020). Yet credit rating agencies have concurrently faced difficulties in rating municipal bonds, given the wide variability in muni ratings, erroneous analyses, and excessively optimistic ratings (Banerji, 2019). This difficulty makes it unclear whether municipal bond yields reflect the default risk driven by their economic fundamentals. In particular, while prior research shows that bond yields are negatively associated with overall municipal revenues (Raman, 1981; Palumbo and Zaporowski, 2012, Gorina et al., 2018), how changes in expected revenue composition would affect bond yields is unclear. As stated previously, the limited SALT deduction under the TCJA would likely impair citizens' willingness to increase real estate taxes and thus limit local governments' ability to use the related tax revenues to fund municipal bonds' interest and principal payments. This, in turn, forces municipalities to increasingly rely on voters' willingness to approve tax increases. Thus, with the passage of the TCJA, municipal bond yields should be less affected by extant real estate tax revenue. Stated formally:

Hypothesis 1: The negative impact of municipal real estate tax revenue on municipal bond yields is weaker after the passage of TCJA.

The impact of the TCJA's SALT deduction limitation will likely depend on counties' reliance on real estate tax revenue. To the extent that they have higher total revenues to finance their operations—derived either from their own or intergovernmental transferred revenue sources—their expected ability to raise future real estate taxes should be less important to their municipal bonds' default risk. We thus expect that for less financially constrained counties, after passage of the TCJA, real estate tax revenue should become less important as a determinant of bond yields particularly for counties with alternative revenue sources. Furthermore, counties with higher home values already derive substantial real estate tax revenues from their current tax assessments, and thus, on the margin, their future ability to raise real estate taxes is less important. This is compounded by the likelihood of taxpayers with higher home values to itemize deductions, as they can deduct a greater amount of interest expense on their tax returns (Glaeser and Shapiro, 2003; Di et al., 2007). Wealthier citizens also tend to be more economically conservative and less likely to support tax increases in general (Page et al., 2013). Moreover, such taxpayers will more likely send their children to private schools and thus will less likely to support added real estate tax assessments that support public schooling and other related municipal services from which they are unlikely to derive benefits.[12] Thus, taxpayers owning more expensive homes should be less likely to support increased real estate tax assessments post-TCJA. Stated formally:

Hypothesis 2: The association between municipalities' real estate tax revenues and municipal bond yields is weaker for municipalities with higher sources of alternative revenues and a stronger presence of wealthier homeowners.

Besides the SALT deduction cap's impact on municipalities' future ability to levy and collect increased real estate taxes, taxpayers must approve referendums to increase real estate taxes. Voting rates are not demographically uniform, and older citizens, who tend to vote the most often (Brandon, 2020), tend to benefit less from municipal spending on public secondary schools that are funded through real estate tax assessments, as they tend not to have school-age children. Consistently, research using Swiss data shows that older citizens prefer to not fund education and would rather allocate funds to health and social welfare security programs, which are not typically funded through real estate taxes (Cattaneo and Wolter, 2009). Furthermore, municipal bond yield spreads are higher for states with stronger presence of older citizens, which, among other factors, is consistent with older taxpayers' unwillingness and inability to support higher tax revenues (Butler and Yi, 2022). Thus, we expect older taxpayers to be less likely to support future real estate

[12] Private school tuition is generally not tax deductible, and families that pay private school tuition often complain that they do not benefit from their real estate tax assessments (Boston, 2014).

tax levies after TCJA. Stated formally:

Hypothesis 3: The association between municipalities' real estate tax revenues and municipal bond yields is weaker for municipalities with a stronger presence of older taxpayers.

Last, we expect taxpayers' political orientation and overall wealth to affect municipalities' ability to levy future real estate taxes. Research suggests that, in general, voters penalize incumbents for tax increases (Niemi et al., 1995), dislike property tax increases, and penalize the incumbent political party for local tax increases (Sances, 2017). More conservative voters are less likely to support (1) tax increases (e.g., Connor, 2018; Austermuhle, 2020)[13] and (2) local municipal spending, as reflected in spending patterns of Republican-controlled municipalities (De Benedictis-Kessner and Warshaw, 2016). More conservative voters are also likely to be wealthier (Zernike and Thee-Brenan, 2010), and higher-income Republicans tend to believe they pay too much in taxes (Pew Research Center, 2019). U.S. political polarization increased during the Trump presidency, and conservatives are more likely to be suspicious of public schools' curricula and move their children to private schools.[14] Considering how Republican representatives have voted in Congress on the Biden Infrastructure Investment and Jobs Act, they are less likely to support additional infrastructure spending (Montanaro, 2021). Overall, these arguments suggest that municipalities with a more conservative population are less likely to support future real estate tax increases in the post-TCJA period. Stated formally:

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[13]For example, the Texas Republican Party states, "Replace the property tax system with an alternative other than the income tax and require voter approval to increase the overall tax burden" (see https://texasgop.org/replace-property-tax/; accessed on November 25, 2021).

[14]A recent manifestation of this is the debate of Critical Race Theory's role in public schools (see, e.g., Anderson, 2021).

Hypothesis 4: The association between municipalities' real estate tax revenues and municipal bond yields is weaker for municipalities with a stronger presence of conservative (Republican) voters and higher-income voters.

3. Data and Research Design

3.1 California Bond Issues

We focus on how the TCJA cap on SALT deductions affects municipal borrowing costs. We argue that if the SALT deduction cap reduces the certainty of real estate tax revenues as a result of voters' reluctance to support property tax renewals, municipal yield spreads should become less sensitive to property taxes afterward. We use California's counties as a setting to test all our hypotheses, because, in addition to having 11.9% of the U.S. population, its residents' socio-economic demographics also vary considerably across counties. California's uniform ballot laws allow us to hold voters' institutional structures constant and examine whether variation in local governments' political economy affects municipal borrowing costs. To our knowledge, our study is the first to examine whether voters' opposition to property taxes can be priced in the municipal bond market. In this regard, we provide evidence that voting behavior is an important channel through which local politics can influence municipal bond pricing.

Another advantage of our approach is that the municipal bonds we study will likely have the same marginal investors, as municipal bond markets are locally concentrated (Cestau, 2019). For example, California provides residents with tax privileges for holding state-issued muni bonds, which Babina et al. (2021) show can lead to home bias in the municipal bond market. Because out-of-state offerings do not receive the same tax advantage as in-state offerings, many California bondholders would find it difficult to diversify their exposure to political uncertainty within the state. In this example, economic theory predicts that the bondholders would demand lower prices (i.e., higher yields) to hold California offerings as compensation for their exposure to statewide political uncertainty (see Pástor and Veronesi, 2013; Gao et al., 2019). Consistent with this economic intuition, Gao et al. (2019) use gubernatorial election cycles to show that municipal yields are higher during periods of political uncertainty. Thus, in our setting, the county-specific political uncertainty of real estate taxes should be priced within the state's municipal bond market. That is, the home bias in municipal bond market helps us identify the pricing effect of the SALT deduction limitation.

3.2 Data

We use the FTSE/Russell Municipal Bond database (formerly called Mergent Muni Securities database) to identify the muni bonds and their issuing characteristics. To ensure a relatively homogeneous relationship between bond yield and local property tax revenues, we include only plain-vanilla tax-exempt bonds (both general obligation bonds and revenue bonds) issued by California county-level governments. In other words, we exclude taxable muni bonds, bonds with put and call features, and bonds issued by non-county local government entities, such as school districts and local public utilities (as the budget of these entities may be separate from the county-level budget we examine). From the FTSE/Russell database, we obtain bond issuing characteristics, such as maturity date and issue size. We also obtain the historical records of the bonds' credit ratings by all three major credit rating agencies: Moody's, S&P, and Fitch. To facilitate statistical analysis, we convert the letter credit ratings into numeric credit rating scores according to the schedule provided in Appendix A.

