# Betting on the State: Sports Gambling Legalization and Municipal Borrowing Costs *

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**Abstract**

I study how sports betting legalization affects the borrowing costs of U.S. state governments. Exploiting the staggered adoption of online sportsbooks in a stacked difference-in-differences design with secondary market bond-month observations, I find that legalization lowers state issuers' yield spreads by 6 to 10 basis points, roughly 15 percent of the sample mean. This effect is robust across three alternative spread measures, holds under alternative control group definitions, and on a propensity-score-matched sample of bonds. Cross-sectional evidence points to fiscal capacity rather than sentiment: spreads fall about twice as much for general obligation bonds as for revenue bonds, the decline increases monotonically with maturity, and it is concentrated in bonds rated below AA and in states with poorly funded pensions, indicating that investors price legalization as a permanent expansion of the general tax base. The reduction scales with the size of each state's betting market and with the sports betting tax revenue the state actually collects. State tax revenue rises after legalization without an offsetting increase in state spending on health, police, welfare, or hospitals over the sample horizon. Bond investors thus reward a policy that other research shows harms household balance sheets, a tension that any welfare assessment of sin-tax revenue has to confront.

**Keywords:** Municipal Finance, Sports Betting, State Government Finance
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## 1. Introduction

Legalized sports betting has become one of the fastest-growing sources of state tax revenue in the United States. For most of the past three decades, sports betting was effectively prohibited outside Nevada under federal law. That changed in May 2018, when the U.S. Supreme Court struck down the Professional and Amateur Sports Protection Act (PASPA), returning authority over sports betting policy to individual states. Since then, 39 states and the District of Columbia have legalized sports betting in some form as of early 2026.[1] The economic scale of the market is now substantial: legal sportsbooks handled $166.9 billion in wagers in 2025, generated $16.96 billion in gross gaming revenue (GGR), and paid $3.71 billion in state taxes, a 32.4 percent year-over-year increase.[2] The rapid expansion of this market has attracted some empirical research on its adverse consequences for individuals and households (Baker, Balthrop, Johnson, Kotter, and Pisciotta, 2024; Couture, Cross, and Wu, 2024; Humphreys and Ruseski, 2024; Hollenbeck, Larsen, and Proserpio, 2025; Goss and Mangrum, 2026). However, it remains an open question whether these localized, micro-level harms aggregate into broader societal burdens or whether they are offset by systemic public benefits. Evaluating the net societal impact of legalization requires examining the other side of the ledger: the fiscal consequences for the state governments that chose to authorize it, and how capital markets assess those consequences.

This question sits at the intersection of public finance and asset pricing: does online sports betting legalization materially influence state fiscal capacity, and if so, do municipal bond investors price that influence? The U.S. municipal bond market, with $4.4 trillion in securities outstanding, is the primary channel through which state and local governments finance infrastructure, education, and public services. A well-established literature shows

[1] Sports Betting States: Latest US Legislation & Bill Tracker https://www.legalsportsreport.com/sports-betting-states/.

[2] US Gaming Revenue Reaches Record in 2025 https://www.americangaming.org/?5c22b0df_page=2&search=KYC&utm_source

that yield spreads on these bonds are sensitive to fiscal fundamentals. Default risk accounts for the majority of the average spread (Schwert, 2017), and fiscal variables, including revenue capacity, pension obligations, and debt levels, directly affect borrowing costs (Capeci, 1994; Bayoumi, Goldstein, and Woglom, 1995; Novy-Marx and Rauh, 2012). Recent work further demonstrates that discrete policy shocks are transmitted to municipal yields, whether through erosion of fiscal monitoring (Gao, Lee, and Murphy, 2020), public health legislation (Gao, Lee, and Murphy, 2022), the opioid crisis (Cornaggia, Hund, Nguyen, and Ye, 2022), or the legalization of marijuana (Cheng, De Franco, and Lin, 2023). However, whether sports betting legalization, a sin policy that simultaneously creates new tax revenue and may generate offsetting social costs, improves or worsens state borrowing conditions has not been studied.

The answer is not obvious. On the one hand, sports betting creates a new tax base for state governments: the U.S. Census Bureau reports that quarterly state sports betting tax collections rose 382 percent between 2021 and 2025.[3] On the other hand, a rapidly growing empirical literature emphasizes the household-level costs of legal sports betting: reduced savings and investment (Baker, Balthrop, Johnson, Kotter, and Pisciotta, 2024), lower credit scores and higher bankruptcy rates (Hollenbeck, Larsen, and Proserpio, 2025; Goss and Mangrum, 2026), adverse mental health effects (Couture, Cross, and Wu, 2024; Humphreys and Ruseski, 2024), increased gambling helpline calls (Taylor, McCarthy, and Wilbur, 2025), and elevated intimate partner violence (Arnesen and Matsuzawa, 2024). If these individual harms translate into higher demand for public services, the resulting expenditure growth could erode or reverse any fiscal benefit, as Cheng, De Franco, and Lin (2023) documents for medical marijuana. Whether the net fiscal effect of online sports betting legalization is positive or negative, and whether bond markets price it, is ultimately an empirical question.

This paper answers that question. I use the staggered adoption of online sports betting

[3]Quarterly Summary of State and Local Tax Revenue Shows Nationwide Surge in Sports Betting Revenue https://www.census.gov/library/stories/2025/12/sports-betting.html.

across U.S. states as a natural experiment and define treatment as the date on which the first licensed online sports betting application became publicly available in each state. To construct a comprehensive bond dataset, I obtain municipal bond trade data and merge it with Mergent, which provides different types of bond characteristics and issuer information, allowing me to identify the state issuer and FIPS codes for each CUSIP. Using a stacked difference-in-differences design following Cengiz, Dube, Lindner, and Zipperer (2019) with approximately 2.8 million secondary market bond-month observations, I estimate that legalization reduces MMA-adjusted yield spreads by 6.4 basis points, Treasury-adjusted spreads by 7.2 basis points, and tax-adjusted spreads by 9.4 basis points, all significant at the 1% level. These magnitudes are economically meaningful: relative to the mean MMA-adjusted spread of 43.7 basis points in my sample, the estimated effect represents a reduction of approximately 15 percent, comparable to effects documented for sea level rise (Goldsmith-Pinkham, Gustafson, Lewis, and Schwert, 2023), newspaper closures (Gao, Lee, and Murphy, 2020), renewable energy mandates (Cornaggia and Iliev, 2023) and the opioid crisis (Cornaggia, Hund, Nguyen, and Ye, 2022). In addition, event study estimates support the parallel trends assumption and reveal that the yield reduction emerges gradually over the quarters following launch. These results are robust to using not-yet-treated states as an alternative control group and to restricting the analysis to a propensity-score-matched sample of bonds with similar pre-treatment characteristics. They also survive clustering standard errors by state, randomization inference that reassigns launch dates across states, dropping each treated state in turn, excluding the COVID-19 period, and the estimator of Callaway and Sant'Anna (2021).

To explore the mechanism, I examine the fiscal channel using three complementary datasets. Annual data from the Census Bureau's Survey of State Government Finances shows that legalization increases per capita selective sales tax revenue by $54.9 per year, approximately $304 million for the average treated state. Quarterly Census data corroborate these magnitudes. Monthly gambling data from the Sports Handle further shows that

legalization increases annual handle by approximately $2.8 billion, gross sportsbook revenue by $232 million, and dedicated sports betting tax collections by $53 million per treated state. On the expenditure side, I find no significant change in state spending on health, police, welfare, or hospitals. The combination of higher revenue and unchanged expenditure implies a positive net fiscal effect, consistent with the observed decline in yield spreads and in direct contrast to medical marijuana, where Cheng, De Franco, and Lin (2023) find that expenditure growth leads to higher yield spreads. The size of the spread reduction tracks the size of the fiscal shock: it is three to four times larger in states whose first-year betting handle is above the median than in states below it, and each $100 per capita of sports betting tax collected since launch is associated with a further decline of 56 to 105 basis points. The reduction is concentrated in bonds rated below AA and in states with poorly funded pensions, while credit ratings, bond issuance, and other tax sources do not change over the sample period.

The setting offers several features that strengthen identification. First, the underlying legal change, the Supreme Court's invalidation of a federal statute, is exogenous to any individual state's fiscal or bond market conditions. Second, the staggered timing of online launch dates provides within-country variation while holding constant federal tax policy, monetary policy, and macroeconomic conditions. Third, sports betting tax revenue is collected and retained at the state level, aligning the unit of fiscal impact with the unit of bond issuance in my sample. Fourth, the distinction between online and retail-only legalization creates a sharp treatment boundary: online platforms generate over 90 percent of total handle, while retail-only states produce negligible tax revenue.

This paper contributes to three strands of literature. First, I contribute to the rapidly growing empirical literature on U.S. sports betting legalization by shifting the focus from household and social costs to state financing. Existing work primarily studies the individual consequences of legal sports betting, documenting adverse effects on savings, consumer

credit, mental health, and other indicators of household welfare (Baker, Balthrop, Johnson, Kotter, and Pisciotta, 2024; Hollenbeck, Larsen, and Proserpio, 2025; Goss and Mangrum, 2026; Couture, Cross, and Wu, 2024; Humphreys and Ruseski, 2024; Taylor, McCarthy, and Wilbur, 2025; Arnesen and Matsuzawa, 2024). A smaller literature considers the fiscal consequences of gambling more broadly and shows that new gambling activity need not translate one-for-one into new public revenue because of substitution across taxable activities (Walker and Jackson, 2011; Humphreys, 2021). I extend this literature by examining whether online sports betting legalization affects states' fiscal capacity in a way that is priced in municipal borrowing costs. In related work, Ellis and Powell (2026) study local-level fiscal outcomes using primary market issuance data. By contrast, I focus on the state level, where the fiscal shock from sports betting legalization operates most directly, and I examine secondary market yield spreads where investors continuously reprice bonds in response to new fiscal information.

Second, I contribute to the municipal bond pricing literature that links yields to policy shocks affecting expected revenues and expenditures. A large body of work documents how adverse fiscal developments raise municipal yields, including pension fund losses (Novy-Marx and Rauh, 2012), newspaper closures (Gao, Lee, and Murphy, 2020), the opioid crisis (Cornaggia, Hund, Nguyen, and Ye, 2022), and population aging (Butler and Yi, 2022). Turning to positive fiscal shocks, Gao, Lee, and Murphy (2022) find that the Affordable Care Act reduced healthcare bond yields, and Adelino, Cunha, and Ferreira (2017) show that exogenous credit rating upgrades lower borrowing costs. I conceptualize statewide online sports-betting legalization as a technology-enabled fiscal shock: it can expand the tax base through sportsbook GGR taxation and licensing, yet it may also generate offsetting expenditure pressures and liabilities. This suggests that bond markets price this sin policy through its net fiscal implications rather than through gross revenue growth alone.

Third, I provide empirical evidence that municipal bond markets price policy shocks

partly through revenue diversification and fiscal signaling channels. Previous literature argues that broadening the tax base reduces revenue volatility and fiscal risk (Seegert, 2015; Yan and Carr, 2019), while a related strand shows that fiscal institutions and policy choices affect borrowing costs by shaping investors' perceptions of a government's fiscal capacity (Poterba and Rueben, 2001; Basu, Naughton, and Wang, 2024). However, direct causal evidence linking the introduction of a new tax base to bond market repricing remains limited. Online sports betting legalization provides such a setting: staggered adoption across states generates plausibly exogenous variation in the timing of tax base expansion and the new revenue source exhibits low short-run sensitivity to the business cycle (Can and Nichols, 2022), making it an attractive setting for testing diversification-based predictions in the bond market.

The remainder of the paper is organized as follows. Section 2 describes the institutional background of U.S. sports betting legalization. Section 3 develops the hypotheses. Section 4 describes the data and sample construction. Section 5 presents the empirical strategy, results, and the mechanism analysis. Section 6 shows robustness tests. Section 7 concludes.

## 2. Institutional Background

For most of the past three decades, federal law effectively prohibited sports betting outside Nevada. The Professional and Amateur Sports Protection Act of 1992 (PASPA) barred states from sponsoring, operating, or authorizing betting schemes based on the outcomes of professional or amateur sporting events.

In 2012, New Jersey challenged PASPA through a series of federal lawsuits, arguing that the statute violated the Tenth Amendment by commandeering state legislatures. After losing twice at the Third Circuit, New Jersey petitioned the U.S. Supreme Court, which agreed to hear the case in June 2017. On May 14, 2018, the Court ruled 7–2 in *Murphy v. National*

*Collegiate Athletic Association* that PASPA's core prohibition was unconstitutional under the anti-commandeering doctrine, striking down the statute in its entirety. Writing for the majority, Justice Alito concluded that PASPA impermissibly dictated what state legislatures could and could not do and that Congress may regulate sports betting directly but cannot compel states to maintain their own prohibitions. The decision returned authority over sports betting policy to individual states.

States moved quickly. Delaware accepted the first legal single-game wager outside Nevada on June 5, 2018, followed by New Jersey on June 14. By year-end, seven states had operational markets. As of early 2026, 39 states and the District of Columbia have legalized sports betting in some form, with approximately 31 of these permitting statewide online or mobile wagering. The remaining legal states restrict betting to retail locations at casinos or tribal facilities. Eleven states have not legalized sports betting, including California, Texas, and Georgia.

The sports gambling market has grown rapidly. Total handle (amount wagered) rose from approximately $6.6 billion during the partial 2018 season to $166.9 billion in 2025. In 2025, legal sportsbooks generated $16.96 billion in gross gaming revenue (GGR) and paid $3.71 billion in state taxes, reflecting a 32.4 percent year-over-year increase. States tax sports betting GGR at rates ranging from 6.75% in Nevada and Iowa to 51% in New York, New Hampshire, and Rhode Island.[4] In addition, the U.S. Census Bureau reports that quarterly state sports betting tax collections rose 382%, from $190 million in the third quarter of 2021 to $917 million in the second quarter of 2025.

A defining feature of the market is the dominance of online and mobile platforms. In states that permit mobile wagering, online channels account for over 90% of total handle.[5] New York illustrates this pattern: the state launched retail-only sports betting at four upstate

[4]Tax Foundation, "Online Sports Betting Taxes, 2025," https://taxfoundation.org/data/all/state/online-sports-betting-taxes/.

[5]US sports betting: The state of play https://igamingbusiness.com/sports-betting/sportsbook/h2-us-sports-betting-2/

casinos in July 2019, generating modest activity, then opened online betting on January 8, 2022. Monthly handle rose from approximately $21 million in December 2021 to over $1.6 billion in January 2022, and online channels now account for 98% of all wagering in the state.[6] Since the online launch, New York has collected over $3.4 billion in cumulative sports betting tax revenue at a 51% rate on GGR.[7] The contrast with retail-only states is stark: South Dakota, which has not authorized online betting, collected just $0.15 million in total sports betting revenue in 2024.[8] Because online legalization generates substantially more betting activity and tax revenue than retail-only authorization, I define treatment as the date on which the first licensed online sports betting application became publicly available in each state. Moreover, since sports-betting tax revenues are predominantly collected and retained at the state level, the fiscal effects of legalization operate primarily through state government budgets. Accordingly, I will focus on state-level municipal bond issuers in this paper. Figure 1 displays the current legalization status across states, distinguishing between states that permit both online and retail wagering, states with retail or limited access only, and states that have not legalized.

[Figure 1 about here]

## 3. Hypothesis Development

This section develops hypotheses about how online sports betting legalization affects municipal bond yield spreads. I consider three channels through which legalization may reduce borrowing costs and one countervailing force that could increase them.

[6] New York State Gaming Commission data; PlayNY, "New York Sports Betting: Market and Tax Revenue Projections," https://www.playny.com/sports-betting/revenue/.

[7] The Motley Fool, "Sports Betting Tax Revenue by State," https://www.fool.com/research/sports-betting-tax-revenue-by-state/.

[8] US Sports Betting Revenue & Handle Tracker by State: 2018–2026 https://www.gamingtoday.com/revenue/

### 3.1. Yield-Reducing Channels

**Fiscal capacity.** Municipal bond spreads are dominated by credit risk: Schwert (2017) finds that default risk accounts for 74 to 84 percent of the average spread after tax adjustment. Because credit risk reflects the issuer's ability to pay back debt, changes in state fiscal fundamentals should produce first-order effects on yield spreads. Novy-Marx and Rauh (2012) quantify this link through the 2008 pension fund losses and estimate that spreads rose 7 to 15 basis points for each 10 percent of state-generated revenue lost. Capeci (1994) and Bayoumi, Goldstein, and Woglom (1995) provide further evidence that fiscal variables directly affect borrowing costs, with the latter showing that the relation between borrowing costs and fiscal conditions is nonlinear.

Whether legalization reduces borrowing costs depends not only on the new tax revenue it generates but also on whether it triggers higher government spending. Two closely related studies illustrate how the direction of the yield effect depends on the net fiscal impact. Gao, Lee, and Murphy (2022) find that the 2012 Supreme Court ruling upholding the Affordable Care Act reduced healthcare municipal bond yields by approximately 8 basis points, as the positive revenue effects from expanded insurance coverage dominate the offsetting cost and reimbursement pressures. In contrast, Cheng, De Franco, and Lin (2023) find that medical marijuana legalization increases state borrowing costs by 7 to 9 basis points, because marijuana causes high spending on law enforcement and public welfare, even though marijuana consumption brings in more tax revenue. This suggests that if sports betting legalization improves a state's net fiscal position—tax revenue rises without a corresponding increase in expenditure—then borrowing costs should fall.

**Revenue diversification.** Beyond the level of new revenue, its risk properties matter. Bond investors care about revenue volatility because shortfalls in adverse economic states are precisely when debt service is hardest to sustain. Seegert (2015) formalizes this intuition

using a tax-portfolio framework: states can reduce revenue volatility by diversifying into tax bases with low covariance to existing sources. Yan and Carr (2019) and Jimenez and Afonso (2022) confirm that broadening the revenue base into sources with distinct economic drivers improves state fiscal health.

This mechanism can be illustrated by modeling total state revenue as a portfolio of $N$ tax instruments with aggregate variance $\sigma_R^2$.[9] Adding a new source with cyclical sensitivity $\beta^{\text{SB}}$ changes total revenue variance by

$$\Delta\sigma^2 = 2B\,\beta^{\text{SB}}\sigma_\theta^2 + (\beta^{\text{SB}})^2\sigma_\theta^2 + \sigma_{\epsilon^{\text{SB}}}^2, \tag{1}$$

where $B$ is the aggregate cyclical exposure of existing taxes and $\sigma_\theta^2$ is the variance of the economic state. Can and Nichols (2022) estimate that sports betting gross revenue in Nevada is stable and insensitive to short-run income shocks, consistent with $\beta^{\text{SB}} \approx 0$ over the time horizons relevant to this study. Under this condition, the covariance term vanishes, and the variance increase is limited to $\sigma_{\epsilon^{\text{SB}}}^2$. For a revenue stream that represents less than one percent of the total state budget, this contribution is negligible. Expected revenue increases while volatility changes little, reducing the probability of fiscal distress and lowering yield spreads.

**Fiscal signaling.** Because municipal bond investors price fiscal news, policy changes that signal an expansion of future fiscal capacity can affect yields even before the full revenue consequences are realized. Poterba and Rueben (2001) show that unexpected deficits are associated with higher state bond yields and that this sensitivity depends on fiscal institutions, suggesting that bond markets respond to fiscal shocks that are informative about a state's underlying fiscal capacity. Basu, Naughton, and Wang (2024) further show that state government trifectas reduce municipal bond yields by 4 to 20 basis points, consistent with investors pricing the political capacity to implement fiscal policies.

[9] A full derivation is provided in Appendix A.

In this context, the legalization of sports gambling may serve as a positive fiscal signal. Even if short-term tax collections are modest, the policy creates a new taxable base and may be interpreted as evidence of greater revenue capacity and diversification. External estimates suggest that broader sports gambling legalization could generate billions of dollars in additional annual state tax revenue, reinforcing the view that legalization is informative about future fiscal resources.[10]

**Hypothesis 1.** *Online sports betting legalization reduces secondary market yield spreads through fiscal capacity enhancement, revenue diversification, and fiscal signaling when the net fiscal impact is positive.*

### 3.2. Social Costs as a Countervailing Force

Gambling legalization also carries social costs that could offset the fiscal benefit as a highly immersive activity (Williams et al., 2011; Mancini et al., 2025). Recent work documents household-level harm: Baker, Balthrop, Johnson, Kotter, and Pisciotta (2024) find that sports betting significantly reduces personal savings and investment; Hollenbeck, Larsen, and Proserpio (2025) estimate that online legalization lowers average credit scores by approximately 2.75 points and increases bankruptcy rates by 10 percent within three to four years; and Taylor, McCarthy, and Wilbur (2025) document a 75 percent increase in gambling helpline calls post-legalization. If this financial distress translates into higher demand for public services— Medicaid, welfare, law enforcement, mental health treatment—then government expenditure rises, eroding the net fiscal benefit. A similar mechanism is documented by Cheng, De Franco, and Lin (2023) for marijuana, where increased spending on police, corrections, and welfare more than offset new tax revenue.

**Hypothesis 2.** *The social costs of sports betting legalization may increase government expenditure and raise municipal bond yield spreads.*

[10]Expanded Sports Betting Legalization Would Generate Billions in Tax Revenue , https://taxfoundation.org/research/all/state/expanded-legalization-sports-betting-tax-revenue/

### 3.3. Gradual Repricing

Standard asset-pricing theory predicts that if legalization has a significant fiscal impact, efficient markets would incorporate it immediately into yields. However, the municipal bond market is characterized by severe information frictions. Cestau, Hollifield, Li, and Schürhoff (2019) describes the municipal bond market as a decentralized and opaque OTC market with limited transparency, substantial fragmentation, low turnover, and significant illiquidity. Transaction costs far exceed those of comparably sized equity trades (Harris and Piwowar, 2006), and dealer intermediation introduces substantial price dispersion (Schultz, 2012). These frictions create a slow-moving capital environment in which prices adjust to fundamental shocks with a delay.

Two additional forces reinforce gradual repricing. First, sports betting revenue accumulates as markets mature—handle and gross gaming revenue exhibit strong growth in the years following launch, so the fiscal signal strengthens over time. Second, revenue data arrive at a monthly frequency as states publish regulatory reports, providing a sequence of information that investors need to incorporate progressively.

**Hypothesis 3.** *The yield effect of legalization emerges gradually over the quarters following launch, reflecting information frictions in the municipal bond market and the progressive accumulation of sports betting revenue or cost.*

## 4. Data and Sample Construction

I construct the dataset for this paper by merging municipal bond transaction records with state-level sports betting legalization dates, benchmark yield curves, and fiscal data from multiple government sources. This section describes each data source and the steps taken to form the final samples for the secondary market and the state government finance analysis.

### 4.1. Municipal Bond Data

Secondary market municipal bond trades come from the Municipal Securities Rulemaking Board (MSRB), which collects virtually all broker-dealer transactions in the U.S. municipal bond market from January 2005 through February 2025. I merge these transaction records with bond-level characteristics from the Mergent Municipal Bond Securities Database (Mergent FISD) by CUSIP and keep only bonds that appear in both databases. Mergent provides bond characteristics, including the coupon rate, maturity date, settlement date, offering yield, offering amount, offering type, and indicators for callable, insured, and general obligation, as well as issuer name and state of issuance. I exclude bonds issued in U.S. territories and retain only the 50 states and the District of Columbia.

I construct a composite credit rating for each bond at the time of trade by choosing the minimum long-term rating across Moody's, Standard & Poor's, and Fitch from Mergent's historical rating files. I assign numerical values to ratings from 1 (D/C) to 21 (AAA/Aaa). Because treatment and tax revenue in this study operate at the state level, I restrict both samples to state-level issuers, identified by matching Mergent issuer names to the MSRB's Electronic Municipal Market Access (EMMA) system and supplemented by manual additions.[11] County FIPS codes for each CUSIP are obtained from Bloomberg based on the procedures in Cornaggia, Hund, Nguyen, and Ye (2022), with remaining cases resolved through a three-round matching procedure using large language models.

For the secondary market sample, I collapse the transaction data to the bond-year-month level by computing volume-weighted averages of yield with par value traded as the weight, following Gao, Lee, and Murphy (2020). I drop missing or non-positive yields and restrict the sample to 2016 through 2024. The final secondary market dataset contains approximately 2.8 million bond-year-month observations.

[11] Municipal securities issuer by state, https://emma.msrb.org/IssuerHomePage/Map

### 4.2. Yield Spread Construction

I construct three measures of yield spreads, each capturing a different dimension of municipal bond pricing.

**MMA-adjusted spread.** The first measure subtracts the maturity-matched yield from the Municipal Market Advisors (MMA) AAA-rated yield curve, obtained from Bloomberg. The MMA curve provides daily par yields for maturities of 1 through 30 years and is widely used as the benchmark for high-grade municipal debt.[12] I merge the MMA yield to each bond observation by matching on the trade date and years to maturity, rounded to the nearest integer and bounded between 1 and 30. The MMA-adjusted spread for bond $i$ observed at time $t$ is

$$\text{Spread}_{it}^{\text{MMA}} = y_{it} - y_t^{\text{MMA}}(\tau_{it}), \tag{2}$$

where $y_{it}$ is the yield of bond $i$ at time $t$ and $y_t^{\text{MMA}}(\tau_{it})$ is the MMA yield at maturity $\tau_{it}$ on date $t$.

**Treasury-adjusted spread.** The second measure subtracts the maturity-matched yield from the U.S. Treasury zero-coupon yield curve estimated by the method of Filipović, Pelger, and Ye (2025), obtained from the U.S. Treasury Discount Bond Database. This curve provides daily continuously compounded yields at monthly maturity increments from 1 to 360 months. I match each bond observation by trade date and months to maturity, rounded to the nearest integer and bounded between 1 and 360. The Treasury-adjusted spread for bond $i$ observed at time $t$ is

$$\text{Spread}_{it}^{\text{Treasury}} = y_{it} - y_t^{\text{Treasury}}(\tau_{it}), \tag{3}$$

where $y_t^{\text{Treasury}}(\tau_{it})$ is the Treasury zero-coupon yield at maturity $\tau_{it}$ on date $t$.

[12] The MMA curve is sometimes referred to as the Municipal Market Data (MMD) curve in earlier literature.

**Tax-adjusted spread.** The third measure accounts for the tax-exempt status of municipal bond interest. Following Garrett, Ordin, Roberts, and Suárez Serrato (2023) and Zimmerschied (2024), I compute the taxable-equivalent yield by dividing the municipal bond yield by $(1 - \tau_{s,t}^{\text{fed}})(1 - \tau_{s,t}^{\text{state}})$, where $\tau_{s,t}^{\text{fed}}$ and $\tau_{s,t}^{\text{state}}$ are the top marginal federal and state income tax rates applicable to state $s$ in year $t$, and then subtract the Treasury yield:[13]

$$\text{Spread}_{it}^{\text{Tax}} = \frac{y_{it}}{(1 - \tau_{s,t}^{\text{fed}})(1 - \tau_{s,t}^{\text{state}})} - y_t^{\text{Treasury}}(\tau_{it}). \tag{4}$$

All three spread measures are expressed in basis points throughout the paper.

### 4.3. Treatment: Online Sports Betting Legalization

I define treatment based on the date when the first licensed online sports betting application became publicly available in each state, rather than the date of legislative passage. This choice reflects the fact that the fiscal effects of legalization, which I argue constitute the primary channel through which sports betting affects municipal bond yields, begin only when betting platforms are operational and generating tax revenue. Figure 2 plots the staggered adoption timeline. As of December 2025, 34 states and the District of Columbia have operational online sports betting markets.

[Figure 2 about here]

### 4.4. State-Level Controls

State population and per capita personal income are from the U.S. Bureau of Economic Analysis. State unemployment rates are from the U.S. Bureau of Labor Statistics. All state-level control variables enter the regressions as one-year lagged values indexed by the year

[13] For states that exempt their own municipal bond interest from state income tax, the state tax adjustment is omitted. These states are Connecticut, Illinois, Iowa, Kansas, Oklahoma, and Wisconsin.

before the observation date, ensuring that the controls are predetermined relative to the outcome.

### 4.5. State Government Finance Data

I form three balanced fiscal datasets for the mechanism analysis. All fiscal variables are expressed in per capita terms using the contemporaneous state population.

**Annual state finances (ASFIN).** The Census Bureau's Annual Survey of State Government Finances provides detailed state-level revenue and expenditure data from 2013 to 2024 for all 50 states. On the revenue side, I focus on selective sales tax revenue and its subcomponents, including the pari-mutuel and sports betting tax category. The Census Bureau reported pari-mutuel taxes as a standalone item (code T14) through fiscal year 2022 and introduced a separate sports betting & pari-mutuel tax item (code T18) beginning in fiscal year 2023. I combine these two items into a single measure to maintain consistency across the sample period. On the expenditure side, I examine several spending measures such as health, police, public welfare, and hospital expenditure.

**Quarterly state tax revenue (QTAX).** The Census Bureau's Quarterly Summary of State and Local Government Tax Revenue provides state-level tax collections at a quarterly frequency from 2013Q1 through 2025Q4 for all 50 states and the District of Columbia. I extract selective sales tax revenue, pari-mutuel and sports betting taxes, and amusement taxes, applying the same T14/T18 harmonization procedure described above.

**Monthly sports betting market data.** I manually collect monthly state-level data on total sports betting handle, gross sportsbook revenue (GGR), and state tax collections from the Sports Handle, which aggregates data from individual state regulatory commission re-

ports. [14] The raw data cover 35 states from their respective launch dates through December 2025. For states without legal sports betting, I set handle, revenue, and tax collections to zero for all months, to create a balanced panel of 51 jurisdictions from June 2013 through December 2025.

Figure 5 displays the geographic distribution of cumulative state tax revenue from sports betting through 2025, showing substantial cross-state variation that reflects differences in legalization timing, tax rates, and market size.

[Figure 5 about here]

### 4.6. Summary Statistics

Table 1 reports summary statistics for the secondary market sample. The average MMA-adjusted spread is 43.7 basis points with a standard deviation of 103.5 basis points. The average Treasury-adjusted spread is 5.4 basis points, reflecting the tax advantage of municipal bonds relative to Treasuries. The average tax-adjusted spread is 164.5 basis points. The typical bond in the sample carries a coupon of 4.4%, has 9.1 years to maturity, and was issued with an offering amount of approximately $8.6 million (the mean of log offering amount is 16.0). Fifty-eight percent of bonds are callable, 7% are insured, and 21% are general obligation bonds. The average credit rating is 17.9 (approximately AA), indicating that the sample consists primarily of high-grade state issuers.

[Table 1 about here]

Table 2 presents summary statistics for the state government finance samples used in the mechanism analysis. Panel A describes the annual ASFIN sample, and Panel B describes

[14] Legal US Sports Betting Revenue, Handle, and Tax Totals Since PASPA Repeal , https://sportshandle.com/sports-betting-revenue/

the quarterly QTAX sample. Average selective sales tax revenue is $512 per capita annually and $138 per capita quarterly. The mean tax revenue for pari-mutuel and sports is $2.03 per capita annually with a standard deviation of 5.69. The large standard deviation relative to the mean reflects the substantial cross-state variation introduced by the staggered legalization of online sports betting markets over the sample period. On the expenditure side, average per capita spending on welfare ($2,211), hospitals ($278), health ($224), and police ($64) indicates that welfare spending constitutes the largest expenditure category among those examined.

[Table 2 about here]

## 5. Empirical Strategy and Results

### 5.1. Municipal Bond Yield Spreads

I estimate the causal effect of online sports betting legalization on municipal bond yield spreads using a stacked difference-in-differences design. Standard two-way fixed effects estimators can produce biased estimates under staggered treatment adoption when the treatment effect varies over time (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Baker et al., 2022). Following Cengiz, Dube, Lindner, and Zipperer (2019), I construct cohort-specific datasets, each pairing a single treatment cohort with a clean control group, and stack them into a pooled regression:

$$\begin{aligned} Spread_{i(s),t(y,c)} = \alpha + \beta \cdot Sports\ Gambling_{s,t} + Bond\ Controls_{i,t} \\ + State\ Controls_{s,y-1} + State\text{-}Cohort\ FE_{s,c} \\ + Year\text{-}Month\text{-}Cohort\ FE_{t,c} + \varepsilon_{i,t} \end{aligned} \tag{5}$$

where $i$ indexes bond, $s$ indexes state, $t$ indexes year-month, $c$ indexes cohort, and $y$ indexes year. $Spread_{i(s),t(y,c)}$ is one of the three yield spread measures defined in Section 4.2,

expressed in basis points. *Sports Gambling*$_{s,t}$ equals one if state $s$ has an active online sports betting market in month $t$. The coefficient $\beta$ captures the average effect of online sports betting legalization on municipal borrowing costs. *Bond Controls*$_{i,t}$ includes coupon rate, log offering amount, years to maturity, and indicators for callable, insured, general obligation, tax-exempt, and negotiated offering. *State Controls*$_{s,y-1}$ includes one-year lagged log state population, per capita income, and unemployment rate. Standard errors are double-clustered at the bond and year-month level.

Treatment cohorts are defined by the calendar month of each state's online launch date. For each cohort, I keep observations within a symmetric 30-month window around the launch date. Three dependent variables are winsorized at the 1st and 99th percentiles within each cohort. The baseline control group consists of never-treated states; not-yet-treated states serve as an alternative control group in the robustness analysis. Nevada is excluded as an always-treated unit.

Table 3 reports the results for the secondary market sample. Across all three spread measures and both specifications, the coefficient on *Sports Gambling* is negative and significant at the 1% level. In the specification with the full set of controls, legalization reduces the MMA-adjusted spread by 6.4 basis points (Column 2), the Treasury-adjusted spread by 7.2 basis points (Column 4), and the tax-adjusted spread by 9.4 basis points (Column 6). The estimates are stable across specifications: the corresponding coefficients without controls are $-7.1$, $-8.0$, and $-10.9$ basis points, indicating that the results are not driven by observable bond or state characteristics. Relative to the mean MMA-adjusted spread of 43.7 basis points, the estimated effect of 6.4 basis points represents a reduction of approximately 15%.

[Table 3 about here]

The magnitude of the estimated effect is comparable to those documented in related settings: Cheng, De Franco, and Lin (2023) find that medical marijuana legalization increases

secondary market spreads by approximately 10 basis points, Gao, Lee, and Murphy (2020) estimate a 6.4 basis point increase following newspaper closures, and Cornaggia, Hund, Nguyen, and Ye (2022) report a 16 basis point increase in counties severely affected by the opioid crisis.

### 5.2. Event Study Evidence

To examine the dynamics of the treatment effect and assess the parallel trends assumption, I estimate the following event study model:

$$\begin{aligned} Spread_{i(s),t(y,c)} = & \sum_{q \neq -1} \beta_q \cdot \mathbf{1}[Bin_{s,t} = q] + Bond\ Controls_{i,t} \\ & + State\ Controls_{s,y-1} + State\text{-}Cohort\ FE_{s,c} \\ & + Year\text{-}Month\text{-}Cohort\ FE_{t,c} + \varepsilon_{i,t} \end{aligned} \tag{6}$$

where $Bin_{s,t}$ assigns each treated-state observation to a three-month bin based on the number of months between the observation date and the state's online launch date; control-group observations are coded as zero for all bins. I define five pre-treatment bins and five post-treatment bins, with tail bins absorbing observations beyond $\pm 16$ months. The omitted category is the bin spanning months $-3$ through $-1$ immediately before launch. All other notation follows Equation (5). The coefficients $\{\beta_q\}_{q<-1}$ on pre-treatment bins should be insignificantly different from zero if the parallel assumption holds.

Figure 6 presents the estimates for the secondary market with never-treated states as the control group. Two features are apparent. First, the pre-treatment coefficients are close to zero and statistically insignificant across all three spread measures, consistent with the parallel trends assumption. Second, the post-treatment coefficients turn negative and grow in magnitude over the quarters following legalization, consistent with a gradual increase in fiscal benefits as betting markets mature and tax revenues accumulate. This gradual adjustment is also consistent with the well-documented information frictions in the municipal bond market,

including large transaction costs, opaque OTC trading (Harris and Piwowar, 2006; Cestau, Hollifield, Li, and Schürhoff, 2019), and fragmented dealer intermediation (Li and Schürhoff, 2019) discussed in Hypothesis 3. In this environment, investors need to incorporate this information gradually as the fiscal consequences of legalization unfold over time. By the fourth and fifth post-treatment quarters, the estimates stabilize at levels broadly consistent with the pooled coefficient from Table 3.

[Figure 6 about here]

### 5.3. Heterogeneity by Bond Maturity

To further examine how municipal bond investors price the fiscal impact of sports betting legalization, I partition the secondary market sample into three maturity buckets: short-term (0–5 years), medium-term (5–15 years), and long-term (15–30 years). If the observed reduction in yield spreads is driven by expectations of a permanent expansion in the state's tax base, the effect should be most pronounced for longer-dated securities that are more sensitive to the long-term fiscal outlook.

Table 4 presents the results. Across all three spread measures, the reduction in borrowing costs is strictly increasing in bond maturity. For the tax-adjusted spread (Panel C), the coefficients move from $-6.58$ basis points for the 0–5 year bucket to a much more substantial $-11.74$ basis points for the 15–30 year bucket.

[Tables 4 about here]

This pattern is consistent with the market's pricing of the growth curve and long-term stability of sports betting tax revenues. As a novel fiscal instrument, sports betting markets require a maturation period characterized by platform rollouts, increasing user penetration, and regulatory stabilization. Consequently, the immediate marginal contribution of these

taxes to a state's general fund during the initial "ramp-up" phase (typically the first 1–5 years) is relatively modest.

However, market participants anticipate that in the long run (15–30 years), sports betting will evolve into a substantial and highly stable revenue stream, providing a structural buffer to state finances. Because long-term bonds represent a claim on a state's distant fiscal capacity, they price in these "long-term future cash flow improvements" more fully than short-term bonds, which are primarily influenced by immediate fiscal conditions. This evidence confirms that investors view sports betting legalization not as a transient windfall, but as a permanent, technology-driven expansion of the state's long-term revenue capacity.

### 5.4. Heterogeneity by Bond Type

Table 5 re-estimates the baseline specification separately for Revenue bonds and General Obligation (GO) bonds. The results reveal a stark divergence: the spread reduction for GO bonds is approximately twice as large as that for Revenue bonds. Using the tax-adjusted measure (Columns 5 and 6), the spread on GO bonds falls by 15.97 basis points, whereas Revenue bonds experience a decline of 8.45 basis points.

[Tables 5 about here]

This differential provides compelling mechanism-driven evidence for the fiscal capacity channel. GO bonds are backed by the issuer's full faith and credit and general taxing authority (Capeci, 1994). Because sports betting tax receipts predominantly flow into state general funds or designated broad-based public budgets, GO bonds are the direct beneficiaries of the expanded tax base. Conversely, Revenue bonds are ring-fenced and serviced exclusively by cash flows from specific enterprise projects, such as toll roads or utility systems (Gao et al., 2020). While Revenue bonds still experience a significant decrease in spreads—likely reflecting positive macroeconomic spillovers and a stronger implicit state guarantee during

fiscal distress—their structural insulation from general tax revenues naturally attenuates the direct pricing impact.

### 5.5. Heterogeneity by Credit Rating and Tax Status

If investors lower the spreads they demand because they view the state as less likely to run into fiscal trouble, the reduction should be concentrated in the bonds that carry the most credit risk to begin with. Bonds already rated AA or better trade close to the benchmark curve and have little default premium left to compress. Table 11, Panel A, splits the sample by the composite rating at the time of trade. For bonds rated below AA, legalization lowers the MMA-adjusted spread by 9.95 basis points, the Treasury-adjusted spread by 10.54 basis points, and the tax-adjusted spread by 12.65 basis points, all significant at the 1% level. Bonds without a rating from any agency also see their spreads fall, by 4.75, 5.98, and 7.34 basis points. For bonds rated AA and above, the coefficients are small and positive, between 2.7 and 4.3 basis points. The full-sample estimate in Table 3 therefore reflects a repricing of the riskier segment of each state's outstanding debt rather than a uniform shift across all bonds.

[Table 11 about here]

Panel B separates tax-exempt bonds from the smaller set of taxable municipal bonds. The spread reduction is confined to tax-exempt bonds: $-6.41$, $-7.31$, and $-9.49$ basis points across the three measures, each significant at the 1% level, compared with statistically insignificant coefficients of $-0.98$, 0.31, and 1.65 basis points for taxable bonds. Taxable municipal bonds tend to be held by institutional and foreign investors who do not benefit from the exemption, and they often finance activities that do not qualify for it, so the two segments have different investor bases. The pattern is consistent with the repricing being concentrated in the segment where in-state investors, who follow state fiscal news most

closely, are the marginal holders. Table 22 in the Appendix reports additional splits by call provision, insurance, bond age, and issue size; the effect is larger for non-callable, uninsured, more recently issued, and larger issues, but it is negative and significant in every subsample except the small group of insured bonds under the tax-adjusted measure.

### 5.6. Mechanism: State Government Finances

I next examine whether sports betting legalization improves state fiscal positions in a manner consistent with the observed decline in secondary market spreads. The hypothesis is that legalization generates new tax revenue, improving the fiscal outlook of legalizing states and reducing the compensation bond investors demand. To test this, I estimate a stacked difference-in-differences specification at the state level:

$$\begin{aligned} \textit{Fiscal Outcome}_{s,t,c} = \alpha + \beta \cdot \textit{Sports Gambling}_{s,t} + \textit{State Controls}_{s,y-1} \\ + \textit{State-Cohort FE}_{s,c} + \textit{Time-Cohort FE}_{t,c} + \varepsilon_{s,t} \end{aligned} \tag{7}$$

where the unit of observation is a state-period (year, quarter, or month depending on the dataset) within a cohort-specific sample. The construction of the stacked samples and the definitions of the fixed effects are similar to those in Equation (5). Standard errors are clustered at the state level. All fiscal outcomes are measured in per capita terms. I examine both the selective sales tax revenue, which includes sports betting alongside other sin taxes such as alcohol and tobacco and thus captures potential spillover effects, and the pari-mutuel and sports betting tax category in isolation.

Table 6 reports estimates using annual data from ASFIN, restricted to a five-year window around the launch date. Legalization increases per capita selective sales tax revenue by \$54.9 per year (Column 2, significant at the 1% level) and per capita pari-mutuel and sports betting tax revenue by \$2.9 per year (Column 4, significant at the 5% level). The selective sales tax estimate implies around \$304 million in additional annual revenue for the average treated

state.

[Table 6 about here]

Table 7 provides corroborating evidence at the quarterly frequency. Selective sales tax revenue rises by $13.0 per capita per quarter (Column 2) and pari-mutuel and sports betting tax revenue by $1.2 per capita per quarter (Column 4), both significant at the 5% level. The quarterly magnitudes are broadly consistent with the annual estimates from ASFIN after accounting for seasonal variation in betting activity.

[Table 7 about here]

Table 8 reports estimates using monthly data on sports betting market activity. Legalization increases monthly per capita handle by $42.3 (Column 4), gross sportsbook revenue by $3.5 (Column 6), and state gambling tax collections by $0.8 (Column 2), all significant at the 1% level. Annualizing these estimates implies approximately $2.8 billion in handle, $232 million in gross sportsbook revenue, and $53 million in sports betting tax revenue per year for the average treated state.[15]

[Table 8 about here]

To examine the dynamics of the fiscal response, I estimate an event-study analog of Equation (7):

$$\begin{aligned} \textit{Fiscal Outcome}_{s,t,c} = & \sum_{q \neq -1} \beta_q \cdot \mathbf{1}[\textit{Bin}_{s,t} = q] + \textit{State Controls}_{s,y-1} \\ & + \textit{State-Cohort FE}_{s,c} + \textit{Time-Cohort FE}_{t,c} + \varepsilon_{s,t} \end{aligned} \tag{8}$$

[15] Dollar amounts are obtained by multiplying the per capita coefficient by 12 months and by the average population of the treated states (5.53 million).

where $Bin_{s,t}$ assigns each treated-state observation to a relative time bin. For the ASFIN sample, bins correspond to individual years relative to the launch year, with the year immediately prior ($q = -1$) omitted as the reference category. For the QTAX sample, bins correspond to individual quarters. For the monthly Sports Handle data, I aggregate relative months into three-month bins to reduce noise. In each case, the omitted category is the period immediately before the online launch date. All other notation follows Equation (7).

Figures 7, 8, and 9 present the corresponding event-study estimates for ASFIN annual tax revenue, QTAX quarterly tax revenue, and monthly handle, gross revenue, and gambling tax collections, respectively. Across all three datasets, the pre-treatment coefficients are close to zero, and the post-treatment coefficients increase over time, consistent with the progressive ramp-up in betting activity and tax collections as markets mature.

[Figures 7, 8, and 9 about here]

A concern is that the fiscal benefits of legalization may be offset by increased social costs. Recent work shows that legal sports betting is associated with increased consumer debt and financial distress among households (Baker et al., 2024; Hollenbeck et al., 2025). Similarly, Cheng, De Franco, and Lin (2023) find that medical marijuana legalization, another policy involving a potentially addictive activity, leads to higher state spending on police, corrections, and public welfare, suggesting that the social costs of such policies can be fiscally significant.

Table 9 tests this possibility directly. The point estimates on health, police, welfare, and hospital expenditure are −5.9, 4.9, 24.7, and −34.0 dollars per capita, respectively (Columns 2, 4, 6, and 8), all statistically insignificant and small relative to their sample means. This null result suggests that the social costs documented at the household level have not translated into higher state government spending over the sample horizon, providing no support for Hypothesis 2.