We obtained county-level financial statement data from Bloomberg which collects this data from county comprehensive annual financial reports (CAFRs). We obtained key variables from this dataset, including general fund property tax revenues (or government fund property tax revenues per Bloomberg as an alternative measure), total revenues, total liabilities, local sales tax revenues, and intergovernmental transfer; the latter four measures derived from Bloomberg's government fund data.[15]

We obtain further county-level non-financial data from the U.S. Census Bureau, whose characteristics include demographics such as population size, residents' age, and education. We record median home values for each California county from the Census Bureau's American Community Value Survey[16] and obtain county-level election data from MIT Election Lab.

Finally, we obtain the secondary-market muni trade data from Municipal Securities Rulemaking Board (MSRB) data, accessed via Wharton Research Data Services. From the MSRB data, we obtain information on the date of trade, trade size, and traded yield.

We combine the data from these different sources for the period 2016–2019. We treat the three years before the Trump tax cuts (i.e., 2016–2018) as the pre-treatment period (POST=0) and 2019 as the post-treatment period (POST=1). Although the SALT deduction limitation became effective near the end of 2017, we treat 2018 as a pre-tax-cut period because we expect that it takes time for users of government financial statements to fully consider the implications of the SALT deduction limitations in their decision-making. This is because (1) the tax law change was passed by Congress almost at the end of 2017; (2) taxpayers would take at least two–three months to fully realize the impact of SALT deduction limitation, as they filed their 2017 taxes during early 2018;

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[15]Bloomberg organizes the CAFRs data into three types of accounts: general funds, government funds, and total government funds.

[16]https://www.census.gov/programs-surveys/acs

and (3) counties typically report their financial results as of June 30 year-end, and these financial statements typically do not become available for several months afterward. Thus, we consider 2019 the year SALT deduction limitation effects can be more clearly observed. Our analysis excludes post-2019 data to avoid the confounding effect of the COVID-19 pandemic, which disrupted the muni market in 2020. This also helps avoid potential confounding effects of passing 2021 SALT deduction workarounds in California.

When relating county-level financial and non-financial characteristics to muni yields, we consider the data's reporting time lags. County fiscal years typically end in June, and we assume a six-month lag for the CAFRs to be publicly available. Thus, we use the county-level financial statement data for the fiscal year ending in June of year t-1 to explain the muni yields in year t. We assume a one-year lag for the availability of the non-financial data, such as population, age, and education. The data obtained from the financial market—including bond yields, maturity, issue size, trade size, and credit rating—are instantaneously available.

To estimate the yield spreads of municipal bonds relative to the Treasury bonds, we obtain the daily zero-coupon Treasury yield curve data from the Federal Reserve, as maintained by Gürkaynak et al. (2007).[17] We calculate the yield spread on a municipal bond in the following way. First, we obtain the traded yield, y_t, on the bond from MSRB. Second, from the Treasury yield curve and the coupons and par value of the muni bond, we calculate the hypothetical price of a Treasury bond with the same cash flows:

$$P_t^* = \sum_{i=1}^{N} \frac{C_i}{\left(1 + r_{i,t}\right)^i} + \frac{\text{Par}}{\left(1 + r_{N,t}\right)^N},$$

where C_i is the coupon with maturity i, Par is the par (face) value of the bond with maturity N, and

[17] https://www.federalreserve.gov/data/nominal-yield-curve.htm.

$r_{i,t}$ is the date-t zero-coupon Treasury rate for maturity i. Given the hypothetical price of this Treasury bond, we further back out its yield to maturity y_t^* from the following pricing equation:

$$P_t^* = \sum_{i=1}^{N} \frac{C_i}{(1 + y_t^*)^i} + \frac{\text{Par}}{(1 + y_t^*)^N}.$$

Finally, the muni yield spread is[18]:

$$s_t = y_t - y_t^*.$$

Panel A of Table 1 summarizes our sample selection procedure. We begin with all municipal bonds in the Mergent Fixed Income Securities Database (FISD) that California entities issue, with an issuer name containing a California county name, an issuing date no later than 2018, and a maturity no earlier than 2017 (thus having at least one year of maturity during the period from 2016 to 2019). We have 40,619 unique bonds that satisfy this selection criteria. We then restrict the sample to tax-exempt bonds with no embedded options (i.e., callable or puttable), with either fixed coupon rate or zero coupon. This restriction results in 11,218 remaining bonds. From this sample, we further exclude bonds issued by school districts and local public utilities, yielding 4,049 unique bonds.

Next, we combine this bond list obtained from FISD with the MSRB bond trade data to include all trades with par values of at least $10,000 during the period 2016–2019. Our analysis is at the bond-month level. For bonds with multiple trades during a calendar month, we keep only the last valid trade of the month. For the 4,049 bonds in our FISD sample, 2,703 had at least one month of valid trading during the period. Altogether, we have 20,248 bond-month trade

[18]Alternatively, the muni spread can be calculated on the basis of tax-adjusted yields, as the muni and Treasury bonds are subject to different tax rates. However, since we focus only on California muni bonds, which face homogeneous federal and state personal income tax rates, we do not adjust for tax.

observations.[19] Panel B of Table 1 reports the annual breakdown of our sample, with all four years represented in our sample approximately evenly.

3.3 Research Design

We use panel regressions to test Hypothesis 1 that the SALT deduction cap enacted by the TCJA reduces the sensitivity of municipal borrowing costs to property tax revenues. To test the effect of the 2018 tax change on municipal bond yield spreads, we estimate the following regression model:

$$SPREAD_{i,t} = a_0 + \beta_1 PROPTAX_{i,t} + \beta_2 PROPTAX_{i,t}{*}Post + \beta_3 Post + \beta_i CONTROLS + c_i + \varepsilon_i, \quad (1)$$

where SPREAD$_{i,t}$ is the yield spread at time t of a muni bond issued by county i. Our main variable of interest is $PROPTAX_{i,t}{*}Post$, which is the interaction between $PROPTAX_{i,t}$, or the ratio of property tax revenue to the total population of county i, as reported in the general fund account of the CAFRs, and *Post*, an indicator that equals 1 for the year 2019 and 0 for the years 2016–2018.[20] c_i represents the county-fixed effects, and *CONTROLS* represents the control variables, which include a set of bond and county characteristics detailed below. Consistent with Gao et al (2020), when calculating the t-statistics, we cluster standard errors by county-issuance date.

The bond control variables include log issue size (*LNSIZE*), log of time to maturity (*LNMAT*, with maturity measured in months), log of trade size (*LNTRADE*, with trade size

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[19]General obligation bonds account for only a small fraction of the trades—271 unique general obligation bonds with 2,451 bond-month trade observations. The remaining are trades on other bonds (mainly revenue bonds).

[20] The TCJA lowered overall federal income tax rates, thus potentially increasing states' costs to issue tax-exempt municipal bonds—as borrowers require higher interest rates on municipal bonds to keep their returns competitive with similar risky taxable bonds (Feldstein and Metcalf, 1987). This effect will be captured by POST.

measured in dollars), and fixed effects for the numeric value of issuers' average credit rating (*RATING*), which includes non-rated issuers coded as 0. The county-level control variables include total liabilities per capita (*LIABILITIES*), log of total assets (*LN_ASSET_GEN*), and demeaned population size (*POP*, measured as the county population at time *t* less the mean county population over the sample period). We obtained bond controls from Bloomberg and MSRB; the county-level financial variables, *LIABILITIES* and *LN_ASSET_GEN*, also from Bloomberg; and *POP* from the U.S. Census Bureau (in our analyses we demean this variable). We measure all controls using information available before time *t*. Specifically, we measure bond characteristics during month *t*; we assume that the county financial statement data are available six months after the fiscal year-end,[21] while *POP* is available during the subsequent year t + 1.

As a robustness check, we also run regressions by replacing $PROPTAX_{i,t}$ with the ratio of property tax revenue from the county's government fund to the county's population size ($PROPTAX\_ALT1_{i,t}$). We derive property tax revenues from government funds to account for real estate tax revenues not being completely fungible for payments to debtholders, due to legal restrictions on the source and use of municipal funds generated through different taxes. As an additional robustness check, we use the ratio of the county's median home value to the county's population size (*HOMEVALUE*).