[Table 9 about here]

However, the expenditure response may be delayed or limited within the time horizon of the sample. Grinols and Mustard (2006) find that crime effects of casino gambling take three or more years to emerge fully, and the bankruptcy effects in Hollenbeck, Larsen, and Proserpio (2025) peak at three to four years. Williams, Rehm, and Stevens (2011) document that gambling's relationship with crime is not straightforward, with evidence pointing in different directions depending on the form of gambling and local context.

A second concern on the revenue side is that sports betting may draw spending away from other taxed activities, so that the new collections partly replace rather than add to existing revenue. Walker and Jackson (2011) and Humphreys (2021) document such substitution for casino and lottery gambling. Table 10 estimates Equation (7) for the tax sources most likely to be affected. In the annual ASFIN data (Panel A), per capita alcohol tax revenue changes by \$0.04 and tobacco tax revenue by \$2.83 after legalization, neither distinguishable from zero, and general sales tax revenue and net lottery revenue are likewise unchanged. The quarterly QTAX data (Panel B) give the same answer for alcohol, tobacco, general sales, and individual and corporate income taxes. Within the sample horizon, the sports betting tax appears to be a net addition to state revenue rather than a reshuffling of existing collections.

[Table 10 about here]

Taken together, these results support a coherent fiscal mechanism. Online sports betting legalization generates substantial new betting activity, which is associated with higher state tax revenue, including both selective sales tax revenue and dedicated sports betting tax collections. By contrast, state expenditure does not increase significantly over the sample horizon. This positive net fiscal effect is consistent with the observed decline in secondary-market yield spreads, suggesting that bond market participants price in the improved fiscal capacity of legalizing states and therefore require lower compensation to hold their bonds. Overall, these estimates are consistent with Hypothesis 1, which predicts that online sports betting legalization lowers municipal borrowing costs when the net fiscal effect is positive.

### 5.7. Treatment Intensity and Realized Tax Revenue

The binary treatment indicator in Equation (5) treats every online launch alike, but the markets that opened were far from alike. Average monthly handle over the first year after launch ranged from $3.6 per capita in Oregon to $33.4 per capita in Colorado, and first-year tax collections ranged from $0.05 per capita in Iowa to $0.97 per capita in the District of Columbia. If the spread reduction reflects the fiscal value of the new market, it should be larger where the market is larger. Table 12 tests this by interacting the treatment indicator with standardized measures of first-year handle and first-year tax collections in each treated state, computed from the monthly Sports Handle data.

[Table 12 about here]

The interaction terms are negative and significant for all three spread measures. A one standard deviation increase in first-year per capita handle across treated states is associated with an additional reduction of 3.44 basis points in the MMA-adjusted spread, 3.96 basis points in the Treasury-adjusted spread, and 4.24 basis points in the tax-adjusted spread (Panel A). Measured by first-year tax collections, the corresponding increments are 4.76, 4.16, and 9.31 basis points (Panel B). Splitting treated states at the median of first-year handle gives the same picture in a simpler form (Panel C): spreads fall by 9.43, 10.13, and 13.33 basis points in the six high-handle states but by only 2.36, 3.11, and 4.04 basis points in the six low-handle states, and the difference between the two groups is significant at the 5% level or better for each spread measure.

Table 13 goes one step further and replaces the treatment indicator with the tax revenue the state actually collected. Each additional dollar of monthly sports betting tax per capita is associated with spreads that are 10.5, 11.4, and 18.4 basis points lower (Panel A). Because the fiscal benefit accumulates as the market matures, the cumulative collections since launch are the more natural measure: each $100 per capita collected since launch lowers the MMA-

adjusted spread by 56.5 basis points, the Treasury-adjusted spread by 61.5 basis points, and the tax-adjusted spread by 104.9 basis points (Panel B). When the treatment indicator and cumulative collections enter together (Panel C), both remain negative and, for the MMA- and Treasury-adjusted spreads, both are significant at the 1% level. That is, part of the repricing occurs at launch and part tracks the revenue as it arrives, which is what the gradual pattern in Figure 6 and Hypothesis 3 would suggest.

[Table 13 about here]

### 5.8. Heterogeneity by State Fiscal Conditions

The fiscal capacity channel also implies that a given amount of new revenue should matter more to a state whose finances are under more strain. Table 14 splits the twelve treated states at the median of two pre-legalization fiscal characteristics and estimates separate treatment effects for each group within the same regression. Panel A uses the state pension funded ratio averaged over 2013–2017. In the six states with below-median funded ratios, legalization lowers spreads by 9.32, 10.21, and 13.26 basis points, all significant at the 1% level. In the six better-funded states, the estimates are $-0.46$, $-0.82$, and $-1.39$ basis points and are not distinguishable from zero. The difference between the two groups is significant at the 1% level for every spread measure.

[Table 14 about here]

Panel B splits treated states by the share of selective sales taxes in total tax revenue over fiscal years 2013–2017. States that already leaned heavily on excise-type taxes see little change in spreads after legalization, with coefficients of $-1.18$, $-2.76$, and $-2.91$ basis points, whereas states with a low reliance on such taxes see reductions of 9.18, 9.26, and 11.81 basis points. This is what the diversification argument in Section 3.1 predicts: the

marginal value of a new revenue base with different economic drivers is highest where the existing tax portfolio is least exposed to it. I read these state-level splits with some caution because they rest on twelve treated states, and Illinois, the state whose exclusion moves the baseline estimate the most, falls into the poorly-funded and low-reliance groups in both panels. Excluding Illinois from the full sample reduces the baseline MMA-adjusted estimate from 6.43 to 3.33 basis points, which remains significant at the 5% level (Figure 13).

### 5.9. Credit Ratings and Bond Supply

Two other explanations for a decline in secondary market spreads deserve attention. The first is that rating agencies upgraded state issuers after legalization, so that lower spreads simply reflect rating changes. Table 15, Panel A, regresses indicators for a rating upgrade or downgrade since the bond's previous trade on the treatment indicator using the stacked bond-month sample. Neither coefficient is different from zero: the probability of an upgrade changes by 0.1 percentage points and the probability of a downgrade by $-0.2$ percentage points. Panel C aggregates the bond sample to the state-year level and finds no change in the average composite rating of a state's outstanding bonds or in the share rated AA$-$ or better. The repricing therefore takes place in the secondary market ahead of, and without, any action by the rating agencies over the sample period.

[Table 15 about here]

The second explanation is that states with a new revenue source issued less debt, and that spreads fell because of a contraction in supply. Panel B uses the primary market sample collapsed to the state-month level. The number of new issues, the log number of issues, and the log dollar amount issued are all unchanged after legalization, with coefficients of $-1.27$ issues, $-0.03$, and 0.03 log points, none of which approaches conventional significance levels. The decline in spreads does not appear to be a supply effect.

## 6. Robustness

This section examines the sensitivity of the main findings. Section 6.1 replaces the never-treated control group with not-yet-treated states. Section 6.2 restricts the sample to a propensity-score-matched set of bonds. Section 6.3 adds bond-by-cohort fixed effects. Section 6.4 re-examines statistical inference with state-level clustering, aggregated regressions, and randomization inference. Section 6.5 drops each treated state in turn, excludes the COVID-19 period, and varies the event window. Section 6.6 reports estimates from the Callaway and Sant'Anna (2021) estimator.

### 6.1. Alternative Control Group

The baseline estimates in Table 3 use never-treated states as the control group in Equation (5). As an alternative, I re-estimate the same specification using not-yet-treated states, which eventually legalized online sports betting but have not yet done so at the time of a given cohort's launch date. This comparison better captures unobservable similarities between treated and control states.

Tables 16 reports the results. The secondary market estimates are quantitatively similar to the baseline: the coefficients with controls are $-6.4$, $-7.0$, and $-9.5$ basis points for the MMA-adjusted, Treasury-adjusted, and tax-adjusted spreads, all significant at the 1% level, compared with $-6.4$, $-7.2$, and $-9.4$ basis points under the never-treated control group (Table 3).

[Tables 16 about here]

Figure 10 presents the corresponding event-study estimates, which support the parallel trends assumption under this alternative control group. The main findings are therefore robust to the choice of control group for different states.

[Figure 10 about here]

### 6.2. Propensity Score Matching

A potential concern with the baseline design is that treated bonds may differ systematically from control bonds in ways that shape yield spreads independently of legalization. To address this concern, I re-estimate the secondary market specification on a propensity-score-matched (PSM) sample. The objective of the PSM is to match treated bonds (those issued in states that legalized online sports betting) with control bonds (those issued in never-treated states) based on pre-treatment characteristics, thereby creating a counterfactual group of bonds issued in states where legalization did not occur. Within each treatment cohort, I estimate a logit propensity score on the following covariates, measured as pre-treatment averages: years to maturity, credit rating, log offering amount, coupon rate, callable, general obligation, tax-exempt status, and yield spread. I restrict the matching pool to bonds with at least five years of remaining maturity at the time of matching, ensuring that each bond is observable throughout the event window. I then perform one-to-one nearest-neighbor matching with replacement and a caliper of 0.05, restricting to the common support region.[16] The matched bond sample is then merged back to the full bond-month panel within a 30-month window around the legalization event, and I re-estimate Equation (5). Table 17 reports the results. Across all three spread measures and both specifications, the coefficient on *Sports Gambling* remains negative and significant at the 1% level. With the full set of controls, legalization reduces the MMA-adjusted spread by 6.1 basis points, the Treasury-adjusted spread by 5.9 basis points, and the tax-adjusted spread by 7.1 basis points. These magnitudes are consistent with the baseline estimates in Table 3, indicating that the effect is not driven by differences in observable bond characteristics between treated and control states.

[Table 17 about here]

[16] The results are similar under one-to-three and one-to-five nearest-neighbor matching.

Figure 11 plots the corresponding event-study estimates on the matched sample. The pre-treatment coefficients are close to zero across all three spread measures, and the post-treatment coefficients turn negative and grow in magnitude over the quarters following legalization, mirroring the dynamics documented in Figure 6.

[Figure 11 about here]

### 6.3. Bond Fixed Effects

A potential concern with the baseline specification is that observable bond characteristics may not fully capture time-invariant unobserved heterogeneity at the individual bond level. Because the secondary market sample tracks individual bonds over time, the treatment variation is primarily within-bond: treated bonds are traded and observed both before and after the legalization event.

To address this concern and more cleanly isolate the yield response to legalization, I estimate a more demanding specification that replaces the state-by-cohort fixed effects with bond-by-cohort fixed effects. This approach absorbs all time-invariant unobserved bond characteristics, relying strictly on the within-bond variation in yield spreads around the online launch dates. Note that while time-invariant bond controls (such as coupon rate and offering amount) are absorbed by the fixed effects, time-varying bond controls and state-level macroeconomic controls are retained.

Table 18 reports the results. The estimates remain highly statistically significant and economically consistent with the baseline findings. Following online sports betting legalization, the MMA-adjusted spread falls by 6.68 basis points, the Treasury-adjusted spread by 7.20 basis points, and the tax-adjusted spread by 8.74 basis points. The adjusted R-squared increases substantially, confirming that the bond fixed effects absorb a significant amount of cross-sectional noise. These results provide strong evidence that the reduction in bor-

rowing costs is driven by within-bond repricing rather than unobserved differences in bond composition across states.

[Table 18 about here]

### 6.4. Inference

The baseline standard errors are double-clustered by bond and year-month. Because treatment is assigned at the state level, residuals for bonds of the same state issuer are likely correlated across bonds and over time within a state, and **?** show that ignoring this correlation can overstate the precision of difference-in-differences estimates. Table 19 addresses this in three ways. Panel A re-estimates the baseline specification with standard errors clustered by state. The coefficients are unchanged by construction, and the standard errors rise to 3.05, 2.98, and 4.44 basis points, so that the estimates of $-6.43$, $-7.15$, and $-9.39$ basis points remain significant at the 5% level with 32 clusters. Panel B follows **?** and collapses the stacked sample to state-cohort-month averages, weighting each cell by the number of bond-months it contains. The aggregated regression yields $-7.09$, $-8.00$, and $-11.10$ basis points, again significant at the 5% level with state clustering.

[Table 19 about here]

Because the sample contains only 32 states, I also conduct randomization inference. I collapse the secondary market data to a state-month panel of the 12 treated and 20 never-treated states over the span of the stacked sample, estimate a two-way fixed effects regression with state and year-month fixed effects, and then randomly reassign the twelve actual launch dates across the 32 states 1,000 times, re-estimating the regression on each draw. Panel C reports the actual estimates, $-10.79$, $-11.00$, and $-18.80$ basis points, and Figure 12 plots the placebo distributions. The share of placebo coefficients at least as large in absolute

value as the actual estimate is 2.6%, 2.7%, and 1.6% for the three spread measures, so the actual estimates are unlikely to have arisen from the particular set of states that happened to legalize.

[Figure 12 about here]

### 6.5. Influential States, the COVID-19 Period, and the Event Window

With twelve treated states, a natural question is whether any one of them drives the result. Figure 13 plots the baseline coefficient when each treated state is dropped in turn. All 36 estimates are negative and significant at the 5% level or better. The largest change comes from excluding Illinois, which lowers the MMA-adjusted estimate to −3.33 basis points (standard error 1.35); excluding any other state leaves the estimate between −5.80 and −7.37 basis points. The corresponding ranges are −4.25 to −8.17 basis points for the Treasury-adjusted spread and −5.53 to −11.05 basis points for the tax-adjusted spread.

[Figure 13 about here]

Several cohorts launched shortly before or during the COVID-19 disruption of the municipal market in spring 2020, when spreads widened sharply and then reversed. Panel A of Table 20 drops all bond-months from March 2020 through December 2020, and alternatively through June 2021, from the stacked sample. The estimates become larger in absolute value, between −10.5 and −11.3 basis points for the MMA- and Treasury-adjusted spreads and between −16.6 and −18.3 basis points for the tax-adjusted spread, all significant at the 1% level. The pandemic period therefore works against rather than for the baseline result.

[Table 20 about here]

Panel B varies the length of the event window around each launch date from the 30 months used in the baseline to 18, 24, and 36 months. The MMA-adjusted estimate is −6.64, −5.61, and −9.07 basis points across the three windows, and the Treasury-adjusted and tax-adjusted estimates move in the same way. The estimate grows with the window length, which is consistent with the gradual pattern in Figure 6: longer windows place more weight on the later post-launch months, where the effect is largest.

### 6.6. Alternative Estimator

The stacked design avoids the negative weighting problems of two-way fixed effects estimation under staggered adoption, but it does so by restricting attention to a fixed window and to never-treated controls. As a check, I estimate the group-time average treatment effects of Callaway and Sant'Anna (2021) on a state-quarter panel of average spreads from 2016Q1 through 2024Q4, treating every state that launched online betting during this period as a treatment group and never-treated states as the comparison group, with the state controls entering through the doubly robust procedure. Table 21 reports the aggregate effect: −4.90 basis points for the MMA-adjusted spread, −4.95 basis points for the Treasury-adjusted spread, and −12.45 basis points for the tax-adjusted spread, each significant at the 5% level with state-clustered standard errors. The estimates are somewhat smaller than the baseline for the first two measures, which is expected because the state-quarter panel weights each state equally rather than by trading volume and includes the later cohorts with fewer post-launch quarters. Figure 14 plots the dynamic estimates. The pre-launch coefficients are small, and the post-launch coefficients decline over the two years after launch, matching the pattern in Figure 6.

[Table 21 and Figure 14 about here]

## 7. Conclusion

This paper studies whether the legalization of online sports betting affects the cost at which U.S. state governments borrow in the municipal bond market. Exploiting the staggered rollout of online sports betting platforms across states following the Supreme Court's 2018 decision in *Murphy v. NCAA*, I find that online legalization reduces secondary-market yield spreads by 6 to 10 basis points, with the effect emerging gradually over the quarters following launch. Mechanism evidence points to a positive net fiscal effect: state tax revenue rises substantially, while expenditure on health, police, welfare, and hospitals does not increase significantly over the sample horizon. Cross-sectional heterogeneity further supports this fiscal capacity channel: the spread reduction is significantly larger for general obligation bonds than for revenue bonds, it is most pronounced for long-term maturities, and it is concentrated in lower-rated bonds and in states with weaker pension funding, indicating that investors price this reform as a permanent, structural expansion of the state tax base. The reduction also grows with the size of the betting market and with the tax revenue each state actually collects, while rating agencies, bond issuance, and other tax sources show no response over the sample period.

These findings carry three broader implications. First, they show that bond markets distinguish between the gross revenue and the net fiscal consequences of sin-tax policies. Specifically, the sign of the yield response depends on the policy's net fiscal effect over the relevant horizon. Second, the gradual emergence of the yield effect highlights the role of information frictions in municipal bond pricing. Unlike equity markets, where policy news is often incorporated quickly, the OTC structure of the municipal market implies that fiscal signals from staggered state regulatory reports are absorbed over quarters rather than instantaneously. Third, the results show a tension between municipal bond pricing and household welfare. A growing literature documents the financial and mental health costs of sports betting for individuals, yet municipal investors respond only to its aggregate fiscal

effects. Policies that lower state borrowing costs can therefore coexist with meaningful harm to household balance sheets, a trade-off that complicates the welfare assessment of sin-tax legalization.

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Figure 1 **Legal Status of Sports Betting Across U.S. States**

This figure shows the legal status of online and retail sports betting in each of the 50 states plus the District of Columbia as of the end of 2025. *Both Online & Retail Legalized* refers to states that allow betting both through online apps and at approved retail locations. *Only Online* or *Only Retail* indicates that betting is only legal through online apps or onsite locations. *Not Legalized* indicates that neither type of sports gambling is allowed.

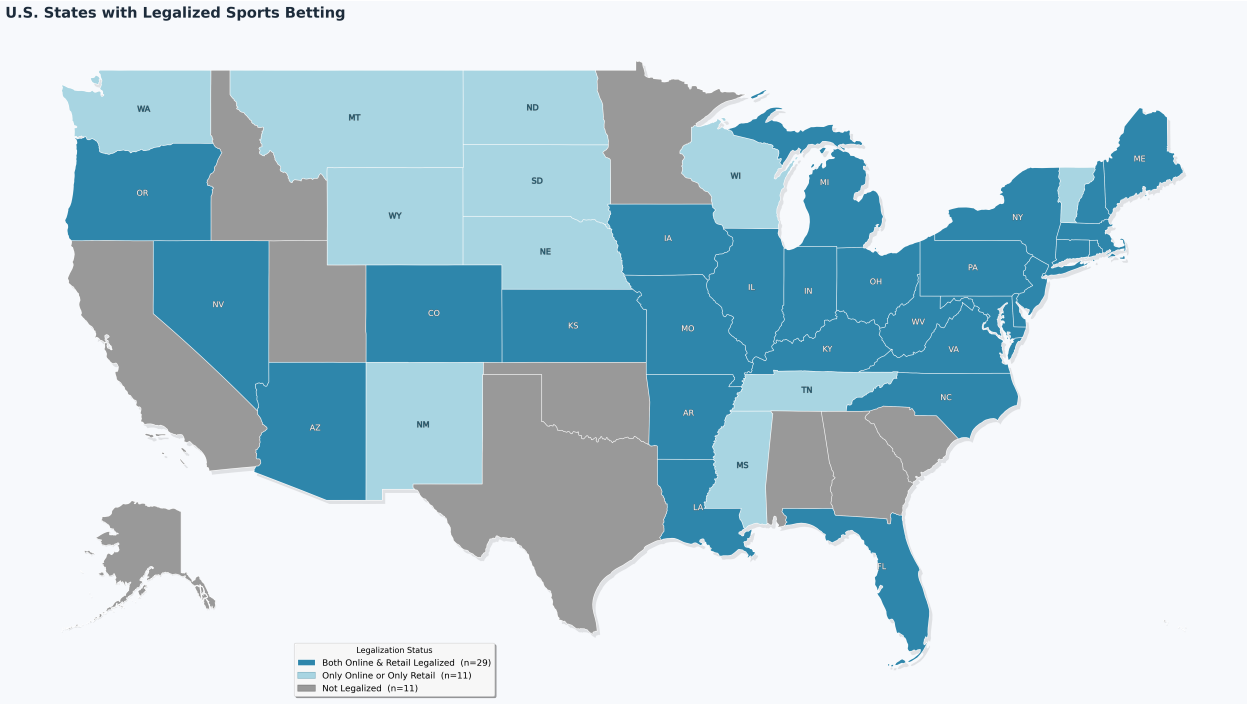


Figure 2 **Staggered Adoption Timeline of Online Sports Betting across U.S. States**

This figure plots the online launch date for each state that legalized sports betting following *Murphy v. National Collegiate Athletic Association* (May 2018), marked by the dashed vertical line. Each diamond indicates the month when the first licensed mobile betting application became publicly available in that state. States are sorted by launch date. Colors distinguish treatment cohorts by launch year. Nevada is excluded from the sample as it has operated a legal sports betting market since 1949.