Table 2 reports summary statistics for the variables that we use in the analysis. First, we find that SPREAD has a mean and median value of -31 basis points and -37 basis points, respectively, which indicates that the counties' yield spreads are generally negative over the sample period. Second, LNSIZE and LNMAT have median values of 18.31 and 4.28, respectively,

[21]All counties in our sample have fiscal years ending in June of the calendar year. We assume that the financial statements for the fiscal year ending in June of year t become available in January of year t + 1.

which measure a median issuance size of $124 million and a median maturity is 6.4 years, respectively. Last, the per-capita property tax revenues in the sample are quite variable, as PROPTAX's mean value ($0.77) is 10 percent larger than PROPTAX's standard deviation ($0.70).

4. Empirical Results

4.1 Main Results

In this part of our analysis, we estimate Equation (1) to test the first hypothesis on whether municipal yield spreads become less sensitive to property tax revenues after the TCJA's SALT deduction cap. Holding all else equal, if future real estate tax revenues become less certain due to the SALT deduction cap, we should observe a generally weakened negative relationship between property tax revenue and municipal bond yields after the tax change. Thus, Hypothesis 1 predicts that the coefficient β_2 on $PROPTAX_{i,t}*Post$ should be positive when we estimate this regression model, indicating that counties with higher per-capita property tax revenues pay relatively higher yield spreads after the tax change.

Table 3 reports the estimated coefficients for our sampled municipal bonds. In Column 1, we find that the estimated coefficient β_2 on $PROPTAX_{i,t}*Post$ is significantly positive, indicating that counties with relatively higher per-capita property tax revenues incur higher borrowing costs after the tax change. The evidence supports the hypothesis that municipal borrowing costs become significantly less sensitive to property tax revenues after the SALT deduction cap. Column 1 shows that a one-percentage point increase in per-capita property tax revenues is associated with a 230-basis-point decline in yield spreads before the passage of the TCJA and a 217-basis-point decline afterward. This 13-basis-point difference in yield spreads is statistically and economically significant. To calculate the dollar costs implied by the difference, we apply the median issuance size ($124 million) and the median maturity (6.4 years) from Table 2; thus, this 13-basis-point increase identifies approximately $1.03 million of added per county borrowing costs per issue over the typical maturity period, for the high-property-tax counties relative to the low-property-tax counties.[22]

As discussed above, not all municipal funds are fungible. We thus replace *PROPTAX* with the per-capita property tax revenues drawn from the government fund, rather than the general fund (*PROPTAX_ALT1*). Applying the alternative measure, Column 2 in Table 3 indicates that after the 2018 tax change, a one-percentage point increase in per-capita property tax revenues is associated with a 21-basis-point increase in yield spreads. The evidence further supports our Hypothesis 1 that the SALT deduction caps reduce municipal offerings sensitivity to property tax revenues.

As a final robustness check, we replace the main independent variable with the ratio of each county's median home values to population size (*HOMEVALUE*). Unlike our prior tests, which assume that market participants rely on counties' financial reporting to price municipal bonds, this regression assumes that the SALT deduction cap should be more costly in counties with relatively higher property values. Thus, using per-capita home values rather than property tax revenues helps us gauge whether our documented property tax effects are generalizable to an alternative setting. Column 3 in Table 3 shows that, before the TCJA, a one-percentage point increase in per-capita home values is associated with yield spreads decreasing by approximately 168 basis points; after the TCJA, the effect is weaker, at approximately 161 basis points. This 7-basis-point increase in yield spreads is statistically significant and consistent with Hypothesis 1. Although the economic magnitude is about half the size of our baseline estimate in Column 1 of Table 3, the home value evidence is consistent with the SALT deduction cap reducing municipal

[22]$1.03 million = ($124 million) × (6.41 years) × (13 basis points).

bond offerings' sensitivity to property values, which supports Hypothesis 1.

The evidence presented in Table 3 shows that muni yields become less sensitive to property tax revenue after the TCJA. The results are consistent with the 2018 SALT deduction cap increasing the municipal borrowing costs associated with property taxes. Next, we investigate the heterogeneous effects of the TCJA's limit on the SALT deduction.

4.2 Channel: Do More County Financing Constraints Strengthen the Effects of the TCJA on the Relationship between Municipal Bond Spreads and Real Estate Tax Revenues?

In Panel A of Table 4, we examine whether the TCJA has differential effects on muni yield spreads' sensitivity to per-capita property tax revenues depending on the financing constraints of the county. We first test Hypothesis 2, which predicts that the TCJA has stronger property tax effects for municipalities that rely more on property taxes and thus are more financially constrained. To test this prediction, we estimate Equation (2), which includes a triple interaction term that allows us to consider additional cross-sectional variation in counties' characteristics that capture their financial constraints:

$$SPREAD_{i,j,t} = a + \boldsymbol{\beta_1}{*}PROPTAX_{j,t} + \boldsymbol{\beta_2}{*}PROPTAX_{j,t}{*}POST_t + \boldsymbol{\beta_3}{*}PROPTAX_{j,t}{*}POST_t{*}PARTITION$$
$$+ \boldsymbol{\beta_4}{*}PARTITION + \boldsymbol{\beta_5}{*}POST_t{*}PARTITION + \boldsymbol{\beta_6}{*}PROPTAX_{j,t}{*}PARTITION$$
$$+ \beta_7{*}POST_t + \beta_i{*}CONTROLS_{i,j,t} + c_j + e_{i,j,t}. \quad (2)$$

Specifically, we independently sort counties by the median home value per capita (HomeValuePerCap), the median sales tax per capita (SalesTaxPerCap), and the median transfers per capita (TransfersPerCap).[23] In pooled regressions, we interact $PROPTAX_{i,t}$*Post*, our main variable of interest, with indicator variables that identify counties with characteristics that are above the sample median. Thus, we categorize whether certain fiscal structures contribute to the TCJA's differential property tax effects on muni yield spreads. The regression models also include the fiscal-indicator variable and its interaction with *Post (PROPTAX*POST*PARTITION)*, to account for differential effects of the TCJA on municipal bond offerings that are not directly related to per-capita property tax revenues. The idea with these sample splits is that counties with higher property values or more constrained resources can implicitly rely more on property tax revenues than counties with lower property values or less constrained operating resources.

That is, we use a three-way interaction design to meaningfully compare regression coefficients across different subsamples in the pre- and post-TCJA periods. In particular, in this design the coefficient on $PROPTAX_{i,t}$ reflects the effect for the "below-median/("control" group) subsample in the pre-treatment period, and the coefficient on $PROPTAX_{i,t}$*Post* reflects the marginal difference in this coefficient for the control group between the pre- and post-TCJA periods. Similarly, the coefficient on $PROPTAX_{i,t}$*PARTITION* reflects the marginal difference in the regression coefficient in the pre-tax cut period for the "above-median/treatment" group. Finally, the three-way interaction term $PROPTAX_{i,t}$*POST*PARTITION* captures the marginal difference in the regression coefficient for the treatment group in the post-TCJA period. We report the results of these regressions in columns ("specifications") (1)-(3) of Panel A of Table 4.

The coefficient on $PROPTAX_{i,t}$ is significantly negative in all specifications, confirming

[23]Transfer accounts typically contain money given by states to municipalities for specific purposes. For example, an account could hold specific money earmarked for road building and education. Transfers are typically restricted funds. The money in transfer accounts cannot be spent on anything other than that for which it is earmarked.

prior findings in the literature (e.g., Palumbo and Zarowski, 2012) that counties with higher levels of property tax revenues enjoy lower municipal bond spreads; this coefficient captures the relationship between property tax revenues and spreads in the pre-tax cut period for the control group. The coefficient on $PROPTAX_{i,t}$*Post*, which captures the marginal effect of the TCJA on the control group, is also negative and significant in specifications 1 (low home values) and 2 (low sales tax per capita), which is not consistent with the results reported in Table 3; however, consistent with the results, the coefficient is positive and significant in specification 3, which captures the cross-sectional effects of counties with low levels of government transfers. In other words, for the counties with lower home values and lower sales taxes, the effect of the SALT deduction cap is not consistent with that observed in Table 3: Bond spreads become more sensitive to the availability of real estate tax revenue, reflecting the greater importance of such revenue for these counties. However, for counties with less dependence on governmental transfers, real estate taxes significantly affect bond spreads even more in the post-TCJA period. This is consistent with the notion that these counties likely have more diversified sources of revenue, do not need government transfers, and thus rely less on real estate tax revenues.

For our pre-TCJA period treatment sample, as reflected in $PROPTAX_{i,t}$*PARTITION*, the coefficient of this interaction term is positive in specifications 1, 2, and 3, which means that for counties with higher home values (specification 1), for counties with higher sales taxes (specification 2), and for counties with higher levels of transfers per capita (specification 3), real estate taxes have a less negative impact on spreads in the pre-TCJA period.

We find that our three-way interaction term $PROPTAX_{i,t}$*POST*PARTITION* is positive and significant in specifications 1 and 2 and negative and insignificant in specification 3; that is, in the post-TCJA period, the municipal bond spreads are less negatively affected by the level of real estate tax revenues per capita for high home value counties, counties with higher sales taxes; we do not find a similar effect for counties with *higher* levels of intergovernmental transfers. In other words, in the post-TCJA period, for counties less financially constrained or less dependent on other government sources (i.e., have lower transfers), real estate taxes become even less important for muni bond pricing.

To illustrate this more clearly, in the bottom part of Panel A in Table 4, we compare total coefficients for the different subgroups. Using Column 1 as an example, this analysis demonstrates that in the post-TCJA period, the relationship between real estate taxes per capita and municipal bond spreads is *more* negative for counties with lower home values (i.e., control group) ($\beta_1 = -8.63$ vs. $\beta_1 + \beta_2 = -8.93$); however, for counties with higher home values (i.e., treatment group), total coefficients become *less* negative after the TCJA ($\beta_1 + \beta_6 = -1.29$ vs. $\beta_1 + \beta_2 + \beta_3 + \beta_6 = -0.99$). These results support our predictions in Hypothesis 2 that local governments' sensitivity to property taxes post-TCJA decreases in less financially constrained counties.

Collectively, the results in Panel A of Table 4 confirm our prediction that the SALT deduction cap heterogeneously reduces yield spreads' sensitivity to property tax revenues. The evidence indicates that municipalities' reliance on such property taxes is an important economic channel through which local governments' borrowing costs increase post-TCJA. We next examine whether counties' political economy contributes to the property tax effects of the TCJA.

As a corollary to our preceding analysis, in Panel B of Table 4, we reported the estimation results of Equation (2) in which we explore cross-sectional variation in our results through liability per capita (LiabPerCap, specification 1), operating expenses per capita (OpexPerCap, specification 2), and cash per capita (CashPerCap, specification 3). The results are consistent with those in Panel A; that is, in the post-TCJA era, for counties with higher level of financial obligations and operating expenses, muni yield spreads are becoming even more dependent on real estate tax revenue in the post-TCJA period, as reflected in the negative coefficient on *PROPTAX*POST*PARTITION* in specifications 1 and 2, while for counties with a higher cash cushion, muni yield spreads become less dependent on real estate taxes, as reflected in the positive coefficient on *PROPTAX*POST*PARTITION* in specification 3. These results provide broad support for Hypothesis 2 that in the post-TCJA period, bond spreads of wealthier counties or counties with greater financial flexibility become less dependent on real estate tax revenues.

4.3 Channel: Does Voter Opposition to Property Taxes Increase the Effects of the TCJA?

In Table 5, we perform analysis on whether the TCJA has stronger property tax effects on offerings from municipalities where voters are less likely to support property tax levies, as predicted in Hypotheses 3 and 4. To test these predictions, we repeat the aforementioned exercises, but with sample split by voting demographics and voter income. Specifically, the cuts to test Hypothesis 3 are based on the share of residents with some college education and the share of residents over age 65. For Hypothesis 4, we cut on the decrease in the share of Democratic voters from the presidential elections of 2016 to 2020 and the county's per-capita income.

Consistent with Hypothesis 3, the regressions in columns (1) and (2) in Table 5 show that the post-TCJA reduction in the sensitivity of municipal bond yield spreads to property taxes is significantly larger for bond offerings from counties where voters are more likely to oppose property taxes (i.e., counties with lower levels of college education [specification 1]) and counties with a higher population share above 65 (specification 2). As discussed previously, college-educated voters are more likely to support property taxes. Thus, if this political behavior is priced into municipal offerings, post-TCJA, counties with larger shares of college-educated residents would be expected to have lower borrowing costs associated with property taxes than counties with smaller shares of such residents. Consistent with this intuition, the negative and significant coefficient on $PROPTAX_{i,t}$*POST*PARTITION* in specification 1 show that, post-TCJA, a one-percentage point increase in per-capita property tax revenues reduces yield spreads of municipal bond offerings from counties with higher and lower shares of college-educated residents by 132 and 44 basis points, respectively. This 88-basis-point difference in yield spreads is nearly seven times the magnitude of our baseline estimate from Column 1 of Table 3, which supports our prediction in Hypothesis 3. Regarding total coefficient estimates, we find that for the subsample of counties with lower shares of college-educated residents, the relationship between municipal bond spreads and real estate tax revenues becomes non-significant post-TCJA (β_1 = –0.74, significant at 0.01 level vs. $\beta_1 + \beta_2$ = –0. 43, not significant); for counties with higher shares of college-educated residents, this relationship becomes even more negative ($\beta_1 + \beta_6$ = –7.62 vs. $\beta_1 + \beta_2 + \beta_3 + \beta_6$ = –8.199).

In Column 2 in Table 5, we partition the sample by the share of residents over age 65. Importantly, we find that in the post-TCJA period, the reduced yield spreads' sensitivity to property taxes is significantly positive (156 basis points) that further indicates that our documented property tax effects are stronger in counties where voters are less likely to support property tax increases. Moreover, the economic magnitude of the post-TCJA property tax effect reported in this test is about 12 times the magnitude of our baseline estimates in Table 3, Column 1. These large differentials in the yield spread suggest that post-TCJA, voters' opposition to property taxes can materially increase municipal borrowing costs, which confirms our Hypothesis 1. These findings are supported by differences in total regression coefficients, in which in the post-TCJA period, the relationship between real estate tax revenues and bond spreads is significantly less negative in the counties with an older population share than in the counties with younger populations.

Consistent with Hypothesis 4, we also find that for counties with decreases in the share of Democratic votes (specification 3), the relationship between municipal bond spreads and real estate tax revenues becomes less negative post-TCJA. This is supported by the pattern in total regression coefficients, suggesting incrementally weakening the negative association between bond spreads and real estate tax revenues is more pronounced for Republican-leaning counties in the post-TCJA period. For counties with higher per-capita income (specification 11), spreads are less sensitive to real estate tax revenues in the post-TCJA period (178 and 122 basis points; respectively), confirming our prediction; moreover, the total coefficient for the treatment sample in the post-TCJA period is no longer significant, but is negative and significant in the pre-TCJA period.

Taken together, the political evidence in Table 5 further supports Hypotheses 3 and 4 that the SALT deduction cap reduces the sensitivity of municipal borrowing costs to property taxes for counties where tax increases are less likely to have citizens' support for all specifications.

4.4 Mechanism: Does Voter Support for Property Taxes Decrease after the TCJA?

In Table 6, we examine the final prediction that voters reduce their support for property tax proposals after the TCJA's SALT deduction cap. To test this prediction, we collect counties' ballot proposals from 2016 to 2019 from the office of the California Secretary of State (SOS).[24] Because having propositions on electoral ballots requires considerable voter engagement, the presence or absence of property tax proposals can indicate voters' support for or opposition to such taxes. The

[24]See https://www.sos.ca.gov/elections/county-city-school-district-ballot-measure-election-results; accessed on October 4, 2022.

SOS categorizes all ballot propositions by type, including whether proposals are for property taxes. We apply the SOS's labeling to create a property tax indicator variable that assigns a value of 1 for property tax proposals and 0 for all other proposals. We then re-estimate Equation (1), but replacing the dependent variable with the property tax indicator variable. Panel A of Table 6 characterizes the data used for this analysis. Our final sample contains 200 proposals, 44 of which are property tax related. We note that the observations in the regressions that include per-capita home values are smaller than 200, because the home value data are not available for all county years. The analysis performed here retains all the control variables from the previous models. When calculating t-statistics, we cluster standard errors by county election date.