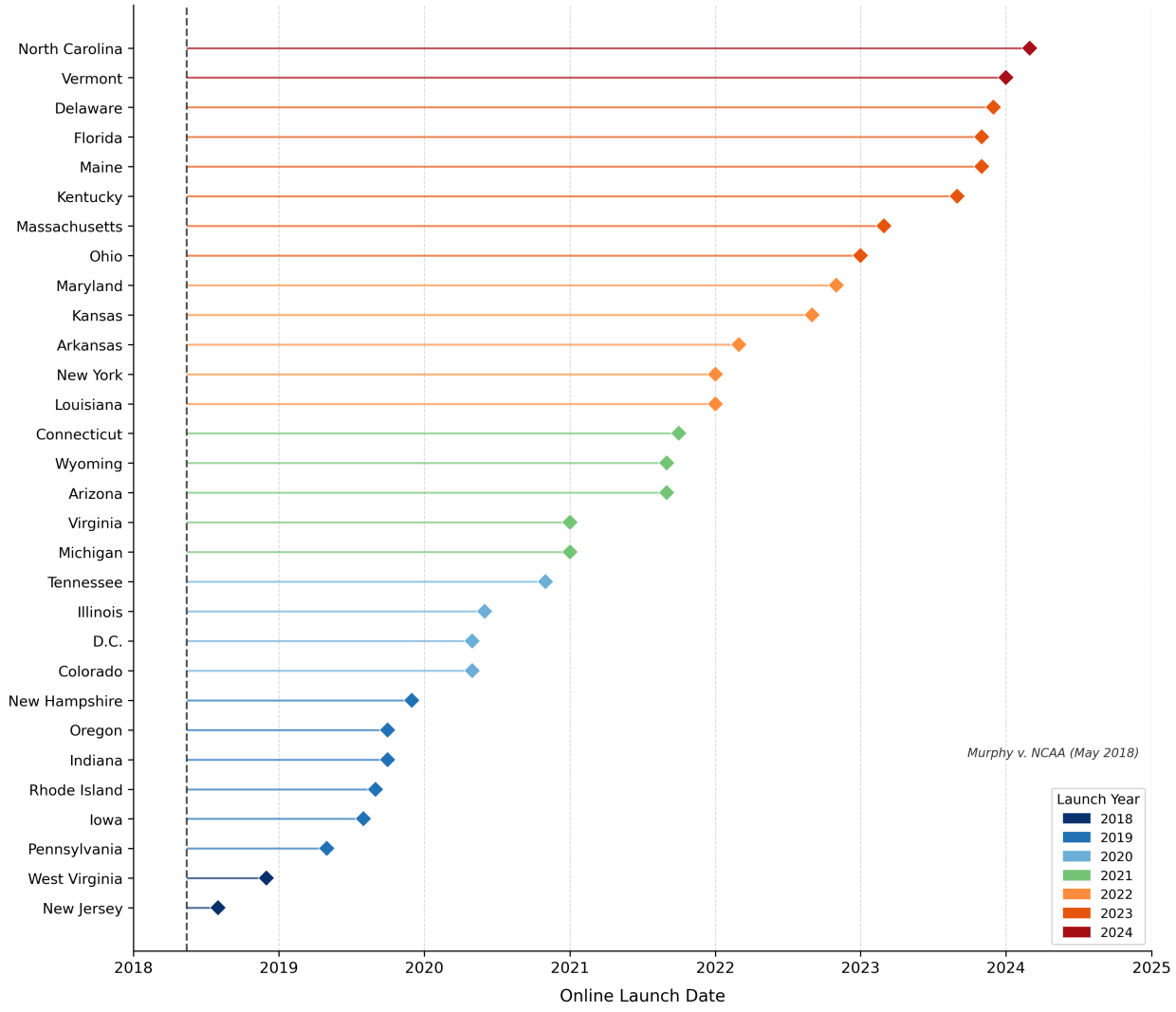


Figure 3 **Monthly Sports Betting Handle and State Adoption in the U.S., June 2018–December 2025**

This figure plots the monthly total sports betting handle and the cumulative number of states with legal sports gambling in the U.S. from June 2018 through December 2025. Bars measure total monthly handle (left axis, USD billions). The stepped line tracks the cumulative number of states with active sports gambling (right axis). The shaded band marks the COVID-19 disruption period (March–July 2020), during which handle collapsed before recovering sharply.

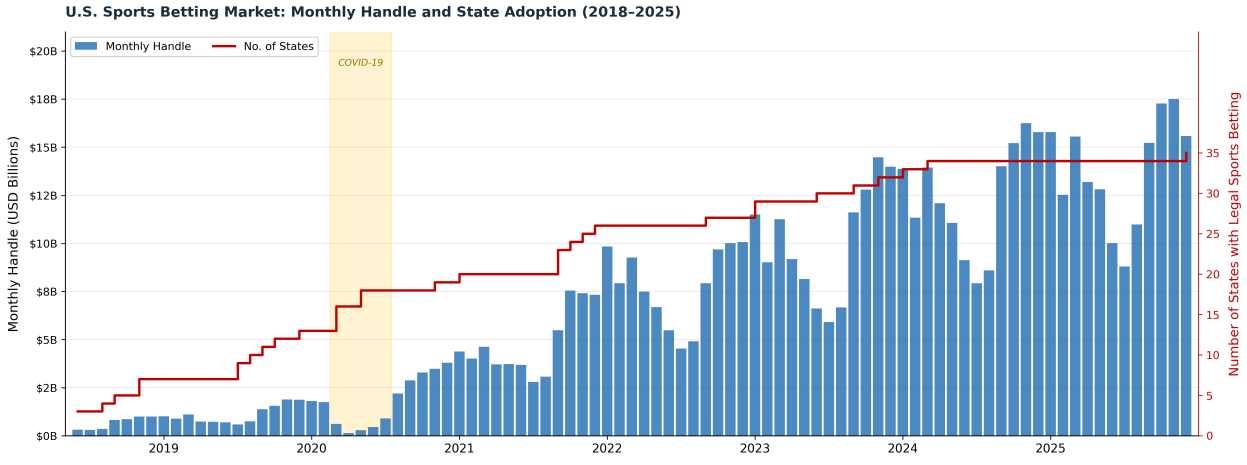


Figure 4 **Monthly State Tax Revenue and GGR Win Rate in the U.S. Sports Betting Market, June 2018–December 2025**

This figure tracks the evolution of monthly state tax revenue and the gross gaming revenue (GGR) win rate in the U.S. sports betting market from June 2018 through December 2025. Bars measure aggregate state tax collections (left axis, USD millions). The solid line plots the GGR win rate, defined as gross sportsbook revenue divided by total handle, and the dashed line marks its sample mean of 8.1% (right axis). The shaded band marks the COVID-19 disruption period (March–July 2020), during which handle collapsed before recovering sharply.

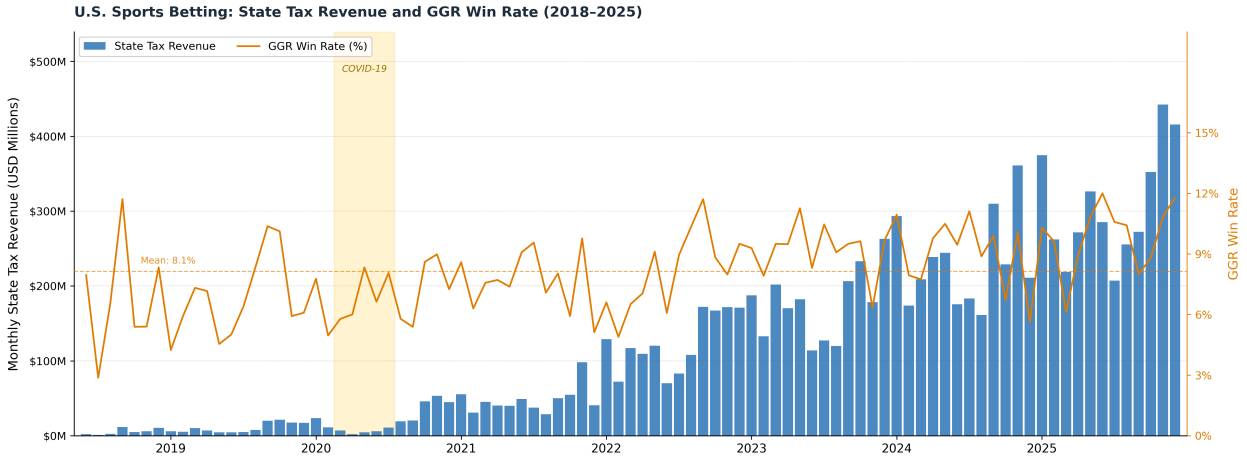
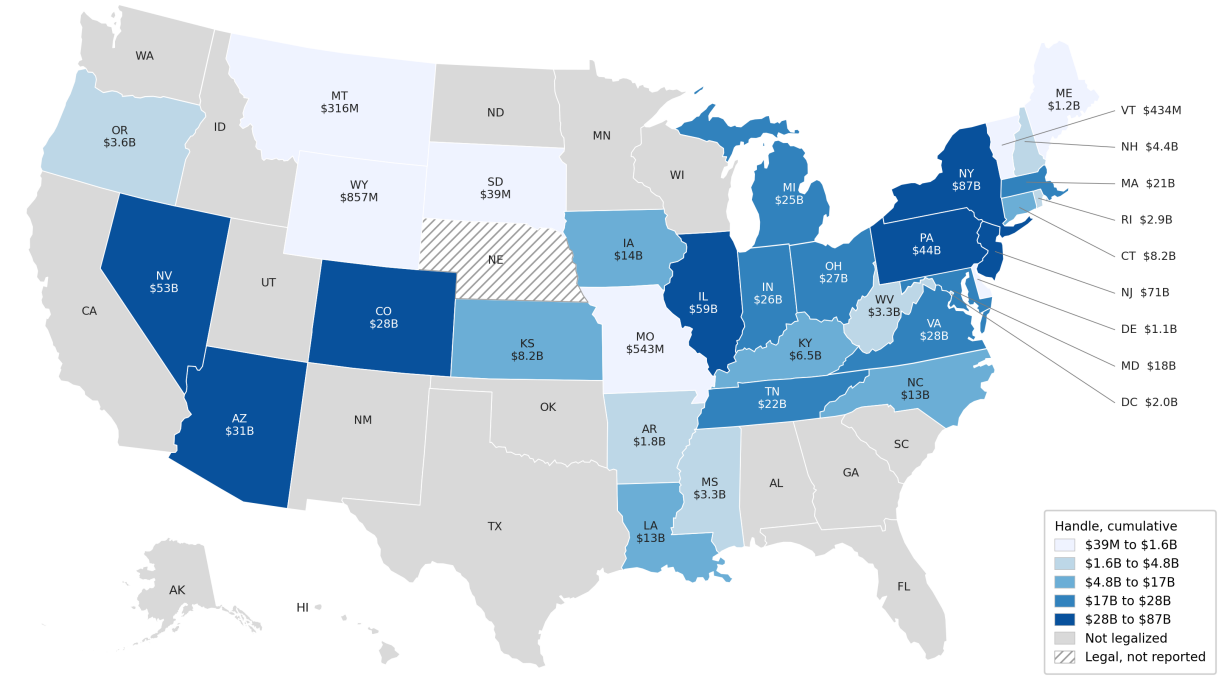


Figure 5 **Geographic Distribution of Cumulative Sports Betting Activity by State**

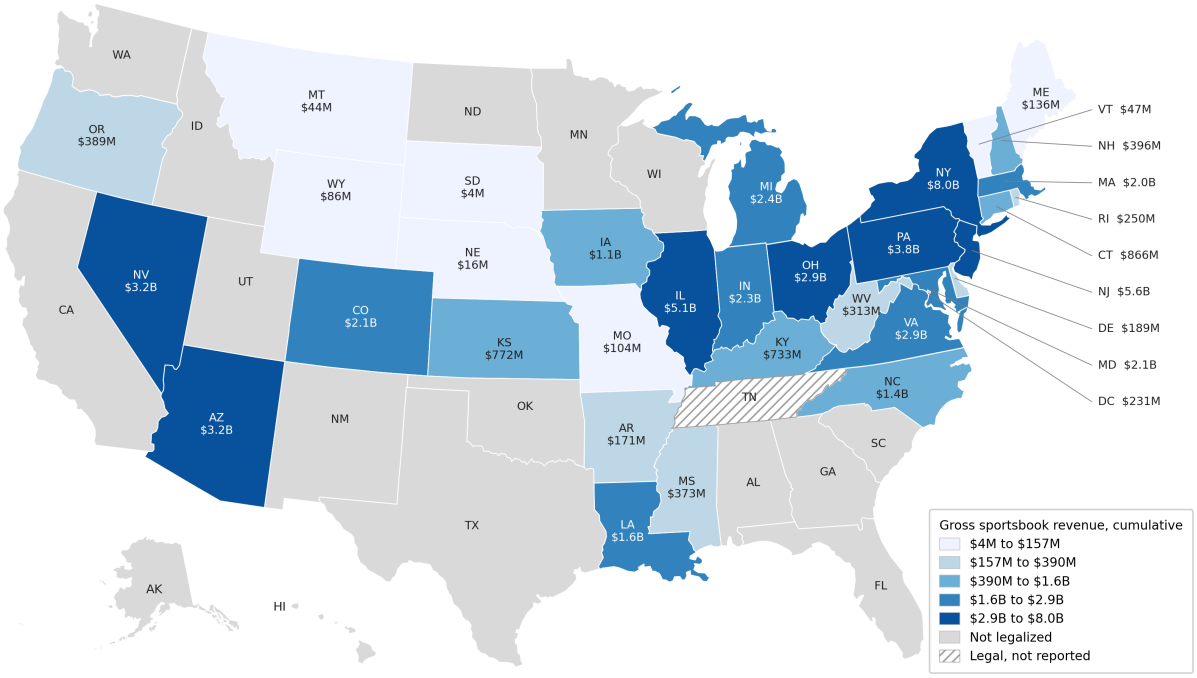
These figures show the geographic distribution of cumulative sports betting activity across U.S. states through the end of 2025, measured by total handle, gross sportsbook revenue, and state tax collections respectively. Color intensity follows a log scale ranging from light orange to dark red. Medium gray indicates states where sports betting has not been legalized. Light gray indicates states where betting is legal but the relevant data are not publicly reported.

**Panel A: Cumulative Sports Betting Handle by State**



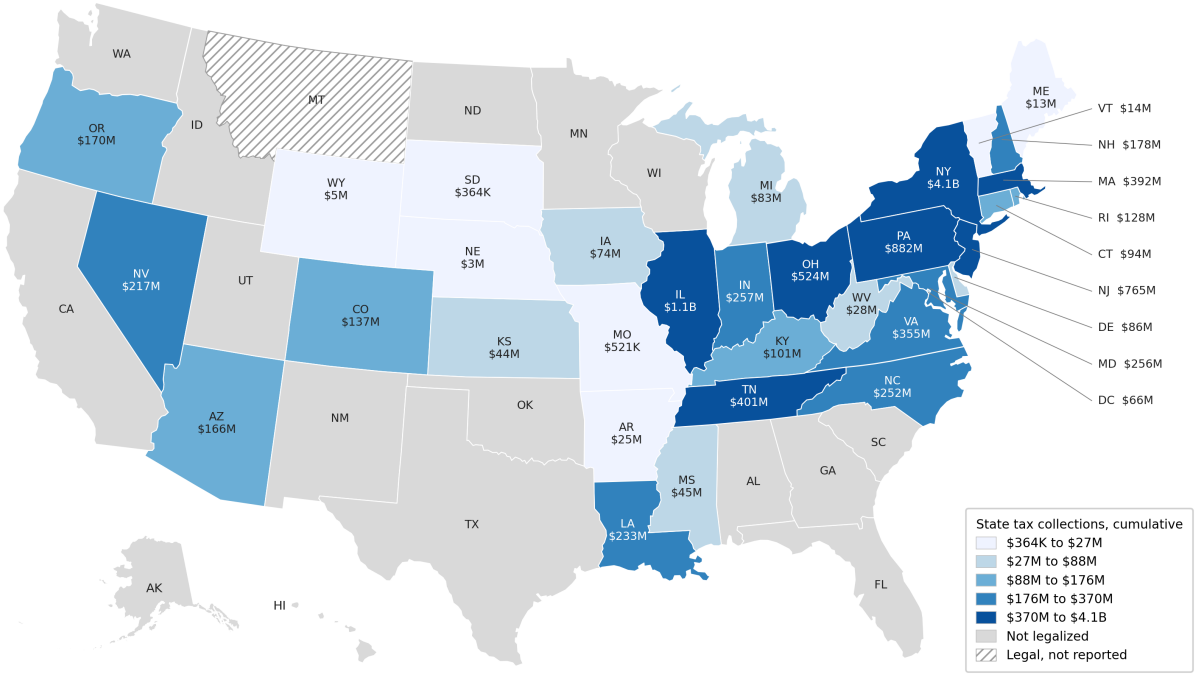

Cumulative totals from each state's launch through December 2025, compiled by Sports Handle from state regulator reports.
Classes are quintiles of the legal states. Gray: no legal sports betting. Hatched: legal but figure not reported.

## Panel B: Cumulative Gross Sportsbook Revenue by State




Cumulative totals from each state's launch through December 2025, compiled by Sports Handle from state regulator reports.
Classes are quintiles of the legal states. Gray: no legal sports betting. Hatched: legal but figure not reported.

## Panel C: Cumulative State Tax Revenue from Sports Betting by State

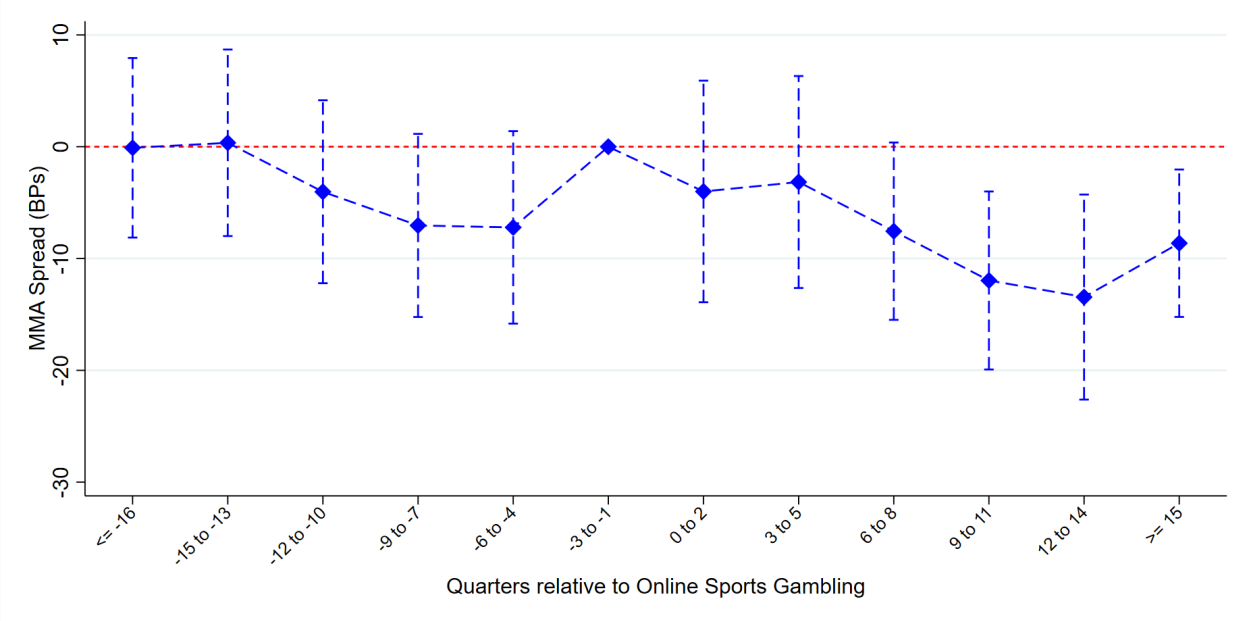



Cumulative totals from each state's launch through December 2025, compiled by Sports Handle from state regulator reports.
Classes are quintiles of the legal states. Gray: no legal sports betting. Hatched: legal but figure not reported.

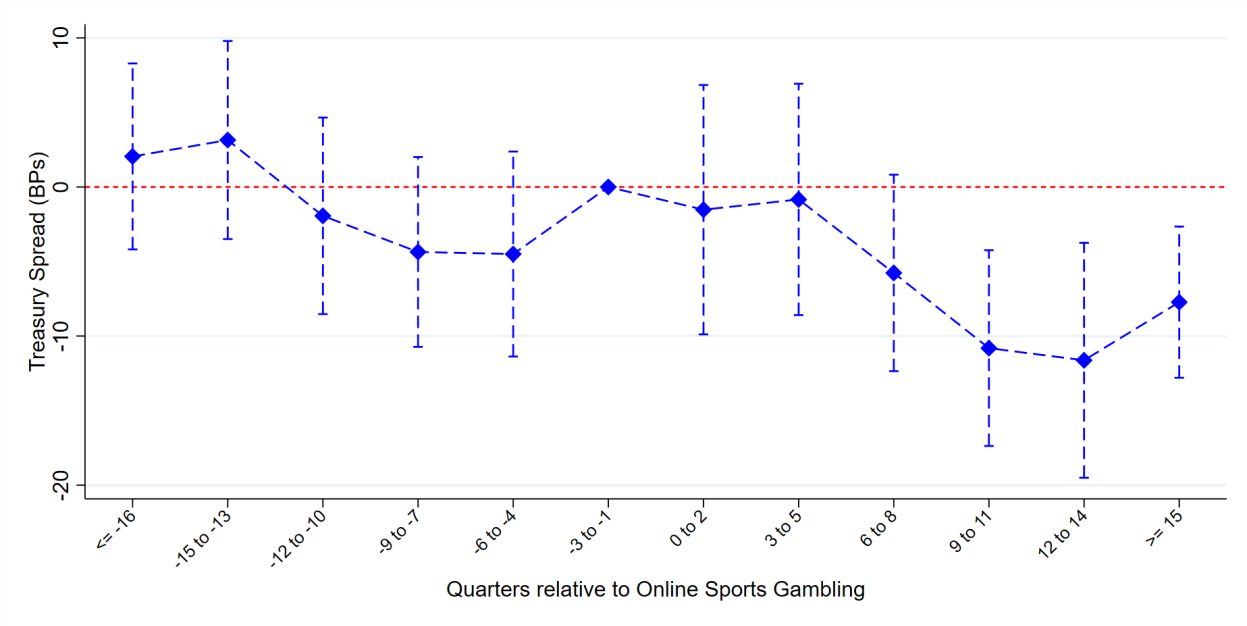
Figure 6 **Secondary Market Yields around Online Sports Betting Legalization (Never-Treated as Control)**

This figure plots estimates of $\beta_q$ from the event-study specification in Equation 6, using never-treated states as the control group. The sample includes bonds issued by state-level governments within a 30-month window around the legalization event. Each bin spans three months. The omitted category is the bin covering months $-3$ through $-1$ immediately prior to the online launch date. All specifications include bond-level controls, lagged state-level controls, and state-by-cohort and year-month-by-cohort fixed effects. Whiskers represent 90% confidence intervals based on standard errors double-clustered at the bond and year-month level.