Panel B of Table 6 presents our ballot regression analysis results. Similar to Table 3, Columns 1 to 3 of Table 6 report the regression results, the main independent variables of which are per-capita property tax revenues from the general fund, the government fund, and per-capita home values, respectively. We predict that the estimated coefficient on $PROPTAX_{i,t}*Post$ (and its alternatives in specifications 2 and 3) is negative, indicating that voters in counties with higher per-capita property tax revenues are less likely to agree to have property tax propositions on the ballot after the 2018 tax change. Consistent with our prediction, Columns 1 and 2 show that the estimated coefficient of the interaction term is significantly negative when we condition voters' support for property tax proposals on residents' per-capita property tax revenues. In Column 3, we find that the relationship between per-capita home values and property tax proposals is non-significant, but it is not more negative post-TCJA. While this evidence based on home value does not support our hypothesis, the per-capita property tax evidence is consistent with voters' opposition to property tax proposals being stronger after the SALT deduction cap, particularly in counties with relatively higher property taxes. The evidence indicates that post-TCJA, voters' opposition to property taxes are informed, at least in part, by their property taxes, which supports the intuition underlying our Hypothesis 1.

A concern for the above analysis is that fewer property tax proposals on the ballot may not actually reflect voters' opposition to property taxes. For example, voters may pass more property tax proposals even if fewer are on the ballot. We directly address this concern in Columns 4–6 of Panel B of Table 6 by replacing the dependent variable with an indicator for the passing of property tax proposals. We find similar results based on this variation of analysis, which provides additional evidence of the mechanism we propose.

Another concern is that voters' opposition to ballot proposals may not be specific to property taxes. That is, we may observe an overall reduction in voters' support for proposals, regardless of the type of measure considered. Our falsification tests in Columns 7–9 of Table 6 address this concern. We replace the dependent variable with an indicator for any proposal passing. When we condition voters' passage of all proposals on per-capita property taxes or home values, we find a significantly positive relationship, including after the TCJA, which indicates that voter support of ballot proposals is generally greater in counties with higher property taxes or home values. This finding is important for our analysis, as it suggests that, after TCJA, voters in higher per-capita property tax counties do not show reluctance in passing general proposals that are unrelated to property tax.

Overall, the results presented in Table 6 show that voters in counties with higher per-capita property taxes are less likely to pass property tax proposals after the TCJA. Importantly, we do not observe voters' opposition to property taxes more generally for all ballot proposals, which suggests that the SALT deduction cap reduces voters' support for future property tax proposals in particular.

5. Robustness

5.1 Robustness Check: Excluding 2018 from the Analysis

Our empirical approach treats 2018 as a transition year of the TCJA. One concern with this approach is that including 2018 traded bonds may have confounding effects on our study. To address this concern, Table 7 repeats Table 3 but excludes 2018 from the analysis. As the table shows, in this restricted sample, muni yield spreads continue to be less sensitive to property taxes after the TCJA. The results confirm that our documented property tax effects are not entirely driven by including offers that traded during the transition year, which supports our prediction that the tax change reduces the sensitivity of municipal borrowing costs to property taxes.

6. Conclusion

Using California counties' 2016–2019 financial and municipal bond data, we examine the impact of the TCJA cap of the SALT deduction on the relationship between counties' municipal bond spreads and real estate tax revenues. While largely confirming prior findings that, in general, counties with higher revenues—driven by real estate tax revenues-- enjoy lower municipal bond spreads and higher credit ratings (e.g. Maher et al 2016, Palumbo and Zaporowski, 2012), we find that after passage of the TCJA, the impact of real estate tax revenue on municipal bond spreads diminishes, consistent with the effect that SALT deduction limitation negatively affects counties' expected ability to raise more real estate tax revenues. We find that this impact is more pronounced for counties where taxpayers are less likely to support future tax increases (i.e., the counties where voters tend to be more Republican or older) and for less financially or fiscally constrained counties (who must rely less on future real estate tax increases for their financing). Using actual California voting data, we consistently find that voters are less likely to support tax ballot proposals for counties with higher real estate tax revenues in the post-TCJA period. Overall, our evidence indicates that real estate tax revenue becomes a less important determinant of municipal bond spreads post-TCJA, likely because of voters' unwillingness to continue supporting future real estate tax increases as a result of voters' expected inability to derive tax deduction benefits from further local tax payments due to the TCJA's limitation of the SALT deduction. Our research thus contributes to the literature on the tax effects on municipal bond spreads and to the literature that documents the economic impact of U.S. tax laws.

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Appendix A: Credit Rating Scores

Moody's		S&P		Fitch	
Rating	Score	Rating	Score	Rating	Score
Aaa	1	AAA	1	AAA	1
Aa1	2	AA+	2	AA+	2
Aa	3	AA	3	AA	3
Aa2	3	AA-	4	AA-	4
Aa3	4	A+	5	A+	5
A1	5	A	6	A	6
A	6	A-	7	A-	7
A2	6	BBB+	8	BBB+	8
A3	7	BBB	9	BBB	9
Baa1	8	BBB-	10	BBB-	10
Baa	9	BB+	11	BB+	11
Baa2	9	BB	12	BB	12
Baa3	10	BB-	13	BB-	13
Ba1	11	B+	14	B+	14
Ba	12	B	15	B	15
Ba2	12	B-	16	B-	16
Ba3	13	CCC+	17	CCC+	17
B1	14	CCC	18	CCC	18
B	15	CCC-	19	CCC-	19
B2	15	CC	20	CC	20
B3	16	C	21	C	21
Caa1	17	D	25	DDD	23
Caa	18	SUSP	26	DD	24
Caa2	18	NR	27	D	25
Caa3	19			SUSP	26
Ca	20			NR	27
C	21				
SUSP	26				
NR	27				

Appendix B: Variables Definitions

Panel A: Bond-level variables	
Yield spreads (SPREAD)	The percentage yield spread between a municipal bond and a coupon-equivalent risk-free bond. Winsorized at the 1st and 99th percentiles. (Source: MSRB)
LNSIZE	The natural log of a municipal bond's issue amount ($M). Winsorized at the 1st and 99th percentiles. (Source: FTSE/Russell Municipal Securities Database)
LNMAT	The natural log of a municipal bond's years to maturity. Winsorized at the 1st and 99th percentiles. (Source: FTSE/Russell Municipal Securities Database)
LNTRADE	The natural log of the par value of a bond trade. Winsorized at the 1st and 99th percentiles. (Source: MSRB)
AVERAGE RATING SCORE	A municipal bond's historical rating expressed in numbers, as reported by Fitch, Moody's, and S&P credit rating agencies. Winsorized at the 1st and 99th percentiles. (Source: FTSE/Russell Municipal Securities Database)
Panel B: County-level variables	
PROPTAX	A county's per-capita property tax, measured in millions of dollars as the lagged property tax revenues from the general fund reported in county financial statements scaled by the county's U.S. Census population. Winsorized at the 1st and 99th percentiles. (Source: Bloomberg, U.S. Census)
PROPTAX_ALT	A county's per-capita property tax alternatively measured in millions of dollars as the lagged property tax revenues from the government fund reported in county financial statements scaled by the county's U.S. Census population. Winsorized at the 1st and 99th percentiles. (Source: Bloomberg, U.S. Census)
HOMEVALUE	A county's median home value scaled by the county's U.S. Census population. Winsorized at the 1st and 99th percentiles. (Source: American Community Survey, U.S. Census)
LIABILITIES	A county's total liabilities from the government fund measured in millions of dollars and scaled by the county's U.S. Census population. Winsorized at the 1st and 99th percentiles. (Source: Bloomberg, U.S. Census)
LN_ASSET_GEN	The natural log of a county's assets from the general fund measured in millions of dollars, as reported in county financial statements. Winsorized at the 1st and 99th percentiles. (Source:

	Bloomberg)
POPULATION	A county’s population as reported in the U.S. Census. Demeaned at the county level and winsorized at the 1st and 99th percentiles. (Source: U.S. Census)

Table 1: Sample Selection and Descriptive Statistics

Table 1 reports summary statistics for the rated municipal bonds issued by California counties from the FTSE/Russell Municipal Bond database (formerly called Mergent Municipal Bond Securities database) for the years 2016–2019. We use issuer names containing county names to match bonds to counties. We restrict the sample to plain-vanilla tax-exempt bonds, with fixed or zero coupons, which have at least one year of maturity at trading and trade at a par value no less than $10,000. We match bonds to county-level financial statement data obtained from Bloomberg. We exclude bonds issued by schools or local public utilities, as these entities report separate financial statements. We then match bonds to county-level demographics data obtained from the U.S. Census. **Panel A** reports the sample selection procedure and resulting bond-month observations. **Panel B** reports bond-month observations by the years in which the bonds are traded.

Panel A: Sample Selection—Traded Bonds

Sample selection procedure	Number of observations remaining
Muni securities issued by California counties with issuer name containing county names and with maturity no earlier than 2017 and issuing date no later than 2019	40,619 unique bonds
Restricted to plain-vanilla (no embedded options) tax-exempt bonds with fixed coupon or zero coupon	11,218 unique bonds
Excluding bonds issued by schools and local public utilities	4,049 unique bonds
Requiring bonds to have valid trades (traded par value above $10,000) in MSRB	2,703 unique bonds with 20,922 bond-month observations
Matching with U.S. Census population data	20,248 bond-month observations

Panel B: Trades Bonds Sample—Bond-Month Observations by Year

Year	Observations
2016	4,710
2017	5,020
2018	5,581
2019	4,937
Total	20,248

Table 2: Sample descriptive statistics (by bond issues in the sample)

Table 2 reports summary statistics for the bond characteristics and county-level control variables in the sample. See Appendix B for more information on these variables.

Variables	Mean	P50	Min	Max	Sd	Count
SPREAD	-0.31	-0.37	-1.14	1.21	0.44	20,248
LNSIZE	18.22	18.31	15.51	20.35	1.07	20,248
LNMAT	4.25	4.28	3.00	5.19	0.44	20,248
LNTRADE	10.61	10.31	9.21	13.98	1.12	20,248
AVERATINGSCORE	3.03	3.50	0.00	10.00	2.31	20,248
PROPTAX	0.77	0.50	0.11	2.22	0.70	20,248
PROPTAX _ALT1	0.91	0.55	0.12	3.00	0.92	20,248
HOMEVALUE	0.71	0.24	0.05	4.23	0.84	19,486
LIABILITIES	8.62	3.08	1.27	35.24	11.03	20,248
LN_ASSET_GEN	7.21	7.55	3.53	8.91	1.37	20,248
POP	0.46	430.88	-52,551.75	47,421.25	15,643.	20,248
POPULATION	3,298,869	1,527,301	12,567	10,100,000	3,693,383	20,248

Table 3: Municipal Bond Spreads and Property Taxes post-TCJA

Table 3 presents the results of Equation 1, estimated with monthly municipal bond yield spreads regressed on counties' per-capita *PROPTAX*, the lagged property tax revenues from the general fund reported in county financial statements scaled by counties' U.S. Census population. The sample covers California municipal bonds from the FTSE/Russell database for the years 2016–2019; *POST* takes a value of 1 in 2019, following the tax change, and 0 otherwise. Columns 2 and 3 present results with per-capita property taxes reported in counties' government fund and home values per capita, respectively. All models include rating and county fixed effects. T-statistics clustered by county-issuance date are reported in parentheses. ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.1$, respectively. All variables are defined in Appendix B.

DEPVAR=SPREAD		(1)	(2)	(3)
PROPTAX	$\boldsymbol{\beta_1}$	**-2.2977*****		
		(-5.398)		
PROPTAX*POST	$\boldsymbol{\beta_2}$	**0.1259*****		
		(2.816)		
PROPTAX_ALT1	$\boldsymbol{\beta_1}$		**0.0510**	
			(0.113)	
PROPTAX_ALT1*POST	$\boldsymbol{\beta_2}$		**0.2109*****	
			(13.444)	
HOMEVALUE	$\boldsymbol{\beta_1}$			**-1.6815*****
				(-12.303)
HOMEVALUE*POST	$\boldsymbol{\beta_2}$			**0.0681*****
				(3.492)
POST	β_3	-0.0279	-0.0608**	0.0595**
		(-0.781)	(-2.093)	(2.249)
LNSIZE	β_4	-0.0476***	-0.0472***	-0.0471***
		(-4.366)	(-4.351)	(-4.330)
LNMAT	β_5	0.0811**	0.0792**	0.0892**
		(2.357)	(2.308)	(2.504)
LNTRADE	β_6	-0.0486***	-0.0488***	-0.0488***
		(-11.869)	(-11.918)	(-11.864)
LIABILITIES	β_7	0.1016***	0.0107	0.0894***
		(5.069)	(0.481)	(8.277)
LN_ASSET_GEN	β_8	-1.7766***	-2.1186***	-2.2216***
		(-8.251)	(-9.182)	(-10.429)
POP	β_9	-0.0000***	-0.0000***	-0.0000***
		(-14.004)	(-16.522)	(-13.633)
CONSTANT	β_0	14.4196***	15.8368***	17.3558***
		(10.602)	(11.327)	(12.403)
Observations		20,248	20,248	19,486
$\beta_1 + \beta_2$		-2.172	0.262	-1.613
F stat of $\beta_1 = \beta_1 + \beta_2$		0.005	0.000	0.001
Adj R^2		0.440	0.435	0.461

Table 4: Cross-sectional Test with Municipal Financing Constraints and Demographics

Table 4 presents the results of Equation 2, estimated with monthly municipal bond yield spreads regressed on counties' per-capita *PROPTAX*, the lagged property tax revenues from the general fund reported in county financial statements scaled by the counties' U.S. Census population. In these regressions, we split the bond sample by counties' financing constraints and demographics to assess whether the Trump tax cuts led to differential property tax effects. The sample covers California municipal bonds from the FTSE/Russell database for the years 2016–2019. **Panel A** reports splits by financing constraints. Columns 1 and 2 split the sample into above- and below-median groups by counties' per-capita home values and per-capita sales tax from the government fund, respectively; Column 3 splits the sample by counties' intergovernmental transfers from the general fund scaled by counties' population size. **Panel B** reports splits by fiscal resources. Columns 1, 2, and 3 split the sample into above- and below-median counties by counties' per-capita liabilities, operating expense, and cash holdings, respectively. All fiscal resources are obtained from counties' government fund. As before, *POST* takes a value of 1 in 2019, following the tax change, and 0 otherwise; all models include rating and county fixed effects. T-statistics clustered by county-issuance date are reported in parentheses. ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.1$, respectively. All variables are defined in Appendix B.