**Panel A: MMA Spread**



**Panel B: Treasury Spread**




**Panel C: Tax-Adjusted Spread**

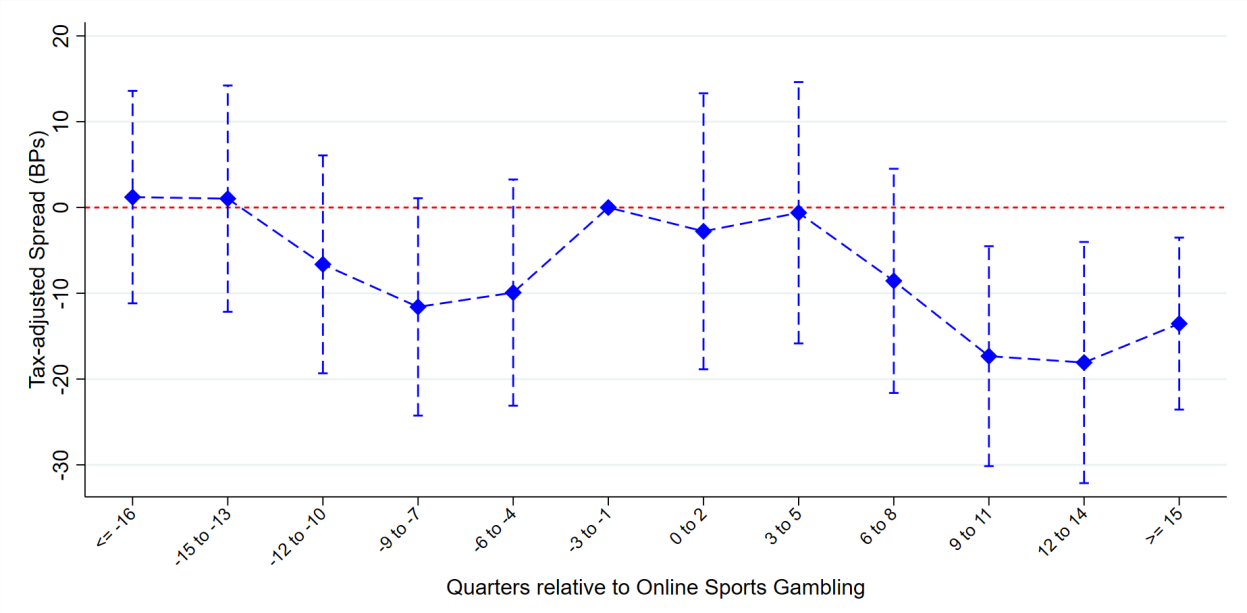
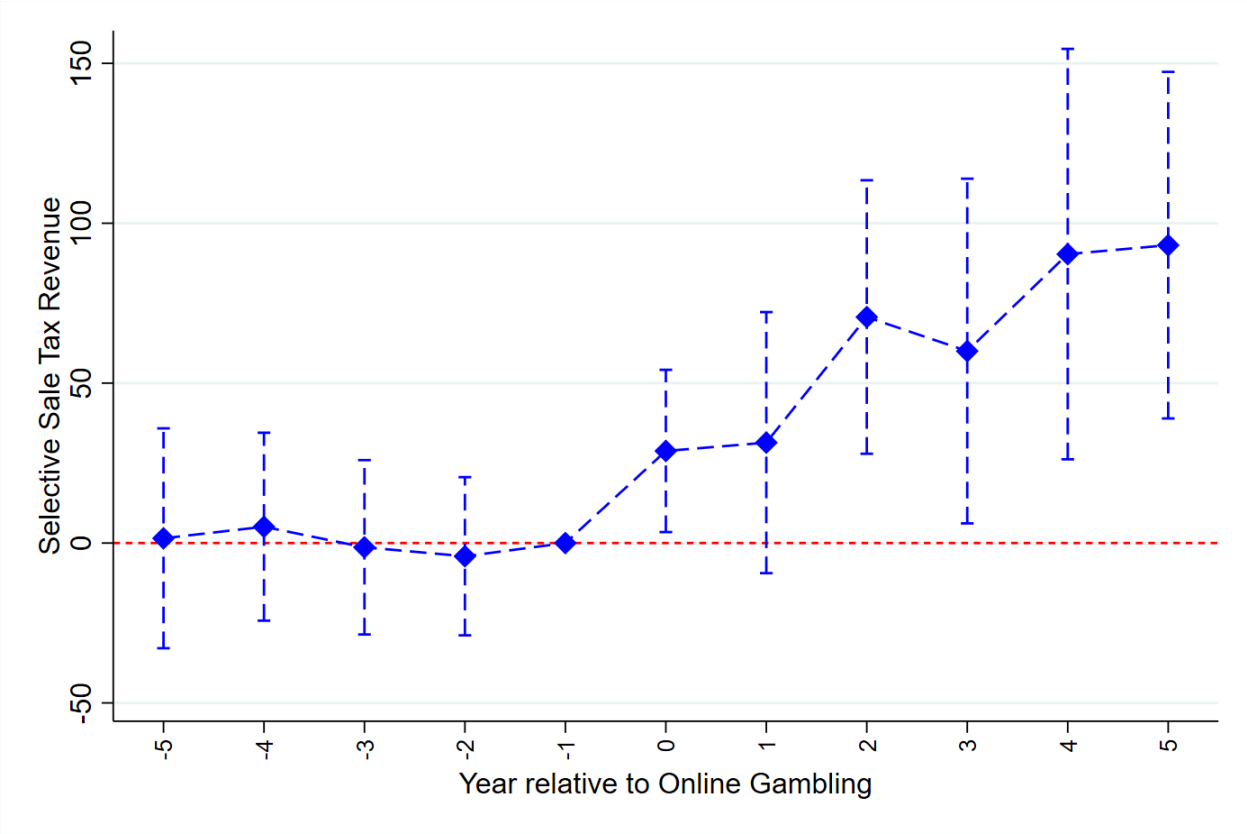


Figure 7 **State Tax Revenue around Online Sports Betting Legalization - Year Level**

This figure plots estimates of $\beta_q$ from the event-study specification in Equation 6, using annual data from the Census Bureau's Annual Survey of State Government Finances (ASFIN) and never-treated states as the control group. The sample includes annual state-level finance data within a five-year window around each legalization event. The omitted category is the year prior to the online launch date. All specifications include lagged state-level controls and state-by-cohort and year-by-cohort fixed effects. Whiskers represent 90% confidence intervals based on standard errors clustered at the state level.

**Panel A: Selective Sales Tax Revenue per Capita**



**Panel B: Pari-mutuel & Sports Tax Revenue per Capita**

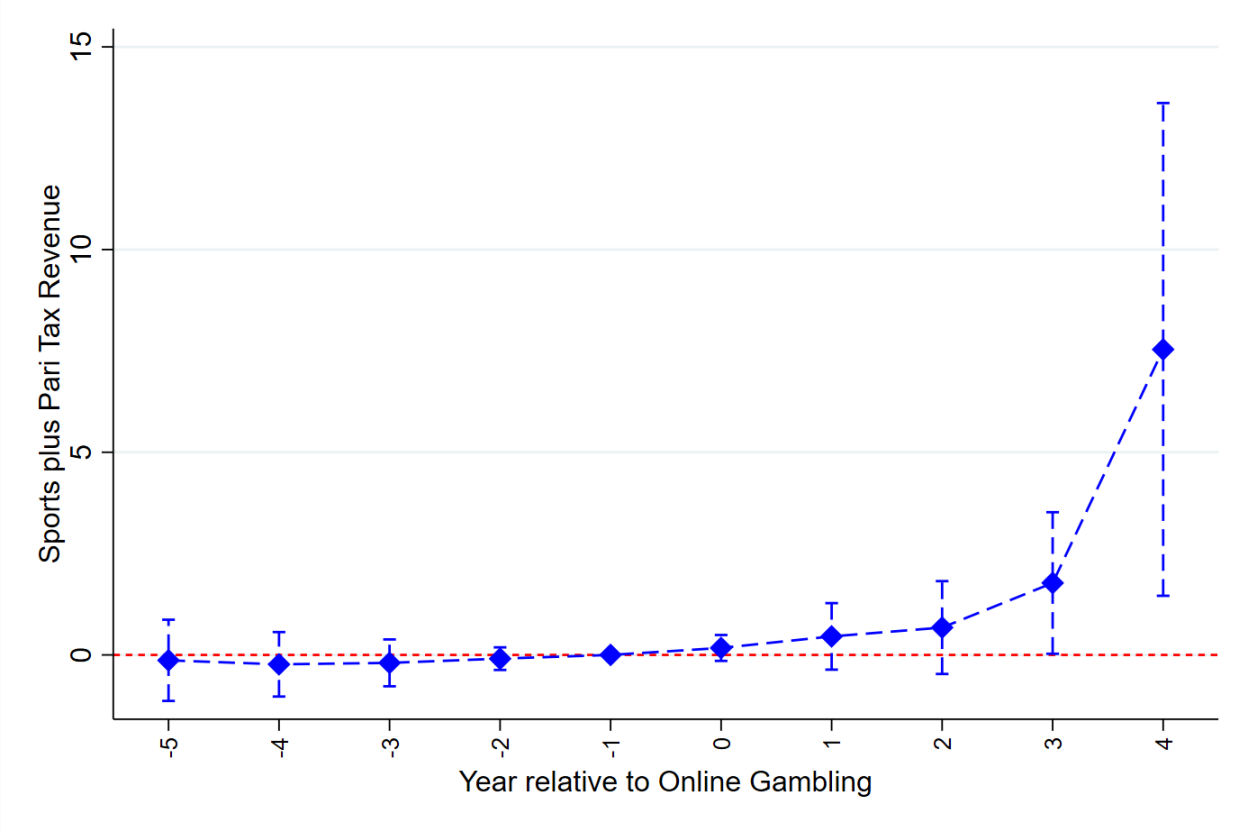


Figure 8 **Pari-mutuel & Sports Tax Revenue around Online Sports Betting Legalization - Quarter Level**

This figure plots estimates of $\beta_q$ from the event-study specification in Equation 6, using quarterly data from the Census Bureau's Quarterly Summary of State and Local Government Tax Revenue (QTAX) and never-treated states as the control group. The sample includes quarterly state-level finance data within a 20-quarter window around each legalization event. The omitted category is the quarter immediately prior to the online launch date. All specifications include lagged state-level controls and state-by-cohort and quarter-by-cohort fixed effects. Whiskers represent 90% confidence intervals based on standard errors clustered at the state level.

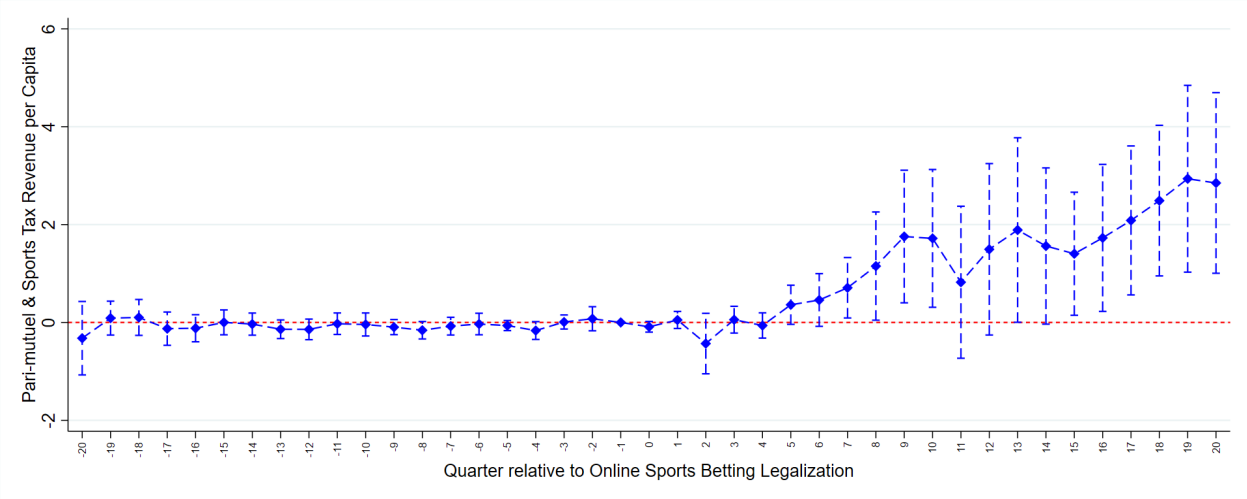
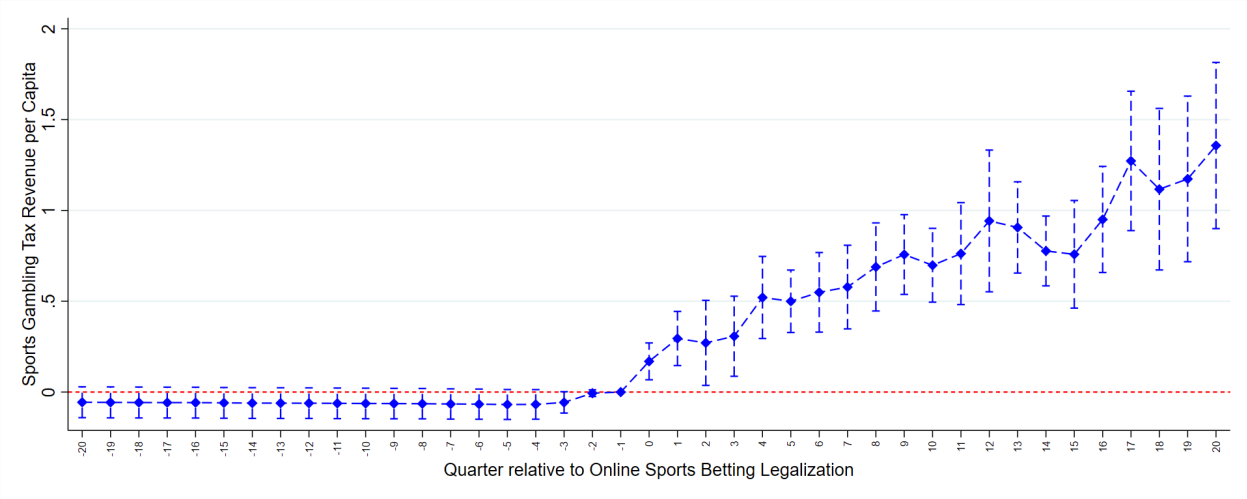


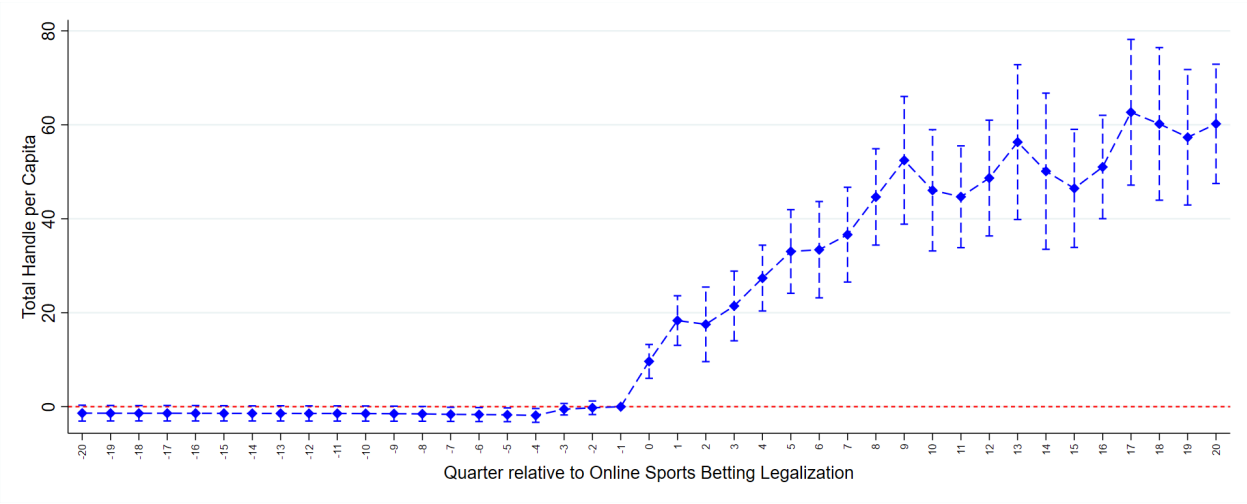
Figure 9 **Sports Gambling Tax Revenue around Online Sports Betting Legalization - Quarter Bins from Monthly Data**

This figure plots estimates of $\beta_q$ from the event-study specification in Equation 6, using monthly data from the Sports Handle and never-treated states as the control group. The sample includes monthly state-level finance data within a 60-month window around each legalization event. To reduce noise, monthly relative time is aggregated into three-month (quarterly) bins, so each coefficient represents the average effect within a three-month interval. The omitted category is the quarter immediately prior to the online launch date. All specifications include lagged state-level controls and state-by-cohort and month-by-cohort fixed effects. Whiskers represent 90% confidence intervals based on standard errors clustered at the state level.

**Panel A: Sports Gambling Tax Revenue per Capita**



**Panel B: Total Handle per Capita**




**Panel C: Gross Sportsbook Revenue per Capita**

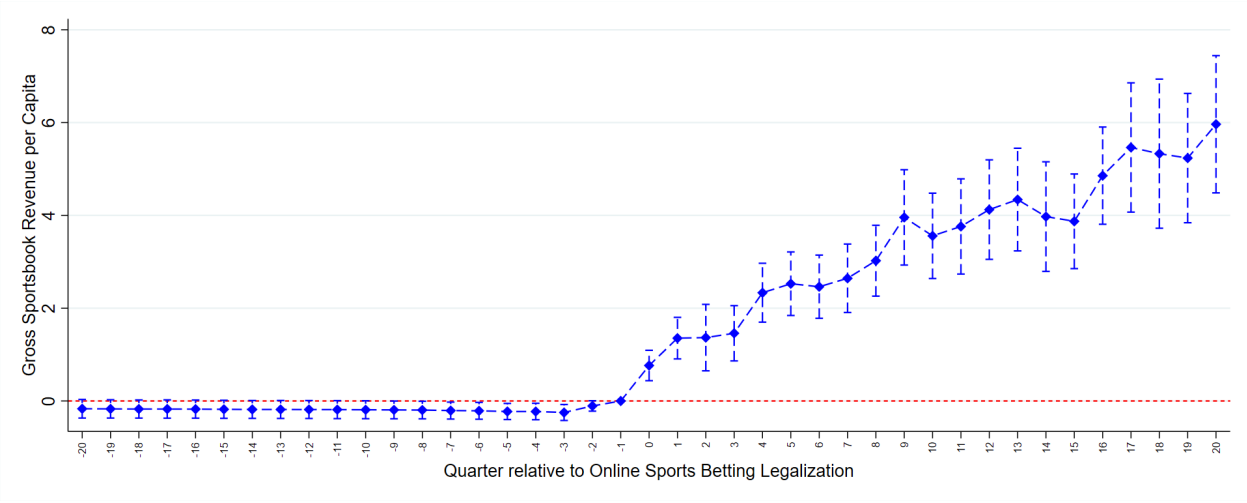
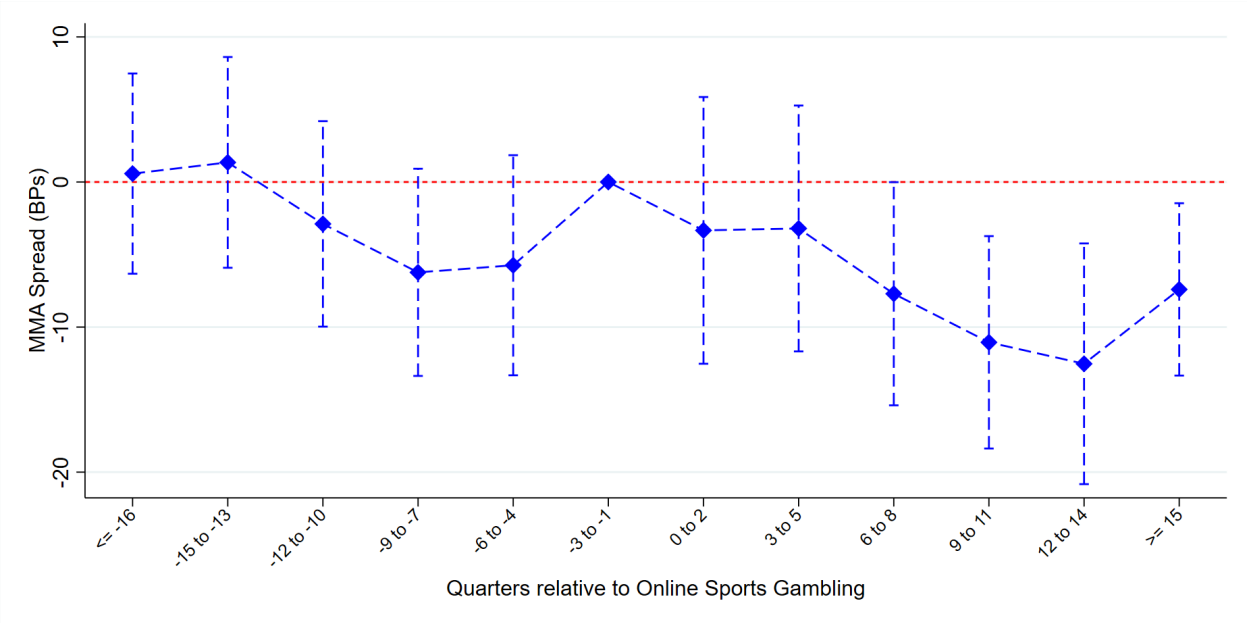


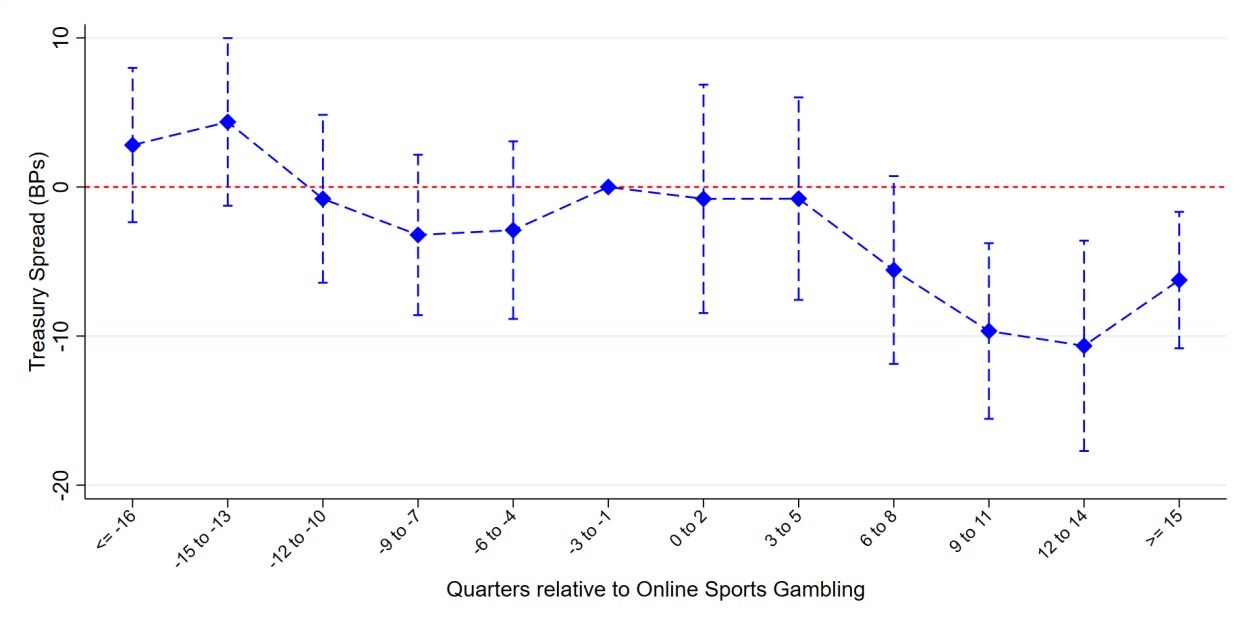
Figure 10 **Secondary Market Yields around Online Sports Betting Legalization (Not-Yet-Treated as Control)**

This figure plots estimates of $\beta_q$ from the event-study specification in Equation 6, using not-yet-treated states as the control group. The sample includes bonds issued by state-level governments within a 30-month window around the legalization event. Each bin spans three months. The omitted category is the bin covering months $-3$ through $-1$ immediately prior to the online launch date. All specifications include bond-level controls, lagged state-level controls, and state-by-cohort and year-month-by-cohort fixed effects. Whiskers represent 90% confidence intervals based on standard errors double-clustered at the bond and year-month level.

**Panel A: MMA Spread**



**Panel B: Treasury Spread**




**Panel C: Tax-Adjusted Spread**

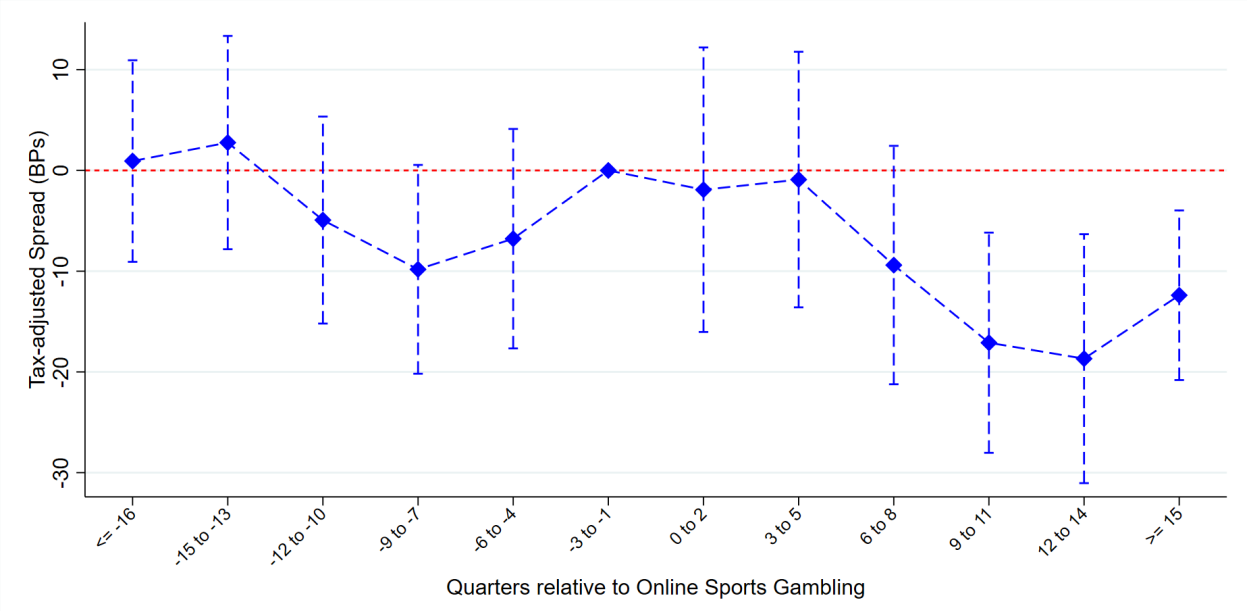
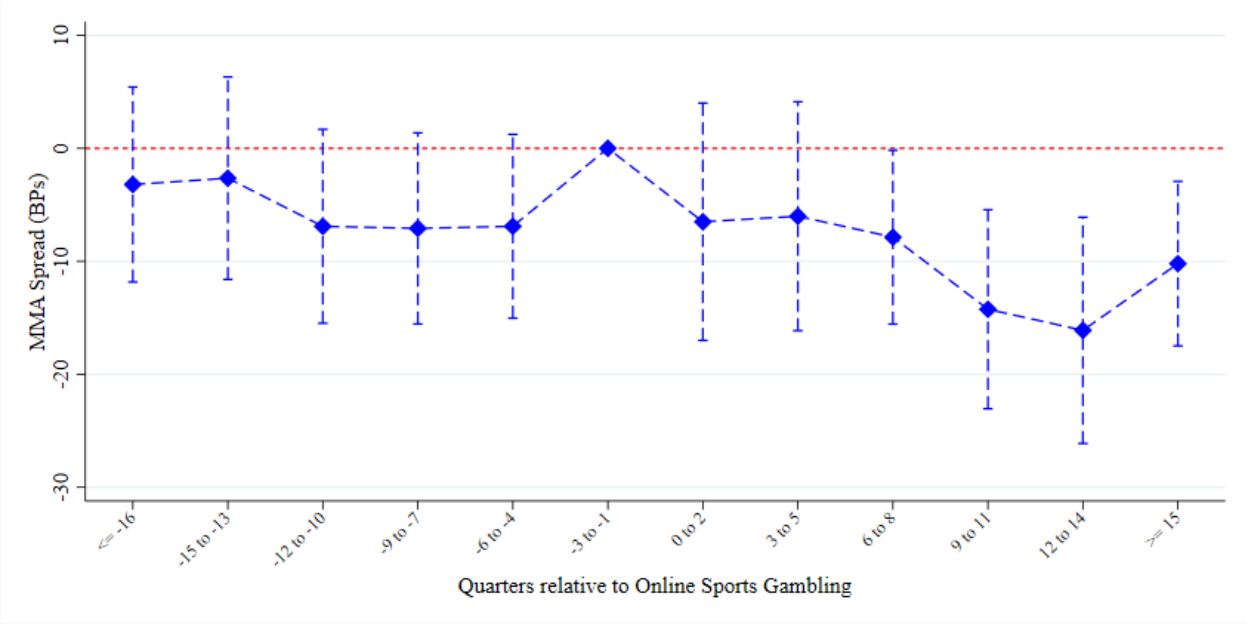


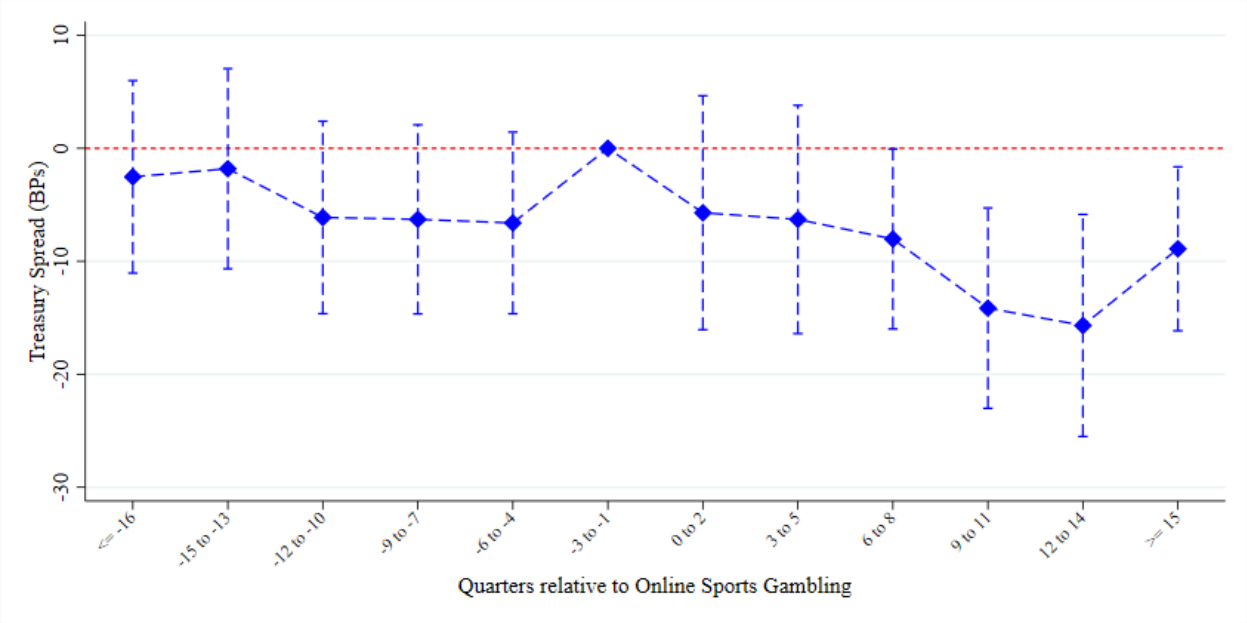
Figure 11 **PSM-DID: Secondary Market Yields around Online Sports Betting Legalization (Never-Treated as Control)**

This figure plots event-study estimates from a stacked difference-in-differences specification on a propensity-score-matched sample of secondary market municipal bonds. Propensity score matching (using one-to-one nearest-neighbor matching with a caliper of 0.05) is applied to match each treated bond with a control bond in a never-treated state, based on years to maturity, credit rating, log offering amount, coupon rate, callable, general obligation, tax-exempt status, and yield spread, using pre-treatment averages. Each bin spans three months. The omitted category is the bin covering months −3 through −1 immediately prior to the online launch date. All specifications include lagged state-level controls and state-by-cohort and year-month-by-cohort fixed effects. Whiskers represent 90% confidence intervals based on standard errors double-clustered at the bond and year-month level.

**Panel A: MMA Spread**



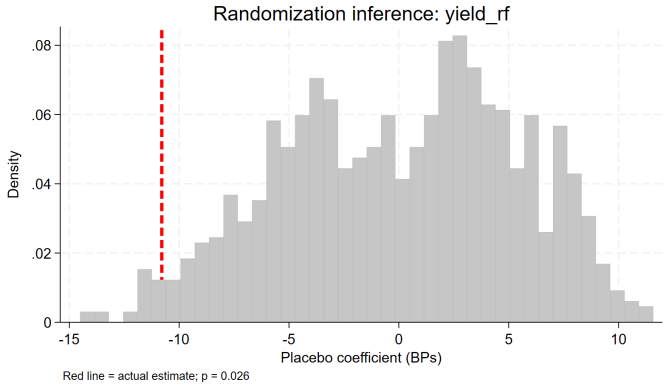
**Panel B: Treasury Spread**



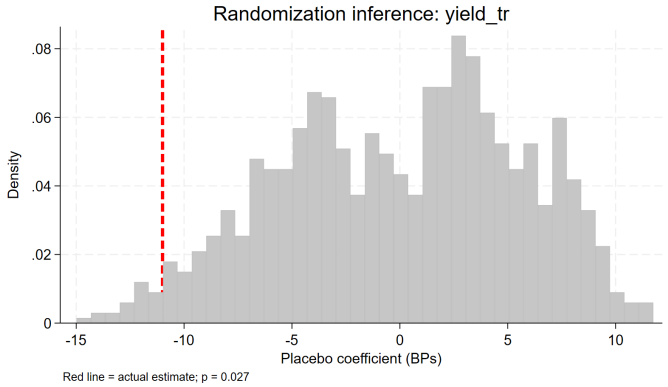

**Panel C: Tax-Adjusted Spread**



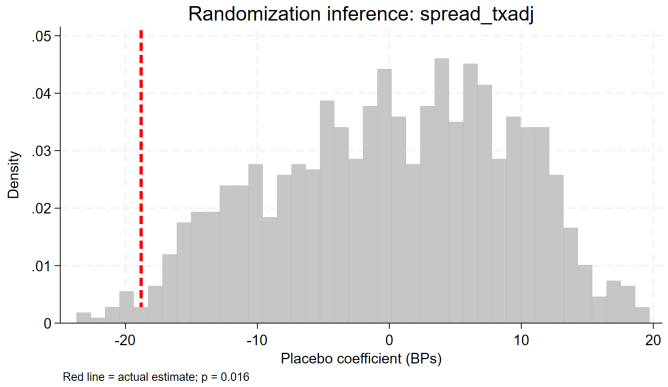
Figure 12 Randomization Inference




(a) MMA-adjusted spread



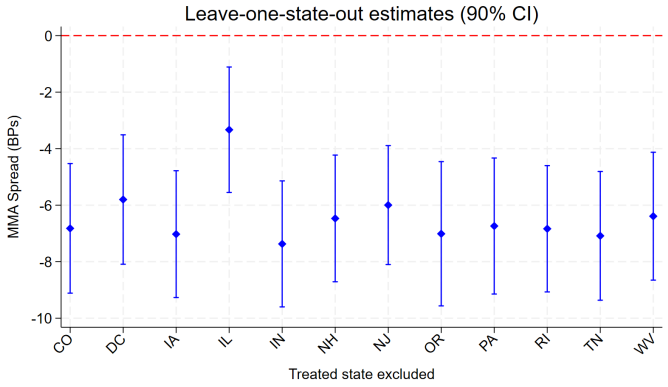

(b) Treasury-adjusted spread



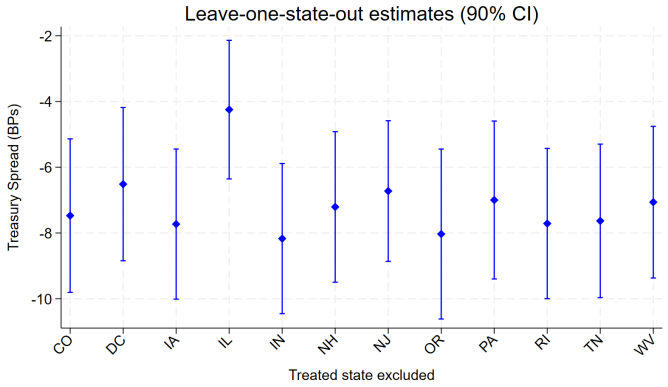

(c) Tax-adjusted spread

This figure plots the distribution of placebo coefficients from 1,000 random reassignments of the twelve online launch dates across the 32 states in the state-month panel described in Panel C of Table 19. In each draw, the vector of launch dates (including the missing dates of the 20 never-treated states) is randomly permuted across states, and the two-way fixed effects regression is re-estimated. The vertical dashed line marks the actual estimate. The $p$-value reported in each panel is the share of placebo coefficients at least as large in absolute value as the actual estimate.

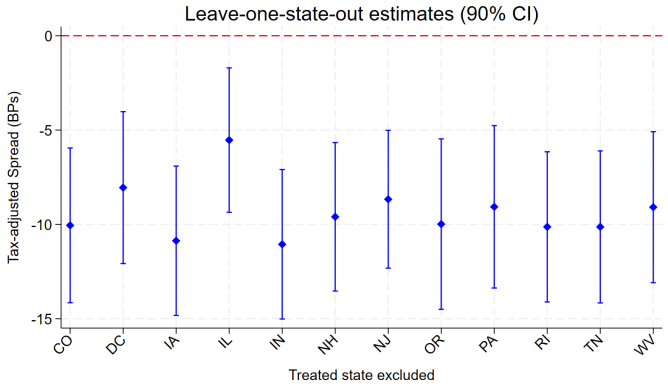
Figure 13 Leave-One-Treated-State-Out Estimates




(a) MMA-adjusted spread



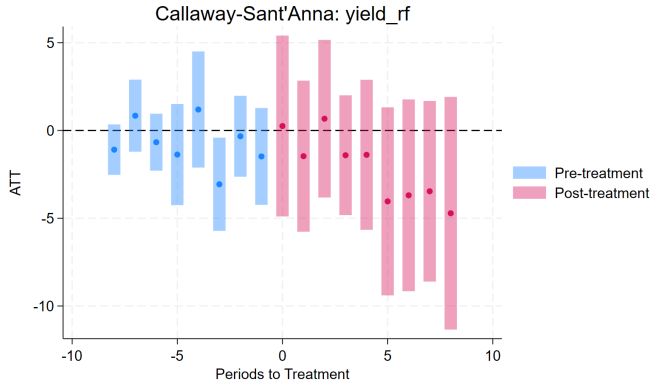

(b) Treasury-adjusted spread



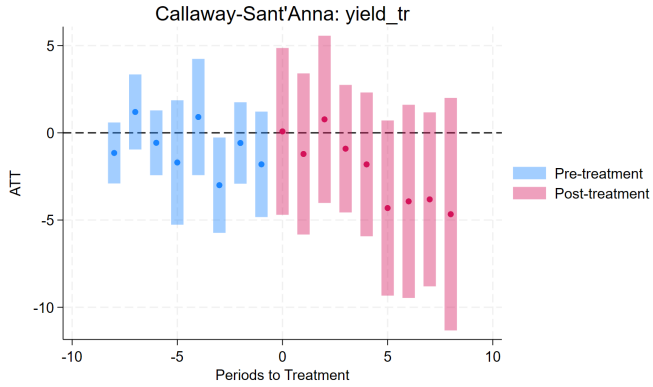

(c) Tax-adjusted spread

This figure plots the coefficient on *Sports Gambling* from Equation (5) with the full set of bond and state controls when each of the twelve treated states is excluded in turn from the stacked sample, together with 90% confidence intervals based on standard errors double-clustered by bond and year-month. The horizontal axis lists the treated state that is excluded. Dependent variables are in basis points.

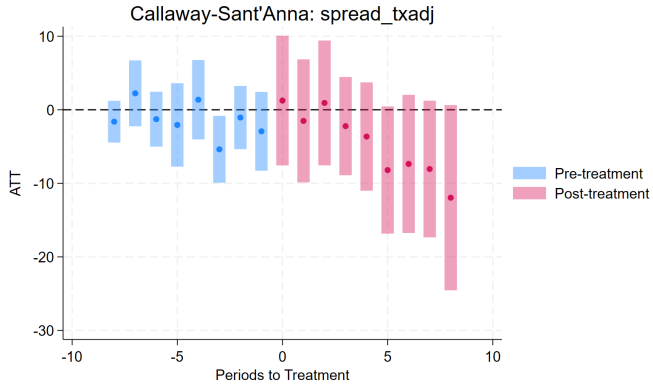
Figure 14 Callaway–Sant'Anna Event-Study Estimates




(a) MMA-adjusted spread




(b) Treasury-adjusted spread




(c) Tax-adjusted spread

This figure plots dynamic average treatment effects from the Callaway and Sant'Anna (2021) estimator on the state-quarter panel described in Table 21, for eight quarters before and after the online launch, with 95% confidence intervals based on standard errors clustered by state. Pre-treatment estimates are period-to-period changes relative to the preceding quarter, as is standard for this estimator, and post-treatment estimates are relative to the quarter before launch. Dependent variables are in basis points.

Table 1 **Summary Statistics: Municipal Bond Sample**

This table provides definitions and summary statistics for key variables for the secondary municipal bond dataset. Data sources are in parentheses. Variables are measured at the bond-year-month level. *MMA Spread* is the difference in basis points between the bond yield to maturity reported by the Municipal Securities Rulemaking Board (MSRB) and the maturity-matched Municipal Market Advisors (MMA) AAA-rated yield curve (Bloomberg). *Treasury Spread* is the difference in basis points between the bond yield to maturity reported by the MSRB and the maturity-matched treasury yield curve (U.S. Treasury Discount Bond Database). *Tax-Adjusted Spread* is the calculated tax-adjusted yield spread, comparing the tax-equivalent yield of a municipal bond to the treasury yield. *Credit Rating* is the lowest numerical rating issued by Moody's, Standard & Poor's, and Fitch (Mergent FISD). Ratings are scaled so that "AAA" is 21 and "D" is 1. *Coupon Rate* is the coupon rate of the bond in percentage (Mergent FISD). *Log Offering Amount* is the natural logarithm of the bond offering amounts in $ millions (Mergent FISD). *Years to Maturity* are years left until the bond matures (Mergent FISD). *Callable* is an indicator equal to one if the bond has a call option (Mergent FISD). *Insured* is an indicator equal to one if the bond is insured (Mergent FISD). *General Obligation Bond* is an indicator of whether the bond is a GO bond (Mergent FISD). *Tax Exempt Status* is an indicator of whether the bond has tax-exempt status (Mergent FISD). *Bond Age* is the years the bond has traded after issuance (Mergent FISD). *Negotiated Offering* is an indicator equal to one if the bond is a negotiated offering (Mergent FISD). *Income per Capita* is the county's per capita personal income in a given year (U.S. Bureau of Economic Analysis). *Log State Population* is the natural logarithm of the state population in a given year (U.S. Census Bureau). *Unemployment Rate* is the state unemployment rate in a given year (U.S. Bureau of Labor Statistics).

| | Mean | SD | P25 | P50 | P75 | Count |
|---|---|---|---|---|---|---|
| **Dependent Variables** | | | | | | |
| MMA Spread | 43.72 | 103.47 | 2.05 | 28.24 | 69.38 | 2,819,950 |
| Treasury Spread | 5.38 | 113.61 | −50.87 | −6.60 | 44.81 | 2,819,950 |
| Tax-Adjusted Spread | 164.50 | 185.67 | 61.32 | 127.34 | 234.31 | 2,819,950 |
| **Bond Characteristics** | | | | | | |
| Credit Rating | 17.92 | 2.30 | 17.00 | 18.00 | 20.00 | 2,065,690 |
| Coupon Rate | 4.41 | 1.06 | 4.00 | 5.00 | 5.00 | 2,819,269 |
| Log Offering Amount | 15.96 | 1.45 | 14.94 | 16.05 | 17.00 | 2,804,237 |
| Years to Maturity | 9.06 | 6.96 | 4.00 | 7.00 | 13.00 | 2,819,950 |
| Callable | 0.58 | 0.49 | 0.00 | 1.00 | 1.00 | 2,819,950 |
| Insured | 0.07 | 0.25 | 0.00 | 0.00 | 0.00 | 2,819,950 |
| General Obligation | 0.21 | 0.40 | 0.00 | 0.00 | 0.00 | 2,819,950 |
| Tax Exempt Status | 0.92 | 0.27 | 1.00 | 1.00 | 1.00 | 2,819,950 |
| Bond Age | 4.70 | 3.47 | 1.95 | 4.31 | 6.96 | 2,819,950 |
| Negotiated Offering | 0.58 | 0.49 | 0.00 | 1.00 | 1.00 | 2,819,950 |
| **State Controls** | | | | | | |
| Income per Capita | 59.47 | 11.09 | 51.35 | 58.40 | 66.46 | 2,819,950 |
| Log State Population | 15.97 | 0.96 | 15.46 | 16.00 | 16.79 | 2,819,950 |
| Unemployment Rate | 4.93 | 1.71 | 3.88 | 4.49 | 5.51 | 2,819,950 |

Table 2 **Summary Statistics: State Government Finance Samples**

This table presents summary statistics for the state government finance samples used in the mechanism analysis. Panel A describes the annual sample from the Census Bureau's Annual Survey of State Government Finances (ASFIN), covering 2013 to 2024. Panel B describes the quarterly sample from the Census Bureau's Quarterly Summary of State and Local Government Tax Revenue (QTAX), covering 2013 to 2025. Both samples are restricted to the 50 states and the District of Columbia, excluding Nevada. All fiscal variables are expressed in per capita terms in dollars. *Selective Sales Tax* is total per capita revenue from selective sales taxes, including amusements, alcohol, tobacco, motor fuel, and public utilities (ASFIN). *Pari-mutuel & Sports Tax* is per capita revenue from pari-mutuel and sports betting taxes combined (ASFIN; QTAX). *Amusements Tax* is per capita quarterly revenue from amusements taxes (QTAX). *Health*, *Police*, *Welfare*, and *Hospitals Expense* are per capita state expenditures on the respective functions (ASFIN). *Income per Capita* is the state per capita personal income in thousands of dollars (Bureau of Economic Analysis). *Log State Population* is the natural logarithm of state population (U.S. Census Bureau). *Unemployment Rate* is the state unemployment rate in percent (U.S. Bureau of Labor Statistics).