Panel A: Partitions by fiscal resources				
DEPVAR=SPREAD		(1)	(2)	(3)
PARTITION=		HomeValue PerCap AboveMed	SalesTax PerCap AboveMed	Transfers PerCap AboveMed
PROPTAX	**β_1**	**-8.6315***** **(-11.816)**	**-3.6697***** **(-6.932)**	**-5.9105***** **(-10.539)**
PROPTAX*POST	**β_2**	**-0.2999*** **(-1.931)**	**-0.9353***** **(-6.181)**	**0.4312***** **(2.842)**
PROPTAX*POST*PARTITION	**β_3**	**0.6056***** **(4.021)**	**1.0138***** **(6.072)**	**-0.2039** **(-1.298)**
PARTITION	β_4	-2.3477*** (-10.693)	-0.4055*** (-3.001)	-1.1839*** (-8.094)
POST*PARTITION	β_5	-0.6919*** (-10.060)	-0.5901*** (-8.339)	-0.1362 (-1.603)
PROPTAX*PARTITION	β_6	7.3377*** (11.786)	1.1450*** (3.487)	4.9636*** (7.947)
POST	β_7	0.3700*** (6.135)	0.5642*** (12.930)	0.0595 (0.755)
LNSIZE	β_8	-0.0478*** (-4.385)	-0.0706*** (-5.485)	-0.0474*** (-4.384)
LNMAT	β_9	0.1106*** (3.216)	0.1360*** (3.507)	0.0873** (2.552)
LNTRADE	β_{10}	-0.0471*** (-12.084)	-0.0444*** (-9.868)	-0.0490*** (-12.056)
LIABILITIES	β_{11}	0.0026 (0.157)	0.0660*** (3.138)	0.0466** (2.094)
LN_ASSET_GEN	β_{12}	-0.5818*** (-4.290)	-0.5716*** (-4.233)	-1.8951*** (-8.687)
POP	β_{13}	-0.0000*** (-16.612)	-0.0000*** (-24.112)	-0.0000*** (-14.506)
CONSTANT	β_0	8.3727*** (10.035)	6.5880*** (7.810)	16.0526*** (11.696)
Observations		19,486	15,383	20,177
The effect of POST = 0 for below-median/control group	β_1	-8.6315***	-3.6697***	-5.9105***
The effect of POST = 1 for below-median/control group	$\beta_1+\beta_2$	-8.931***	-4.605***	-5.479***
The effect of POST = 0 for above-median/treatment group”	$\beta_1+\beta_6$	-1.294***	-2.525***	-0.947*
The effect of POST = 1 for above-median/treatment effect:	$\beta_1+\beta_2+\beta_3+\beta_6$	-0.988***	-2.446***	-0.720***
F-stat of $\beta_1 + \beta_2 + \beta_3 + \beta_6 = \beta_1 + \beta_2$		0.000	0.000	0.000
Adj R^2		0.498	0.489	0.449

Panel B: Partitions by financing constraints				
PARTITION==		(1)	(2)	(3)
VARIABLES		LiabPerCap AboveMed	OpexPerCap AboveMed	CashPerCap AboveMed
PROPTAX	**β_1**	**-3.2560*** (-4.913)**	**-0.0043 (-0.008)**	**-3.4309*** (-4.635)**
PROPTAX*POST	**β_2**	**0.4828*** (3.715)**	**1.6538** (2.031)**	**-0.6817** (-1.968)**
PROPTAX*POST*PARTITION	**β_3**	**-0.4432*** (-2.684)**	**-1.3877* (-1.699)**	**1.2988*** (4.189)**
PARTITION	β_4	-0.3142*** (-4.787)	0.4807*** (4.751)	1.1130*** (7.189)
POST*PARTITION	β_5	0.0295 (0.369)	0.2382 (0.888)	-0.4120*** (-4.206)
PROPTAX*PARTITION	β_6	0.1631 (0.876)	-1.1647*** (-4.853)	-2.4883*** (-7.249)
POST	β_7	0.0007 (0.011)	-0.4045 (-1.523)	0.4090*** (3.595)
LNSIZE	β_8	-0.0481*** (-4.452)	-0.0486*** (-4.463)	-0.0473*** (-3.848)
LNMAT	β_9	0.0867** (2.535)	0.0884** (2.564)	0.1555*** (4.172)
LNTRADE	β_{10}	-0.0486*** (-12.012)	-0.0478*** (-11.757)	-0.0469*** (-10.038)
LIABILITIES	β_{11}	0.1504*** (5.765)	0.0557*** (2.685)	-0.1865*** (-3.812)
LN_ASSET_GEN	β_{12}	-1.9884*** (-9.752)	-1.8458*** (-8.737)	-0.6446*** (-3.278)
POP	β_{13}	-0.0000*** (-8.231)	-0.0000*** (-15.788)	-0.0000*** (-18.027)
CONSTANT	β_0	16.3679*** (12.332)	13.9764*** (10.523)	6.8624*** (5.061)
Observations		20,248	20,248	15,732
The effect of POST = 0 for below-median/control group	β_1	-3.2560***	-0.0043	-3.4309***
The effect of POST = 1 for below-median/control group	$\beta_1+\beta_2$	-2.773***	1.649	-4.113***
The effect of POST = 0 for above-median/treatment group	$\beta_1+\beta_6$	-3.093***	-1.169***	-5.919***
The effect of POST = 1 for above-median/treatment effect: $\beta_1 + \beta_2 + \beta_3 + \beta_6$	$\beta_1+ \beta_2+ \beta_3+ \beta_6$	-3.053*	-0.903***	-5.302**
F-stat of $\beta_1 + \beta_2 + \beta_3 + \beta_6 = \beta_1 + \beta_2$		0.064	0.003	0.017
Adj R^2		0.456	0.454	0.497

Table 5: Cross-sectional Test with Voter Demographics

Table 5 presents the results of Equation 2, estimated with monthly municipal bond yield spreads regressed on counties' per-capita *PROPTAX*, the lagged property tax revenues from the general fund reported in county financial statements scaled by the counties' U.S. Census population. In these regressions, we split the bond sample by counties' voter demographics to assess whether the Trump tax cuts led to differential property tax effects. The sample covers California municipal bonds from the FTSE/Russell database for the years 2016–2019. Columns reports splits by demographics. Columns 1 and 2 split the sample into above- and below-median counties by the share of residents with some college education and over age 65, respectively. Column 3 splits the sample into counties that experienced a decrease in the Democratic vote share from the 2016 and 2020 presidential elections. Column 4 splits the sample into above- and below-median counties by counties' per-capita income. As before, *POST* takes a value of 1 in 2019, following the tax change, and 0 otherwise; all models include rating and county fixed effects. T-statistics clustered by county-issuance date are reported in parentheses. ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.1$, respectively. All variables are defined in Appendix B.

Partitions by voter demographics					
PARTITION=		(1) SomeCollege AboveMed	(2) PopShare65 AboveMed	(3) Dem16to20 Decrease	(4) PerCapInc AboveMed
VARIABLES					
PROPTAX	**β_1**	**-0.7385***	**-0.8880***	**-3.6674*******	**0.1864**
		(-1.781)	**(-1.809)**	**(-8.645)**	**(0.098)**
PROPTAX*POST	**β_2**	**0.3042*******	**-1.1235*******	**-0.0881***	**-1.0101*******
		(7.465)	**(-6.329)**	**(-1.948)**	**(-4.107)**
PROPTAX*POST*PARTITION	**β_3**	**-0.8835*******	**1.5587*******	**1.7812*******	**1.2217*******
		(-5.345)	**(9.440)**	**(6.534)**	**(4.968)**
PARTITION	β_4	-	-0.2232**	-	0.4044
			(-2.292)		(0.711)
POST*PARTITION	β_5	0.6524***	-0.8925***	-1.1847***	-0.4484***
		(8.918)	(-10.639)	(-8.268)	(-5.046)
PROPTAX*PARTITION	β_6	-6.8814***	0.2659***	-3.7004***	-1.7277
		(-5.560)	(4.646)	(-5.761)	(-0.983)
POST	β_7	-0.1882***	0.5652***	0.1749***	0.3573***
		(-4.480)	(7.474)	(4.342)	(4.557)
LNSIZE	β_8	-0.0494***	-0.0506***	-0.0486***	-0.0476***
		(-4.588)	(-4.717)	(-4.446)	(-4.407)
LNMAT	β_9	0.0919***	0.0984***	0.0970***	0.0867**
		(2.710)	(2.907)	(2.864)	(2.547)
LNTRADE	β_{10}	-0.0483***	-0.0486***	-0.0473***	-0.0483***
		(-12.024)	(-12.321)	(-11.885)	(-11.982)
LIABILITIES	β_{11}	0.0275	0.0127	0.1356***	0.0701***
		(1.569)	(0.545)	(7.064)	(3.573)
LN_ASSET_GEN	β_{12}	-1.7069***	-1.9459***	-0.9494***	-1.8460***
		(-7.937)	(-8.966)	(-6.405)	(-8.504)
POP	β_{13}	-0.0000***	-0.0000***	-0.0000***	-0.0000***
		(-14.442)	(-17.504)	(-17.017)	(-12.530)
CONSTANT	β_0	13.8938***	15.2252***	9.6615***	14.1645***
		(10.356)	(10.813)	(10.247)	(9.354)
Observations		20,248	20,248	20,248	20,248