| | Mean | SD | P25 | P50 | P75 | Count |
|---|---|---|---|---|---|---|
| **Panel A: ASFIN Sample** | | | | | | |
| *Dependent Variables* | | | | | | |
| Selective Sales Tax | 512.23 | 168.11 | 372.33 | 493.13 | 621.79 | 517 |
| Pari-mutuel & Sports Tax | 2.03 | 5.69 | 0.13 | 0.41 | 1.12 | 493 |
| Health Expense | 223.88 | 142.48 | 116.58 | 184.76 | 282.92 | 517 |
| Police Expense | 64.12 | 40.37 | 40.80 | 52.36 | 74.90 | 517 |
| Welfare Expense | 2,210.60 | 805.17 | 1,580.99 | 2,027.98 | 2,638.16 | 517 |
| Hospitals Expense | 278.35 | 258.83 | 68.14 | 220.64 | 441.01 | 517 |
| *State Controls* | | | | | | |
| Income per Capita | 52.76 | 11.14 | 44.20 | 51.47 | 59.48 | 564 |
| Log State Population | 15.23 | 1.00 | 14.43 | 15.35 | 15.81 | 564 |
| Unemployment Rate | 4.93 | 1.81 | 3.52 | 4.50 | 6.11 | 564 |
| **Panel B: QTAX Sample** | | | | | | |
| *Dependent Variables* | | | | | | |
| Selective Sales Tax | 137.62 | 59.16 | 93.73 | 126.61 | 167.54 | 2,600 |
| Pari-mutuel & Sports Tax | 0.96 | 2.06 | 0.05 | 0.15 | 0.59 | 2,048 |
| Amusements Tax | 8.25 | 12.73 | 0.03 | 2.64 | 10.96 | 2,150 |
| *State Controls* | | | | | | |
| Income per Capita | 54.68 | 12.78 | 45.26 | 52.66 | 62.23 | 2,600 |
| Log State Population | 15.17 | 1.04 | 14.38 | 15.34 | 15.84 | 2,600 |
| Unemployment Rate | 4.85 | 1.79 | 3.48 | 4.40 | 6.00 | 2,600 |

Table 3 **Municipal Bond Yields and Online Sports Betting Legalization (Never-Treated as Control)**

This table reports stacked difference-in-differences estimates of the effect of online sports betting legalization on secondary market municipal bond yield spreads. The sample includes bonds issued by state-level governments within a 30-month window around the legalization event. The control group consists of never-treated states. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given month. Odd columns include no bond or state controls. Even columns add bond-level controls and lagged state-level controls defined in Table 1. All specifications include state-by-cohort and year-month-by-cohort fixed effects. Standard errors are double-clustered at the bond and year-month level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | MMA Spread | | Treasury Spread | | Tax-adj Spread | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Sports Gambling | −7.124*** | −6.431*** | −8.033*** | −7.153*** | −10.878*** | −9.388*** |
| | (1.411) | (1.325) | (1.443) | (1.350) | (2.527) | (2.348) |
| Coupon Rate | | −3.969*** | | −3.866*** | | −6.552*** |
| | | (0.645) | | (0.669) | | (1.142) |
| Log Offering Amount | | −6.667*** | | −6.988*** | | −12.521*** |
| | | (0.363) | | (0.361) | | (0.631) |
| Years to Maturity | | 0.947*** | | 2.457*** | | 7.173*** |
| | | (0.100) | | (0.244) | | (0.294) |
| Callable | | −15.222*** | | −8.622*** | | −8.013*** |
| | | (1.032) | | (1.599) | | (1.905) |
| Insured | | 6.323*** | | 6.727*** | | 10.641*** |
| | | (1.498) | | (1.496) | | (2.640) |
| General Obligation | | −15.361*** | | −14.527*** | | −23.569*** |
| | | (1.198) | | (1.154) | | (1.988) |
| Tax Exempt Status | | −81.329*** | | −82.125*** | | −141.651*** |
| | | (2.130) | | (2.370) | | (3.863) |
| Negotiated Offering | | 4.770*** | | 4.514*** | | 6.124*** |
| | | (0.949) | | (0.959) | | (1.635) |
| Income per Capita | | 1.045*** | | 0.745*** | | 1.657*** |
| | | (0.143) | | (0.175) | | (0.433) |
| Log State Population | | −22.259 | | −21.808 | | −127.125*** |
| | | (21.968) | | (24.153) | | (45.505) |
| Unemployment Rate | | 0.423* | | 0.637** | | −1.412*** |
| | | (0.236) | | (0.253) | | (0.528) |
| State × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Month × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.102 | 0.270 | 0.317 | 0.444 | 0.229 | 0.445 |
| Observations | 5,657,577 | 5,629,789 | 5,657,577 | 5,629,789 | 5,657,577 | 5,629,789 |

Table 4 **Heterogeneity Analysis: Yield Spreads by Time to Maturity**

This table reports stacked difference-in-differences estimates of the effect of online sports betting legalization on secondary market yield spreads, partitioned by bonds' remaining time to maturity (0–5 years, 5–15 years, and 15–30 years). Panel A reports the results for the MMA-adjusted spread, Panel B for the Treasury-adjusted spread, and Panel C for the tax-adjusted spread. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given month. All specifications include time-varying bond controls, lagged state-level macroeconomic controls, state-by-cohort fixed effects, and year-month-by-cohort fixed effects. Standard errors are double-clustered at the bond and year-month level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

**Panel A: MMA-adjusted Spread**

| | (1) 0–5 Years | (2) 5–15 Years | (3) 15–30 Years |
|---|---|---|---|
| Sports Gambling | −5.417*** | −6.821*** | −7.747*** |
| | (1.516) | (1.524) | (2.263) |
| Bond Controls | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes |
| State × Cohort FE | Yes | Yes | Yes |
| Year-Month × Cohort FE | Yes | Yes | Yes |
| Adj. R-squared | 0.272 | 0.349 | 0.251 |
| Observations | 2,133,024 | 2,527,009 | 969,756 |

**Panel B: Treasury-adjusted Spread**

| | (1) 0–5 Years | (2) 5–15 Years | (3) 15–30 Years |
|---|---|---|---|
| Sports Gambling | −5.603*** | −7.084*** | −8.126*** |
| | (1.572) | (1.528) | (2.326) |
| Bond Controls | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes |
| State × Cohort FE | Yes | Yes | Yes |
| Year-Month × Cohort FE | Yes | Yes | Yes |
| Adj. R-squared | 0.548 | 0.505 | 0.367 |
| Observations | 2,133,024 | 2,527,009 | 969,756 |

**Panel C: Tax-adjusted Spread**

| | (1)<br>0–5 Years | (2)<br>5–15 Years | (3)<br>15–30 Years |
|---|---|---|---|
| Sports Gambling | −6.584** | −10.208*** | −11.736*** |
| | (2.595) | (2.711) | (4.239) |
| Bond Controls | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes |
| State × Cohort FE | Yes | Yes | Yes |
| Year-Month × Cohort FE | Yes | Yes | Yes |
| Adj. R-squared | 0.385 | 0.448 | 0.367 |
| Observations | 2,133,024 | 2,527,009 | 969,756 |

Table 5 **Heterogeneity Analysis: Yield Spreads by Bond Type**

This table reports stacked difference-in-differences estimates of the effect of online sports betting legalization on secondary market yield spreads, partitioned by bond type (Revenue vs. General Obligation). Columns 1–2 use the MMA-adjusted spread, Columns 3–4 use the Treasury-adjusted spread, and Columns 5–6 use the tax-adjusted spread. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given month. All specifications include time-varying bond controls, lagged state-level controls, state-by-cohort fixed effects, and year-month-by-cohort fixed effects. Standard errors are double-clustered at the bond and year-month level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | MMA Spread | | Treasury Spread | | Tax-adj Spread | |
|---|---|---|---|---|---|---|
| | (1) Revenue | (2) GO | (3) Revenue | (4) GO | (5) Revenue | (6) GO |
| Sports Gambling | −5.512*** | −10.633*** | −6.831*** | −10.276*** | −8.448*** | −15.971*** |
| | (1.647) | (3.110) | (1.713) | (2.985) | (2.913) | (5.371) |
| Bond Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| State × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Month × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.318 | 0.384 | 0.472 | 0.513 | 0.473 | 0.498 |
| Observations | 1,528,938 | 1,532,839 | 1,528,938 | 1,532,839 | 1,528,938 | 1,532,839 |

Table 6 **Online Sports Betting Legalization and State Tax Revenue - Year Level**

This table reports stacked difference-in-differences estimates of the effect of online sports betting legalization on yearly state tax revenue per capita, using annual data from the Census Bureau's Annual Survey of State Government Finances (ASFIN). The sample covers 2013 to 2024 within a five-year window around each legalization event. The sample covers the same set of cohorts as the municipal bond dataset. The control group consists of never-treated states. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given year. *Selective Sales Tax* is the total per capita revenue from selective sales taxes, including amusements, alcohol, tobacco, motor fuel, and public utilities. *Pari-mutuel & Sports Tax* is per capita revenue from pari-mutuel and sports betting taxes combined. Odd columns include no state controls. Even columns add lagged state-level controls, including income per capita, log state population, and unemployment rate. All specifications include state-by-cohort and year-by-cohort fixed effects. Standard errors are clustered at the state level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | Selective Sales Tax | | Pari-mutuel & Sports Tax | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Sports Gambling | 65.000*** | 54.877*** | 2.348 | 2.924** |
| | (20.483) | (19.542) | (1.499) | (1.091) |
| Income per Capita | | 6.418*** | | 0.054 |
| | | (2.231) | | (0.171) |
| Log State Population | | −292.092 | | 31.769 |
| | | (257.086) | | (41.247) |
| Unemployment Rate | | −12.580** | | −0.454 |
| | | (5.322) | | (0.288) |
| State × Cohort FE | Yes | Yes | Yes | Yes |
| Year × Cohort FE | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.937 | 0.944 | 0.156 | 0.165 |
| Observations | 709 | 709 | 598 | 598 |

Table 7 **Online Sports Betting Legalization and State Tax Revenue - Quarter Level**

This table reports stacked difference-in-differences estimates of the effect of online sports betting legalization on quarterly state tax revenue per capita, using data from the Census Bureau's Quarterly Summary of State and Local Government Tax Revenue (QTAX). The sample covers 2013 to 2025 within a 20-quarter window around each legalization event. The sample covers the same set of cohorts as the municipal bond dataset. The control group consists of never-treated states. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given quarter. *Selective Sales Tax* is total per capita quarterly revenue from selective sales taxes. *Pari-mutuel & Sports Tax* is per capita quarterly revenue from pari-mutuel and sports betting taxes combined. *Amusements Tax* is per capita quarterly revenue from amusements taxes. Odd columns include no state controls. Even columns add lagged state-level controls including income per capita, log state population, and unemployment rate. All specifications include state-by-cohort and quarter-by-cohort fixed effects. Standard errors are clustered at the state level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | Selective Sales Tax | | Pari-mutuel & Sports Tax | | Amusements Tax | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Sports Gambling | 13.650** | 12.969** | 1.046* | 1.224** | 0.871 | 0.497 |
| | (5.582) | (5.577) | (0.596) | (0.503) | (1.164) | (1.152) |
| Income per Capita | | 0.232 | | −0.001 | | −0.090 |
| | | (0.650) | | (0.029) | | (0.135) |
| Log State Population | | −41.303 | | 5.645 | | −36.264** |
| | | (71.242) | | (9.180) | | (15.916) |
| Unemployment Rate | | −2.235 | | −0.260 | | 0.108 |
| | | (1.405) | | (0.236) | | (0.363) |
| State × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.790 | 0.791 | 0.227 | 0.239 | 0.724 | 0.728 |
| Observations | 6,232 | 6,232 | 4,267 | 4,267 | 4,710 | 4,710 |

Table 8 **Online Sports Betting Legalization and Gambling Market Outcomes - Month Level (Never-Treated as Control)**

This table reports stacked difference-in-differences estimates of the effect of online sports betting legalization on the monthly gambling market outcomes per capita, using data from the Sports Handle. The sample covers 2013 to 2025 within a 120-month window around each legalization event. The sample covers the same set of cohorts as the municipal bond dataset. The control group consists of never-treated states. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given month. *Gambling Tax* is monthly state sports betting tax revenue per capita in dollars. *Total Handle* is monthly total gambling volume per capita in dollars. *Gross Revenue* is the monthly gross sportsbook revenue per capita in dollars. Odd columns include no state controls. Even columns add lagged state-level controls including income per capita, log state population, and unemployment rate. All specifications include state-by-cohort and month-by-cohort fixed effects. Standard errors are clustered at the state level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | Gambling Tax | | Total Handle | | Gross Revenue | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Sports Gambling | 0.786*** | 0.775*** | 42.701*** | 42.309*** | 3.507*** | 3.456*** |
| | (0.133) | (0.131) | (5.230) | (5.233) | (0.390) | (0.389) |
| Income per Capita | | 0.003 | | 0.036 | | 0.005 |
| | | (0.003) | | (0.148) | | (0.016) |
| Log State Population | | −0.412 | | −19.020 | | −2.389 |
| | | (0.320) | | (18.359) | | (1.983) |
| Unemployment Rate | | −0.002 | | 0.082 | | −0.002 |
| | | (0.002) | | (0.232) | | (0.019) |
| State × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Month × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.621 | 0.623 | 0.728 | 0.729 | 0.670 | 0.673 |
| Observations | 25,077 | 25,077 | 25,484 | 25,484 | 25,623 | 25,623 |

Table 9 **Online Sports Betting Legalization and State Government Expenditure - Year Level**

This table reports stacked difference-in-differences estimates of the effect of online sports betting legalization on yearly state expenditure per capita, using annual data from the Census Bureau's Annual Survey of State Government Finances (ASFIN). The sample covers 2013 to 2024 within a five-year window around each legalization event. The sample covers the same set of cohorts as the municipal bond dataset. The control group consists of never-treated states. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given year. Dependent variables are per capita state expenditures on health, police, welfare, and hospitals, respectively. Odd columns include no state controls. Even columns add lagged state-level controls, including income per capita, log state population, and unemployment rate. All specifications include state-by-cohort and year-by-cohort fixed effects. Standard errors are clustered at the state level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | Health Expense | | Police Expense | | Welfare Expense | | Hospitals Expense | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Sports Gambling | −2.057 | −5.920 | 6.002 | 4.937 | 153.311 | 24.664 | −29.671 | −33.964 |
| | (30.147) | (32.042) | (4.131) | (4.488) | (97.931) | (110.374) | (41.141) | (47.129) |
| Income per Capita | | 3.810 | | −0.611 | | 44.817* | | 1.046 |
| | | (5.357) | | (0.727) | | (23.445) | | (5.545) |
| Log State Population | | −8.547 | | −99.336 | | −5,094.581*** | | −255.715 |
| | | (242.579) | | (74.068) | | (1,749.667) | | (920.477) |
| Unemployment Rate | | 2.575 | | 0.204 | | 55.529* | | −15.971 |
| | | (4.166) | | (0.712) | | (29.141) | | (11.273) |
| State × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.838 | 0.840 | 0.948 | 0.950 | 0.921 | 0.938 | 0.830 | 0.832 |
| Observations | 709 | 709 | 709 | 709 | 709 | 709 | 709 | 709 |

Table 10 Other State Tax Sources after Online Sports Betting Legalization

This table estimates Equation (7) for tax sources other than sports betting to examine whether the new revenue displaces existing collections. Panel A uses the annual ASFIN sample with a five-year window around the launch year, and Panel B uses the quarterly QTAX sample with a 20-quarter window around the launch quarter, both restricted to the 2018–2020 launch cohorts with never-treated states as the control group, as in Tables 6 and 7. All dependent variables are in dollars per capita. Regressions include the lagged state controls, state-cohort fixed effects, and time-cohort fixed effects. Standard errors, clustered by state, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

*Panel A: Annual (ASFIN)*

| | (1) Alcohol Tax | (2) Tobacco Tax | (3) General Sales Tax | (4) Net Lottery Rev. |
|---|---|---|---|---|
| Sports Gambling | 0.035 | 2.830 | −34.534 | −5.734 |
| | (1.304) | (4.809) | (67.438) | (6.748) |
| Adj. R-squared | 0.960 | 0.929 | 0.955 | 0.965 |
| Observations | 758 | 758 | 655 | 580 |

*Panel B: Quarterly (QTAX)*

| | (1) Alcohol Tax | (2) Tobacco Tax | (3) General Sales Tax | (4) Indiv. Income Tax | (5) Corp. Income Tax |
|---|---|---|---|---|---|
| Sports Gambling | 0.179 | 1.287 | −13.164 | −31.472 | 6.468 |
| | (0.471) | (1.196) | (16.737) | (22.765) | (16.681) |
| Adj. R-squared | 0.769 | 0.887 | 0.890 | 0.841 | 0.669 |
| Observations | 6,232 | 6,232 | 5,587 | 5,135 | 5,658 |

Table 11 Heterogeneity by Credit Rating and Tax Status

This table re-estimates Equation (5) with the full set of bond and state controls on subsamples of the secondary market sample and reports the coefficient on *Sports Gambling* for each spread measure. In Panel A, bonds are grouped by the composite credit rating at the time of trade: AA and above (numerical rating of 19 or higher), below AA (rating of 18 or lower), and bonds with no rating from any of the three agencies. In Panel B, bonds are split by whether interest is exempt from federal income tax; the tax-exempt indicator is dropped from the bond controls in this panel. Never-treated states serve as the control group. Dependent variables are in basis points. Standard errors, double-clustered by bond and year-month, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

| Subsample | (1) MMA Spread | (2) Treasury Spread | (3) Tax-adj Spread | Observations |
|---|---|---|---|---|
| *Panel A: Credit rating* | | | | |
| AA and above | 2.675*** | 4.254*** | 4.316** | 1,843,696 |
| | (0.955) | (1.107) | (1.676) | |
| Below AA | −9.949*** | −10.542*** | −12.651*** | 2,381,020 |
| | (1.962) | (2.017) | (3.437) | |
| Unrated | −4.751*** | −5.984*** | −7.336*** | 1,405,073 |
| | (1.344) | (1.446) | (2.401) | |
| *Panel B: Tax status* | | | | |
| Tax-exempt | −6.407*** | −7.312*** | −9.494*** | 5,228,947 |
| | (1.333) | (1.380) | (2.322) | |
| Taxable | −0.977 | 0.305 | 1.651 | 400,842 |
| | (2.772) | (2.903) | (5.115) | |

Table 12 Treatment Intensity: First-Year Betting Activity and Tax Collections

This table interacts the treatment indicator in Equation (5) with measures of how large each state's online sports betting market turned out to be. For each of the twelve treated states, I compute average monthly per capita handle and average monthly per capita sports betting tax collections over the first twelve months after the online launch from the monthly Sports Handle data. *Handle intensity* is the log of first-year per capita handle, standardized to mean zero and unit standard deviation across the twelve treated states; *Tax intensity* is first-year per capita tax collections standardized in the same way. Oregon, whose lottery-run sportsbook does not report a separate sports betting tax, is dropped from the tax-intensity specification in Panel B. *High-handle* states are those whose first-year per capita handle exceeds the median among treated states (Colorado, the District of Columbia, Illinois, Indiana, New Jersey, and Tennessee); the remaining six treated states are *Low-handle* states. The row $p$(High = Low) reports the $p$-value of a test that the two coefficients are equal. All specifications include the full set of bond and state controls, state-cohort fixed effects, and year-month-cohort fixed effects. Dependent variables are in basis points. Standard errors, double-clustered by bond and year-month, are in parentheses. $^{*}$, $^{**}$, and $^{***}$ denote significance at the 10%, 5%, and 1% levels.

| | (1) MMA Spread | (2) Treasury Spread | (3) Tax-adj Spread |
|---|---|---|---|
| *Panel A: Handle intensity* | | | |
| Sports Gambling | $-5.862^{***}$ | $-6.498^{***}$ | $-8.687^{***}$ |
| | (1.160) | (1.201) | (2.060) |
| Sports Gambling × Handle intensity | $-3.439^{***}$ | $-3.964^{***}$ | $-4.238^{**}$ |
| | (1.197) | (1.182) | (2.075) |
| Adj. R-squared | 0.270 | 0.445 | 0.445 |
| Observations | 5,629,789 | 5,629,789 | 5,629,789 |
| *Panel B: Tax intensity (Oregon excluded)* | | | |
| Sports Gambling | $-7.922^{***}$ | $-8.826^{***}$ | $-11.763^{***}$ |
| | (1.486) | (1.509) | (2.659) |
| Sports Gambling × Tax intensity | $-4.763^{***}$ | $-4.159^{***}$ | $-9.313^{***}$ |
| | (0.944) | (1.003) | (1.634) |
| Adj. R-squared | 0.270 | 0.444 | 0.445 |
| Observations | 5,592,875 | 5,592,875 | 5,592,875 |
| *Panel C: High- versus low-handle states* | | | |
| Sports Gambling × High-handle state | $-9.426^{***}$ | $-10.134^{***}$ | $-13.329^{***}$ |
| | (2.150) | (2.137) | (3.777) |
| Sports Gambling × Low-handle state | $-2.364^{***}$ | $-3.106^{***}$ | $-4.038^{**}$ |
| | (0.800) | (0.957) | (1.554) |
| $p$(High = Low) | 0.002 | 0.002 | 0.020 |
| Adj. R-squared | 0.270 | 0.445 | 0.445 |
| Observations | 5,629,789 | 5,629,789 | 5,629,789 |

Table 13 Yield Spreads and Realized Sports Betting Tax Collections

This table replaces the treatment indicator in Equation (5) with the sports betting tax revenue actually collected by the state. *Monthly SB tax per capita* is the state's sports betting tax collections in the month of the bond observation, in dollars per capita, from the monthly Sports Handle data. *Cumulative SB tax per capita* is the sum of monthly per capita collections from the online launch through the month of the observation, in hundreds of dollars per capita. Both variables equal zero before launch and for never-treated states. Panel C includes the treatment indicator and the cumulative tax variable together. All specifications include the full set of bond and state controls, state-cohort fixed effects, and year-month-cohort fixed effects. Dependent variables are in basis points. Standard errors, double-clustered by bond and year-month, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

| | (1) MMA Spread | (2) Treasury Spread | (3) Tax-adj Spread |
|---|---|---|---|
| *Panel A: Monthly tax collections* | | | |
| Monthly SB tax per capita ($) | −10.530*** | −11.363*** | −18.368*** |
| | (1.674) | (1.671) | (3.191) |
| Adj. R-squared | 0.270 | 0.445 | 0.445 |
| *Panel B: Cumulative tax collections since launch* | | | |
| Cumulative SB tax per capita ($100) | −56.459*** | −61.468*** | −104.907*** |
| | (6.606) | (7.007) | (13.859) |
| Adj. R-squared | 0.270 | 0.444 | 0.445 |
| *Panel C: Treatment indicator and cumulative tax collections* | | | |
| Sports Gambling | −4.394*** | −5.008*** | −4.392* |
| | (1.518) | (1.525) | (2.627) |
| Cumulative SB tax per capita ($100) | −33.342*** | −35.125*** | −81.802*** |
| | (7.672) | (7.633) | (16.357) |
| Adj. R-squared | 0.270 | 0.444 | 0.445 |
| Observations | 5,629,789 | 5,629,789 | 5,629,789 |

Table 14 Heterogeneity by Pre-Legalization State Fiscal Conditions

This table interacts the treatment indicator in Equation (5) with indicators for pre-legalization fiscal conditions of the treated states. In Panel A, treated states are split at the median of the state pension funded ratio (plan assets as a percent of liabilities) averaged over 2013–2017; *Well-funded* states are the District of Columbia, Indiana, Iowa, Oregon, Tennessee, and West Virginia, and *Poorly-funded* states are Colorado, Illinois, New Hampshire, New Jersey, Pennsylvania, and Rhode Island. In Panel B, treated states are split at the median share of selective sales taxes in total state tax revenue over fiscal years 2013–2017 from ASFIN; *High sin-tax reliance* states are Indiana, New Hampshire, Pennsylvania, Rhode Island, Tennessee, and West Virginia, and *Low sin-tax reliance* states are Colorado, Illinois, Iowa, New Jersey, and Oregon. The District of Columbia is not covered by ASFIN and is excluded from Panel B. SG denotes the *Sports Gambling* indicator. Both interaction terms enter the same regression, so each coefficient is the treatment effect for the corresponding group of states relative to never-treated states. The row $p$(High = Low) reports the $p$-value of a test that the two coefficients are equal. All specifications include the full set of bond and state controls, state-cohort fixed effects, and year-month-cohort fixed effects. Dependent variables are in basis points. Standard errors, double-clustered by bond and year-month, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

| | (1) MMA Spread | (2) Treasury Spread | (3) Tax-adj Spread |
|---|---|---|---|
| *Panel A: Pension funded ratio* | | | |
| SG × Well-funded pensions | −0.456 | −0.820 | −1.385 |
| | (0.880) | (0.986) | (1.527) |
| SG × Poorly-funded pensions | −9.320*** | −10.215*** | −13.257*** |
| | (2.004) | (2.044) | (3.476) |
| $p$(High = Low) | 0.000 | 0.000 | 0.003 |
| Adj. R-squared | 0.270 | 0.445 | 0.445 |
| Observations | 5,629,789 | 5,629,789 | 5,629,789 |
| *Panel B: Reliance on selective sales taxes* | | | |
| SG × High sin-tax reliance | −1.179 | −2.758** | −2.913 |
| | (0.995) | (1.105) | (1.933) |
| SG × Low sin-tax reliance | −9.177*** | −9.257*** | −11.806*** |
| | (2.010) | (2.029) | (3.547) |
| $p$(High = Low) | 0.000 | 0.001 | 0.011 |
| Adj. R-squared | 0.270 | 0.445 | 0.445 |
| Observations | 5,617,186 | 5,617,186 | 5,617,186 |

## Table 15 Credit Rating Actions and Primary-Market Issuance

This table examines whether the decline in yield spreads is accompanied by rating agency actions or by changes in bond supply. Panel A uses the stacked bond-month sample of Equation (5). *Upgrade* (*Downgrade*) equals one if the bond's composite rating in the current month is higher (lower) than in the month of its previous trade within the same cohort sample, and zero otherwise. The regressions include the state controls, state-cohort fixed effects, and year-month-cohort fixed effects, with standard errors double-clustered by bond and year-month. Panel B collapses the primary-market sample of state issuers to a state-month panel of the number and dollar amount of new issues, with months without issuance set to zero, and stacks the panel by cohort using the same 30-month windows and never-treated control group as the baseline. *Log(1+Issues)* is the log of one plus the number of new issues in the state-month, *Log(1+Amount)* is the log of one plus the total offering amount in millions of dollars, and *Number of Issues* is the raw count. Panel B regressions include state-cohort and year-month-cohort fixed effects, with standard errors clustered by state. Panel C reports state-year regressions of the average composite rating of state issuers' bonds and of the share of bond-months rated AA− or better, weighted by the number of rated bond-months. The state-year panel is stacked by launch-year cohort with a five-year window on each side of the launch year and never-treated states as the control group, and the regressions include state-cohort and year-cohort fixed effects with standard errors clustered by state. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

*Panel A: Rating changes, bond-month level*

| | (1) Upgrade | (2) Downgrade |
|---|---|---|
| Sports Gambling | 0.001 | −0.002 |
| | (0.003) | (0.005) |
| Adj. R-squared | 0.023 | 0.018 |
| Observations | 3,904,309 | 3,904,309 |

*Panel B: Primary-market issuance, state-month level*

| | (1) Log(1+Issues) | (2) Log(1+Amount) | (3) Number of Issues |
|---|---|---|---|
| Sports Gambling | −0.032 | 0.031 | −1.272 |
| | (0.120) | (0.164) | (2.047) |
| Adj. R-squared | 0.284 | 0.313 | 0.450 |
| Observations | 12,932 | 12,932 | 12,932 |

*Panel C: Average rating of state issuers, state-year level*

| | (1) Avg. Rating (1–21) | (2) Share AA− or Better |
|---|---|---|
| Sports Gambling | −0.286 | −0.064 |
| | (0.231) | (0.057) |
| Adj. R-squared | 0.921 | 0.893 |
| Observations | 768 | 768 |

Table 16 **Robustness: Municipal Bond Yields and Online Sports Betting Legalization (Not-Yet-Treated as Control)**

This table reports stacked difference-in-differences estimates of the effect of online sports betting legalization on secondary market municipal bond yield spreads. The sample includes bonds issued by state-level governments within a 30-month window around the legalization event. The control group consists of not-yet-treated states. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given month. Odd columns include no bond or state controls. Even columns add bond-level controls and lagged state-level controls defined in Table 1. All specifications include state-by-cohort and year-month-by-cohort fixed effects. Standard errors are double-clustered at the bond and year-month level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | MMA Spread | | Treasury Spread | | Tax-adj Spread | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Sports Gambling | −6.409*** | −6.350*** | −7.357*** | −6.964*** | −10.383*** | −9.522*** |
| | (1.302) | (1.177) | (1.309) | (1.212) | (2.165) | (2.028) |
| Coupon Rate | | −6.671*** | | −6.303*** | | −10.642*** |
| | | (0.485) | | (0.532) | | (0.869) |
| Log Offering Amount | | −5.913*** | | −6.312*** | | −10.756*** |
| | | (0.272) | | (0.275) | | (0.467) |
| Years to Maturity | | 0.733*** | | 2.270*** | | 6.744*** |
| | | (0.080) | | (0.235) | | (0.280) |
| Callable | | −13.323*** | | −7.770*** | | −6.885*** |
| | | (1.086) | | (1.589) | | (1.939) |
| Insured | | 10.670*** | | 10.968*** | | 17.963*** |
| | | (1.319) | | (1.334) | | (2.293) |
| General Obligation | | −3.168*** | | −2.759** | | −4.719*** |
| | | (1.069) | | (1.092) | | (1.702) |
| Tax Exempt Status | | −81.940*** | | −82.778*** | | −139.547*** |
| | | (1.784) | | (1.959) | | (3.107) |
| Negotiated Offering | | 2.134*** | | 1.219 | | 1.866 |
| | | (0.758) | | (0.758) | | (1.396) |
| Income per Capita | | 0.736*** | | 0.530** | | 1.578*** |
| | | (0.234) | | (0.256) | | (0.377) |
| Log State Population | | 84.774* | | 99.968** | | 40.772 |
| | | (43.317) | | (40.018) | | (61.721) |
| Unemployment Rate | | 0.475 | | −0.636 | | −0.559 |
| | | (0.362) | | (0.417) | | (0.649) |
| State × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Month × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.120 | 0.289 | 0.347 | 0.469 | 0.232 | 0.445 |
| Observations | 9,153,936 | 9,116,803 | 9,153,936 | 9,116,803 | 9,153,936 | 9,116,803 |

Table 17 **Robustness: PSM-DID - Municipal Bond Yields and Online Sports Betting Legalization (Never-Treated as Control)**

This table reports stacked difference-in-differences estimates on a propensity-score-matched sample of secondary market municipal bonds. Propensity score matching (using one-to-one nearest-neighbor matching with a caliper of 0.05) is applied to match each treated bond (issued in a state that legalized online sports betting) with a control bond in a never-treated state. Matching is based on years to maturity, credit rating, log offering amount, coupon rate, callable, general obligation, tax-exempt status, and MMA-adjusted spread, using pre-treatment averages. Matched bonds must have at least five years of remaining maturity. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given month. Odd columns include no controls. Even columns add lagged state-level controls. All specifications include state-by-cohort and year-month-by-cohort fixed effects. Standard errors are double-clustered at the bond and year-month level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | MMA Spread | | Treasury Spread | | Tax-adj Spread | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Sports Gambling | −6.242*** | −6.069*** | −6.112*** | −5.929*** | −7.256*** | −7.120*** |
| | (1.584) | (1.504) | (1.618) | (1.544) | (2.750) | (2.616) |
| Income per Capita | | 0.771** | | 0.587 | | 0.908 |
| | | (0.378) | | (0.408) | | (0.744) |
| Log State Population | | −0.248 | | 6.633 | | −19.408 |
| | | (46.167) | | (46.583) | | (81.213) |
| Unemployment Rate | | 0.287 | | −0.011 | | −2.018* |
| | | (0.577) | | (0.631) | | (1.049) |
| State × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Month × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Adj. R-squared | 0.169 | 0.169 | 0.336 | 0.336 | 0.275 | 0.275 |
| Observations | 370,014 | 370,014 | 370,014 | 370,014 | 370,014 | 370,014 |

Table 18 **Robustness: Municipal Bond Yield Spreads with Bond Fixed Effects**

This table reports stacked difference-in-differences estimates controlling for unobserved time-invariant bond characteristics. The sample consists of secondary market municipal bonds within a symmetric 30-month window around the online launch date. The state-by-cohort fixed effects from the baseline specification are replaced with bond-by-cohort fixed effects, isolating the within-bond variation in yield spreads. *Sports Gambling* equals one after state $s$ legalizes online sports betting in a given month. All specifications include time-varying bond controls, lagged state-level controls, and year-month-by-cohort fixed effects. Standard errors are double-clustered at the bond and year-month level and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

| | (1)<br>MMA Spread | (2)<br>Treasury Spread | (3)<br>Tax-adj Spread |
|---|---|---|---|
| Sports Gambling | $-6.683^{***}$ | $-7.195^{***}$ | $-8.736^{***}$ |
| | (1.363) | (1.390) | (2.421) |
| Bond Controls | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes |
| Bond × Cohort FE | Yes | Yes | Yes |
| Year-Month × Cohort FE | Yes | Yes | Yes |
| Adj. R-squared | 0.710 | 0.745 | 0.754 |
| Observations | 5,621,267 | 5,621,267 | 5,621,267 |

Table 19 Alternative Inference: State-Level Clustering, Aggregated Regressions, and Randomization Inference

This table re-examines the statistical significance of the baseline estimates in Table 3 under inference procedures that allow for arbitrary correlation within states. Panel A re-estimates Equation (5) with the full set of bond and state controls and clusters standard errors by state (12 treated and 20 never-treated states). Panel B collapses the stacked bond-month sample to state-cohort-month means, weights each cell by the number of bond-month observations it contains, and estimates the same specification with state controls, state-cohort fixed effects, and year-month-cohort fixed effects; standard errors are clustered by state. Panel C estimates a two-way fixed effects regression on a state-month panel of the 32 sample states over 2016m2–2023m5, weighted by the number of bond-months, with state and year-month fixed effects, state controls, and standard errors clustered by state. The randomization inference $p$-value in Panel C is the share of 1,000 random reassignments of the twelve launch dates across the 32 states that produce a placebo coefficient at least as large in absolute value as the actual estimate. Dependent variables are in basis points. Standard errors are in parentheses. $^{*}$, $^{**}$, and $^{***}$ denote significance at the 10%, 5%, and 1% levels.

| | (1) MMA Spread | (2) Treasury Spread | (3) Tax-adj Spread |
|---|---|---|---|
| *Panel A: Baseline specification, standard errors clustered by state* | | | |
| Sports Gambling | $-6.431^{**}$ | $-7.153^{**}$ | $-9.388^{**}$ |
| | (3.048) | (2.984) | (4.438) |
| Number of clusters | 32 | 32 | 32 |
| Adj. R-squared | 0.270 | 0.444 | 0.445 |
| Observations | 5,629,789 | 5,629,789 | 5,629,789 |
| *Panel B: State-cohort-month aggregated regression* | | | |
| Sports Gambling | $-7.093^{**}$ | $-7.997^{**}$ | $-11.096^{**}$ |
| | (3.434) | (3.377) | (5.078) |
| Adj. R-squared | 0.905 | 0.977 | 0.957 |
| Observations | 12,932 | 12,932 | 12,932 |
| *Panel C: State-month TWFE regression and randomization inference* | | | |
| Sports Gambling | $-10.791^{**}$ | $-10.999^{**}$ | $-18.803^{***}$ |
| | (4.217) | (4.073) | (6.616) |
| Randomization inference $p$-value | 0.026 | 0.027 | 0.016 |
| Observations | 2,816 | 2,816 | 2,816 |

Table 20 Sensitivity to the COVID-19 Period and to the Event Window

This table re-estimates Equation (5) with the full set of bond and state controls on modified samples and reports the coefficient on *Sports Gambling* for each of the three spread measures. In Panel A, bond-month observations dated March 2020 through December 2020 (Column 1) or March 2020 through June 2021 (Column 2) are dropped from the stacked sample. In Panel B, the symmetric 30-month window around each cohort's online launch date used in the baseline is replaced with windows of 18, 24, and 36 months on each side; the stacked samples are rebuilt and dependent variables are re-winsorized within cohort for each window. Never-treated states serve as the control group throughout. Dependent variables are in basis points. Standard errors, double-clustered by bond and year-month, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

*Panel A: Excluding the COVID-19 period*

| | (1) | (2) |
|---|---|---|
| Months excluded | 2020m3–2020m12 | 2020m3–2021m6 |
| MMA Spread | −10.521*** | −11.030*** |
| | (0.819) | (0.863) |
| Treasury Spread | −11.271*** | −11.317*** |
| | (0.859) | (0.889) |
| Tax-adj Spread | −16.627*** | −18.293*** |
| | (1.501) | (1.554) |
| Observations | 4,722,781 | 4,203,427 |

*Panel B: Alternative event windows*

| | (1) | (2) | (3) |
|---|---|---|---|
| Window (months) | ±18 | ±24 | ±36 |
| MMA Spread | −6.637*** | −5.609*** | −9.073*** |
| | (1.622) | (1.497) | (1.427) |
| Treasury Spread | −7.121*** | −6.334*** | −9.679*** |
| | (1.606) | (1.511) | (1.479) |
| Tax-adj Spread | −7.708*** | −6.551** | −15.683*** |
| | (2.623) | (2.581) | (2.577) |
| Observations | 3,391,191 | 4,510,123 | 6,753,373 |

Table 21 Callaway–Sant'Anna Estimates on a State-Quarter Panel

This table reports the aggregate average treatment effect on the treated from the estimator of Callaway and Sant'Anna (2021). The secondary market sample is collapsed to a state-quarter panel of average yield spreads for the state issuers in each state, covering 2016Q1 through 2024Q4, with Nevada excluded. Every state that launched online sports betting during this period forms a treatment group defined by its launch quarter, and never-treated states serve as the comparison group. The estimator uses the doubly robust inverse-probability-weighting procedure with the state controls of Equation (5) as covariates. The reported ATT averages the group-time effects across all treated groups and post-launch quarters. Dependent variables are in basis points. Standard errors, clustered by state, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

| | (1) MMA Spread | (2) Treasury Spread | (3) Tax-adj Spread |
|---|---|---|---|
| ATT | −4.900** | −4.954** | −12.445** |
| | (2.389) | (2.517) | (5.347) |
| Observations | 1,721 | 1,721 | 1,721 |

Table 22 Heterogeneity by Other Bond Characteristics

This table re-estimates Equation (5) with the full set of bond and state controls on subsamples of the secondary market sample split by call provision, bond insurance, bond age, and issue size, and reports the coefficient on *Sports Gambling* for each spread measure. Bond age is the number of years elapsed since issuance at the time of the trade, split at the sample median; issue size is the offering amount, split at the sample median. The indicator used to form each split is dropped from the bond controls in the corresponding panel. Never-treated states serve as the control group. Dependent variables are in basis points. Standard errors, double-clustered by bond and year-month, are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

| Subsample | (1) MMA Spread | (2) Treasury Spread | (3) Tax-adj Spread | Observations |
|---|---|---|---|---|
| *Panel A: Call provision* | | | | |
| Callable | −5.134*** | −5.692*** | −6.903*** | 3,303,055 |
| | (1.476) | (1.471) | (2.572) | |
| Non-callable | −8.433*** | −9.525*** | −12.804*** | 2,326,734 |
| | (1.414) | (1.533) | (2.511) | |
| *Panel B: Bond insurance* | | | | |
| Insured | −6.461** | −6.194** | −7.443 | 331,542 |
| | (2.877) | (3.082) | (5.083) | |
| Uninsured | −6.884*** | −7.663*** | −10.297*** | 5,298,247 |
| | (1.318) | (1.339) | (2.359) | |
| *Panel C: Bond age* | | | | |
| Below median | −8.360*** | −9.063*** | −13.497*** | 2,822,871 |
| | (1.188) | (1.189) | (2.189) | |
| Above median | −4.562** | −5.264*** | −6.414** | 2,806,918 |
| | (1.808) | (1.836) | (3.113) | |
| *Panel D: Issue size* | | | | |
| Large issue | −9.139*** | −10.922*** | −14.609*** | 2,815,325 |
| | (2.187) | (2.258) | (3.995) | |
| Small issue | −4.520*** | −4.792*** | −6.047*** | 2,814,464 |
| | (0.875) | (0.930) | (1.448) | |

## Appendix A. Revenue Portfolio Variance Decomposition

This appendix derives the change in total state revenue variance from adding a new tax source, following the tax-portfolio framework of Seegert (2015).

**Setup.** Suppose each tax source $j$ generates revenue according to a single-factor model:

$$R_j = \mu_j + \beta_j \, \theta + \epsilon_j, \quad j = 1, \ldots, N, \tag{9}$$

where $\theta$ is a common economic state variable with $\mathrm{E}[\theta] = 0$ and $\mathrm{Var}(\theta) = \sigma_\theta^2$, $\beta_j$ measures the cyclical sensitivity of tax source $j$, and $\epsilon_j$ is an idiosyncratic disturbance with $\mathrm{E}[\epsilon_j] = 0$ and $\mathrm{Var}(\epsilon_j) = \sigma_{\epsilon_j}^2$. I assume $\mathrm{Cov}(\theta, \epsilon_j) = 0$ for all $j$ and $\mathrm{Cov}(\epsilon_j, \epsilon_k) = 0$ for $j \neq k$.

**Existing revenue.** Total revenue from the $N$ existing tax sources is

$$R = \sum_{j=1}^{N} R_j = \sum_{j=1}^{N} \mu_j + B \, \theta + \sum_{j=1}^{N} \epsilon_j, \tag{10}$$

where $B \equiv \sum_{j=1}^{N} \beta_j$ is the aggregate cyclical exposure. The variance of total revenue is

$$\sigma_R^2 = B^2 \, \sigma_\theta^2 + \sum_{j=1}^{N} \sigma_{\epsilon_j}^2. \tag{11}$$

**Adding sports betting revenue.** Let the new tax source generate revenue

$$R^{\mathrm{SB}} = \mu^{\mathrm{SB}} + \beta^{\mathrm{SB}} \, \theta + \epsilon^{\mathrm{SB}}, \tag{12}$$

with $\mathrm{Var}(\epsilon^{\mathrm{SB}}) = \sigma_{\epsilon^{\mathrm{SB}}}^2$ and $\epsilon^{\mathrm{SB}}$ uncorrelated with $\theta$ and all $\epsilon_j$. New total revenue is $R' = R + R^{\mathrm{SB}}$, and by the standard variance decomposition,

$$\sigma_{R'}^2 = \mathrm{Var}(R) + \mathrm{Var}(R^{\mathrm{SB}}) + 2 \, \mathrm{Cov}(R, \, R^{\mathrm{SB}}). \tag{13}$$

**Variance of the new source.** From Equation (12),

$$\mathrm{Var}(R^{\mathrm{SB}}) = (\beta^{\mathrm{SB}})^2\,\sigma_\theta^2 + \sigma_{\epsilon^{\mathrm{SB}}}^2. \tag{14}$$

**Covariance with existing revenue.** Because $\epsilon^{\mathrm{SB}}$ is uncorrelated with $\theta$ and all $\epsilon_j$, only the systematic components contribute:

$$\mathrm{Cov}(R,\, R^{\mathrm{SB}}) = \mathrm{Cov}\Big(B\,\theta + \textstyle\sum_j \epsilon_j,\ \beta^{\mathrm{SB}}\,\theta + \epsilon^{\mathrm{SB}}\Big) = B\,\beta^{\mathrm{SB}}\,\sigma_\theta^2. \tag{15}$$

**Change in total variance.** Substituting Equations (14) and (15) into Equation (13) yields

$$\Delta\sigma^2 \equiv \sigma_{R'}^2 - \sigma_R^2 = 2B\,\beta^{\mathrm{SB}}\,\sigma_\theta^2 + (\beta^{\mathrm{SB}})^2\,\sigma_\theta^2 + \sigma_{\epsilon^{\mathrm{SB}}}^2. \tag{16}$$

**Interpretation.** The first term captures the covariance between the new source and the existing tax portfolio. The second term is the systematic variance of the new source itself. The third term is its idiosyncratic variance. When sports betting revenue is approximately acyclical ($\beta^{\mathrm{SB}} \approx 0$), as suggested by Can and Nichols (2022) for Nevada, the first two terms vanish, and the variance increase reduces to $\sigma_{\epsilon^{\mathrm{SB}}}^2$. For a revenue stream that represents less than one percent of the total state budget, this contribution is negligible relative to $\sigma_R^2$. Expected revenue increases by $\mu^{\mathrm{SB}}$ while revenue volatility barely changes, producing a net reduction in the probability of fiscal distress and, through the credit risk channel, a decline in municipal bond yield spreads.