The effect of POST = 0 for below-median/control group	β_1	-0.7385*	-0.8880*	-3.6674***	0.1864
The effect of POST = 1 for below-median/control group	$\beta_1+\beta_2$	-0.434	-2.012***	-3.755***	-0.824
The effect of POST = 0 for above-median/treatment group	$\beta_1+\beta_6$	-7.620***	-0.622	-7.368***	-1.541***
The effect of POST = 1 for above-median/treatment effect: $\beta_1 + \beta_2 + \beta_3 + \beta_6$	$\beta_1+ \beta_2+ \beta_3+ \beta_6$	-8.199***	-0.187***	-5.675***	-1.330
F-stat of $\beta_1 + \beta_2 + \beta_3 + \beta_6 = \beta_1 + \beta_2$		0.000	0.000	0.002	0.774
Adj R^2		0.463	0.469	0.468	0.451

Table 6: Voting Behavior on Property Tax Ballot Proposals post-TCJA

Table 6 presents the results of Equation 3, estimated with an indicator for counties' ballot proposals related to property taxes regressed on counties' per-capita *PROPTAX*, the lagged property tax revenues from the general fund reported in county financial statements scaled by the counties' U.S. Census population. The sample covers elections in California counties as reported by the Office of California's Secretary of State (SoS) for the years 2016–2019; we use SoS classifications to identify ballot measures as property tax related. **Panel A** reports the sample selection and ballot proposals by year. We restrict the sample to counties with the financial and demographic controls used in the regression analysis. **Panel B** reports regression results of Equation 3. Columns 1–3 and 4–6 present results in which the dependent variable is an indicator for property proposals appearing on the ballot and passing, respectively. Columns 7–9 present falsification tests in which the dependent variable is an indicator for any ballot measure that passes. As before, *POST* takes a value of 1 in 2019, following the tax change, and 0 otherwise. All models include county-fixed effects. T-statistics clustered by county-election date are reported in parentheses. ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.1$, respectively. All variables are defined in Appendix B.

Panel A: Sample Selection and Ballot Proposals by Year

Variable	Count
All	527
Matched	200
Property tax	44
Passed	29

Ballot Proposals: By Year

Year	All Proposals	Property Tax	Property Tax Passed	Mean	P50	Min	Max	Sd
2016	104	19	12	0.115	0	0	1	0.321
2017	11	6	2	0.182	0	0	1	0.405
2018	72	13	10	0.139	0	0	1	0.348
2019	13	6	5	0.385	0	0	1	0.506
Total	200	44	29	0.145	0	0	1	0.353

Panel B: Impact of Trump Tax Cuts on Voting Behavior

VARIABLES		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Property Tax Ballot Proposed=1			Property Tax Ballot Passed=1			Any Proposal Ballot Passed=1		
PROPTAX	β_1	**-2.0969***			**-0.7820**			**3.9171*****		
		(-1.960)			**(-0.721)**			**(4.232)**		
PROPTAX*POST	β_2	**-0.6193*****			**-0.4624****			**0.2826***		
		(-4.247)			**(-2.240)**			**(1.674)**		
PROPTAX_ALT1	β_3		**-2.5754****			**-2.2374****			**3.1233*****	
			(-2.447)			**(-2.068)**			**(3.185)**	
PROPTAX_ALT1*POST	β_4		**-0.2287*****			**-0.2236****			**-0.1369**	
			(-3.598)			**(-2.435)**			**(-1.260)**	
HOME_VALUE	β_5			**-0.7366**			**-0.1115**			**1.1185*****
				(-1.463)			**(-0.273)**			**(3.760)**
HOME_VALUE*POST	β_6			**0.0637**			**0.0787**			**0.0360**
				(0.673)			**(0.802)**			**(0.468)**
POST	β_7	0.9777***	0.6591***	0.1353	0.8872***	0.6915***	0.1511	0.0019	0.3713	0.1236
		(4.671)	(4.182)	(0.470)	(2.833)	(2.967)	(0.576)	(0.009)	(1.661)	(0.531)
LIABILITIES	β_8	0.0406	0.0883	-0.0753	-0.0029	0.0634	-0.0627	-0.0527	-0.0721	0.0757**
		(0.658)	(1.404)	(-1.227)	(-0.042)	(0.903)	(-1.053)	(-1.144)	(-1.173)	(2.281)
LN_ASSET_GEN	β_9	1.2391	1.5544	1.5153	0.6706	1.3972	0.8144	-2.5574***	-2.4338***	-2.1199***
		(1.247)	(1.362)	(1.379)	(0.671)	(1.210)	(0.774)	(-3.181)	(-2.773)	(-3.113)
POP	β_{10}	-0.0000	-0.0000	-0.0000	0.0000	-0.0000	0.0000	0.0000***	0.0000***	0.0000***
		(-1.357)	(-1.537)	(-1.097)	(0.073)	(-0.315)	(0.208)	(3.904)	(3.619)	(3.617)
Constant	β_{11}	-5.6245	-7.2054	-7.3443	-3.1151	-6.4634	-4.1585	12.7974***	12.6008***	11.4016***
		(-1.078)	(-1.215)	(-1.197)	(-0.588)	(-1.073)	(-0.707)	(3.084)	(2.757)	(2.979)
Observations		200	200	171	200	200	171	200	200	171
Sum(2Coefs)	$\beta_1 + \beta_2$	-2.716	-2.804	-0.673	-1.244	-2.461	-0.0328	4.200	2.986	1.155
F-statistic of $\beta_1 + \beta_2$		0.000	0.001	0.503	0.028	0.017	0.425	0.098	0.211	0.641
Adj R^2		0.207	0.211	0.193	0.184	0.199	0.158	0.105	0.092	0.158

Table 7: Robustness: Excluding 2018

Table 7 repeats Table 3, which estimates Equation 1 but excludes all observations that occurred in 2018. In these tests, we consider 2018 a transition year for the 2017 tax change. As before, the sample covers California municipal bonds from the FTSE/Russell database for the years 2016, 2017, and 2019. Also as before, *POST* takes a value of 1 in 2019, following the tax change, and 0 otherwise. All models include rating and county-fixed effects. T-statistics clustered by county-issuance date are reported in parentheses. ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.1$, respectively. All variables are defined in Appendix B.

DEPVAR=SPREAD		(1)	(2)	(3)
PROPTAX	β_1	**-3.4724*****		
		(-8.839)		
PROPTAX*POST	β_2	**0.6558*****		
		(11.020)		
PROPTAX_ALT1	β_1		**-1.3094*****	
			(-3.991)	
PROPTAX_ALT1*POST	β_2		**0.4883*****	
			(8.924)	
HOMEVALUE	β_1			**-0.4381***
				(-1.883)
HOMEVALUE*POST	β_2			**0.0071**
				(0.280)
POST	β_3	-0.4892***	-0.4762***	-0.2571***
		(-17.171)	(-15.283)	(-10.416)
LNSIZE	β_4	-0.0537***	-0.0536***	-0.0551***
		(-4.840)	(-4.864)	(-4.910)
LNMAT	β_5	0.0795**	0.0767**	0.0749**
		(2.543)	(2.450)	(2.293)
LNTRADE	β_6	-0.0442***	-0.0442***	-0.0441***
		(-9.824)	(-9.770)	(-9.450)
LIABILITIES	β_7	0.0089	-0.0064	0.0637***
		(0.756)	(-0.464)	(8.892)
LN_ASSET_GEN	β_8	-0.5842***	-0.9080***	-1.4450***
		(-4.832)	(-6.549)	(-11.048)
POP	β_9	-0.0000***	-0.0000***	-0.0000***
		(-7.693)	(-6.573)	(-11.279)
CONSTANT	β_0	7.6517***	8.6675***	11.3581***
		(10.607)	(10.855)	(12.665)
Observations		14,665	14,665	14,094
$\beta_1 + \beta_2$		-2.817	-0.821	-0.431
F stat of $\beta_1 = \beta_1 + \beta_2$		0.000	0.000	0.780
Adj R^2		0.487	0.484	0.482